PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

EDITED BY

J. Y. W. MacALISTER

UNDER THE DIRECTION OF

THE EDITORIAL COMMITTEE

VOLUME THE TENTH

SESSION 1916-17

PART II

SECTIONS:—

ELECTRO-THERAPEUTICS
EPIDEMIOLOGY AND STATE MEDICINE
HISTORY OF MEDICINE
LARYNGOLOGY
MEDICINE
NEUROLOGY
OBSTETRICS AND GYNÆCOLOGY



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1917

LONDON:
JOHN BALE, SONS AND DANIELSSON, LTD.,
OXFORD HOUSE,
83-91, GREAT TITCHFIELD STREET, OXFORD STREET, W. 1.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

EDITED BY

J. Y. W. MacALISTER

UNDER THE DIRECTION OF

THE EDITORIAL COMMITTEE

VOLUME THE TENTH

SESSION 1916-17

SECTION OF ELECTRO-THERAPEUTICS



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1917

Section of Electro-Therapeutics.

OFFICERS FOR THE SESSION 1916-17.

President—

G. HARRISON ORTON, M.D.

Representative on the Council of the Society—

W. DEANE BUTCHER.

Past Presidents—

W. IRONSIDE BRUCE, M.D.
W. DEANE BUTCHER.
C. THURSTAN HOLLAND.
REGINALD MORTON, M.D.
ARCHIBALD D. REID.
S. SLOAN, M.D.

Vice-Presidents—

A. E. BARCLAY, M.D.
Sir JAMES MACKENZIE DAVIDSON, M.B.
CECIL R. C. LYSTER.
S. GILBERT SCOTT.

Hon. Secretaries—

E. P. CUMBERBATCH, M.B.
ROBERT KNOX, M.D.

Other Members of Council—

WILLIAM J. S. BYTHELL, M.D.
J. A. CODD, M.D.
R. H. COOPER.
F. HERNAMAN-JOHNSON, M.D.
F. HOWARD HUMPHRIS, M.D.
A. C. JORDAN, M.D.
W. LINDSAY LOCKE, M.B.
STANLEY MELVILLE, M.D.
SIDNEY RUSS, D.Sc.
WALTER J. TURRELL, M.D.

Representative on Library Committee—

REGINALD MORTON, M.D.

Representative on Editorial Committee—

G. HARRISON ORTON, M.D.

SECTION OF ELECTRO-THERAPEUTICS.

CONTENTS.

October 20, 1916.

PAGE

E. P. Cumberbatch, M.B.

Case of Rodent Ulcer of Scalp treated by Diathermy 1

H. Annesley Eccles, M.D.

War Injuries of the Jaw 2

Agnes Savill, M.D.

X-ray Appearances in Gas Gangrene 4

H. Martin Berry, M.D.

The Recognition of Gas within the Tissues 17

Kenneth Goadby, D.P.H.Cantab.

Note on the Microscopic Histology and Bacteriology of Gas Gangrene... 25

W. J. Turrell, Major R.A.M.C.

Ultra-violet Radiation 31

November 17, 1916.

DISCUSSION ON THE TREATMENT OF WAR INJURIES BY ELECTRICAL METHODS.

The President (G. Harrison Orton, M.D.) 37

W. J. Turrell, M.D., Major R.A.M.C.

The Electrical Treatment of the Wounded 37

Dr. Grainger Stewart (p. 50)—Captain F. Hernaman-Johnson, R.A.M.C. (p. 52)—Captain F. Howard Humphris, R.A.M.C. (p. 55)—Dr. Noel Burke (p. 57)—Dr. E. P. Cumberbatch (p. 59)—Dr. Donald Baynes (p. 60)—Dr. G. B. Batten (p. 60)—Dr. Ettie Sayer (p. 61)—Major Turrell (in reply) (p. 61).

329172

December 15, 1916.

N. S. Finzi, M.B., Captain R.A.M.C. PAGE

Pharyngeal Pouches 63

R. W. A. Salmond, M.D., Ch.M.

The Changes observed in Cases of Osteomyelitis 77

January 19, 1917.

E. P. Cumberbatch, M.B.

The Electrical Department of St. Bartholomew's Hospital 87

March 16, 1917.

W. M. Bayliss, M.A., D.Sc., F.R.S.

On the Origin of Electric Currents led off from the Human Body, especially in Relation to "Nerve-leaks" 95

April 20, 1917.

A. E. Barclay, Captain R.A.M.C.

Further Note on a New Method of Bullet Extraction 103

February 16, 1917.

Martin Berry, M.D.

Trauma in the Ætiology of Arthritis 105

May 18, 1917.

J. C. Mottram, M.B.Lond., and S. Russ, D.Sc.

A Contribution to the Study of Dosage in Radium Therapy 121

The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

John Bale, Sons & Danielsson, Ltd., 83-91, Great Titchfield Street, London, W. 1.

Section of Electro-Therapeutics.

President—Dr. G. Harrison Orton.

(*October* 20, 1916.)

Case of Rodent Ulcer of Scalp treated by Diathermy.

By E. P. Cumberbatch, M.B.

This case was shown in order to illustrate, first, the results of treatment of malignant ulcer by diathermy, and secondly, the effect that such treatment may have on bone when the bone is in the neighbourhood of the treated tissue.

The patient, a woman, aged 67, came to the Electrical Department of St. Bartholomew's Hospital in May, 1913, for treatment of an ulcer of her scalp. She said that she had first noticed an ulcer in 1909. In May, 1913, it measured 1 in. by $\frac{3}{4}$ in. It was very painful. It was treated first with X-rays. As the rays produced no visible effect, zinc ionization was tried. This was equally unsuccessful. Then the two methods of treatment were combined. Still there was no beneficial result. The treatment mentioned lasted over a period of ten months, and at the end of that time the ulcer had increased in size to $1\frac{3}{4}$ in. by $1\frac{1}{2}$ in. In March, 1914, the base and edges of the ulcer were cauterized by diathermy. The cauterized parts sloughed away and bare bone was left on the base. Three months later there was an area of dry dead bone measuring 2 in. by $1\frac{3}{8}$ in., with evidently healthy tissue at the surrounding scalp-edges. There had been no pain since the diathermy. At the present time—seventeen months after the diathermy—the bone has the same appearance; it measures $1\frac{7}{8}$ in. by $1\frac{1}{2}$ in., and it shows no signs of separation. The scalp-edges are red in parts, but not thickened nor infiltrated. It will be seen, then, that there has been no recurrence of

growth for seventeen months. There has been occasional pain but it has not been severe. The periosteum covering the bone in the region of the ulcer had evidently been destroyed by the diathermy. If such destruction was unnecessary some method of preventing it during the diathermy must be devised.

Major TURRELL, R.A.M.C.: In my opinion the ordinary method of bipolar surgical diathermy is too uncertain in its depth of action for the treatment of growths with important subjacent structures. Such cases are more safely treated by condenser couch diathermy fulguration.[1] By this method the amount of destruction can be observed during the whole of the treatment, and, though its action is limited in depth, by repeated séances extensive growths of considerable depth can be safely removed.

(*October* 20, 1916.)

War Injuries of the Jaw.

By H. ANNESLEY ECCLES, M.D.

JAW radiography still retains its limitations, and there is yet a large field of research work open to those who have the time and opportunity in this connexion. The superposition of the adjacent bony structures make this part of the skeleton difficult to radiograph. The opposite side of the jaw, and the spine, will at times appear in the picture, and make it difficult or impossible to read. However, notwithstanding its limitations, experience has proved that the surgeon and dental surgeon find jaw radiography of great value, otherwise they would not have recourse to its aid so frequently and persistently. At the special jaw hospital to which I am attached at present, every patient is radiographed at the very earliest opportunity after admission, and frequently three or even four subsequent radiographs are taken to note the effect of treatment or the presence of sequelæ.

Jaw radiography is used for the following purposes, and I show a few lantern slides to illustrate the various uses:—

(1) To diagnose the presence or absence of a fracture.

(2) To ascertain the position of the fragments before and after treatment.

[1] See *Proceedings*, 1916, ix, p. 92.

(3) To note whether or not there are roots of teeth in the fracture. A very frequent cause of non-union is a root in the fracture, and radiography appears to be the only certain way of ascertaining its presence.

(4) To ascertain the degree of union of the fracture.

(5) To ascertain the state of the bone with regard to disease—for example, inflammation, necrosis, cystic and malignant disease, &c.

(6) To ascertain the state of a bone-graft.

(7) To detect and localize foreign bodies.

The skiagrams I show are taken without the aid of any special apparatus, and the methods are so simple that they can be carried out in any X-ray room.

DISCUSSION.

Dr. G. B. BATTEN: How does Dr. Eccles manage to keep the patient's head still when taking the antero-posterior view? This appears to be simple, but I find it is rather difficult to accomplish.

Dr. ECCLES: In reply to Dr. Batten's question, the patient should lie on his back on the X-ray couch, a small sandbag being placed underneath the head just above the occipital protuberance, so as to tilt the chin downwards and backwards. This enables the full length of the rami to be radiographed, and avoids the superposition of the mastoid or zygomatic processes. The head should be placed in as straight a position as possible with the nose pointing vertically upwards. The fluorescent screen should rest upon two adjustable supports such as those described by Dr. Finzi and myself in the *Journal of the Röntgen Society* for April, 1916. It should now be lowered so as to exert a moderate amount of pressure on the patient's nose, and afterwards removed, and the plate placed in position with the film side next to the face. The screen is now replaced so as to hold the plate and the patient's head firmly in position. I fully agree with a remark made as to the importance of the radiography of the maxilla, and I am of opinion that the method adopted at St. Bartholomew's Hospital is the best to show this portion of the jaw. The patient lies on his back on the couch and the head is inclined obliquely at an angle, and the maxilla is screened in this position. In answer to the inquiry with regard to the radiography of the condyle, I find that this is the most difficult portion of the mandible to show clearly, but I have devised a method which seems to be of service. Time will not permit me to describe it, but I hope that some day, perhaps after the War is over, we may meet to discuss this subject more fully.

(*October* 20, 1916.)

X-ray Appearances in Gas Gangrene.

By Agnes Savill, M.D.[1]

Undoubtedly the most terrible of all the horrors connected with war which come under the notice of the surgeon is gas gangrene. Dramatic in the suddenness of its onset, the rapidity of its progress, and the repulsiveness of its too frequently fatal outcome, it has reaped a cruel harvest of our young and vigorous manhood. Throughout the laboratories of Europe the bacteriologists are working to unravel this sinister problem. Just as tetanus has practically disappeared, so we may hope that gas gangrene will yield before the knowledge of science. At present so many germs are detected in the infected tissues that it is impossible to attribute the disease to any one microbe in particular. The depth of tissue invasion, the kind of tissue attacked—subcutaneous tissue, muscle or blood-vessel—may prove to be as important factors in determining the benign or fatal outcome as the variety and number of hostile germs. It is now well established that even when the most dangerous organisms are present the patient may run a steady course towards recovery, and hence research is directed to discover the direct and the contributory causes of the two types of gas gangrene—the localized and the massive forms.

At the Scottish Women's Hospital, Royaumont, France, of the cases admitted between July 1 and mid-September, 1916, no fewer than 304 contained the organisms of gas gangrene, and of these over 100 presented clinical evidences of gas. Royaumont is situated only 25 miles behind the firing line, and cases reached us from six hours to three or four days after being wounded according as they were sent to us direct or detained just behind the line for a few nights' rest. We had received warning that the offensive was to begin at the end of June, and when, on June 25, we heard the great guns thundering by day and by night we knew that our share in the labour entailed by all military success was about to begin, and we held ourselves accordingly in readiness. On July 2 the anticipated rush commenced, and for ten

[1] Physician to the Electrical and X-ray Department, Scottish Women's Hospital, Royaumont, France.

days, with scarcely any intermission, our chauffeuses were on the road; our surgeons, nurses and orderlies remained at their posts for sixteen to twenty-two hours out of the twenty-four.

It was our rule that every wounded man was examined at once on admission by one of the surgeons, and a specimen was at the same time sent to the bacteriological laboratory. Cases with clinical signs obviously demanding immediate surgical intervention were taken to the X-ray room and operation theatre with as little delay as possible. Cases which did not present any clinical evidence of gas, but whose wounds were reported to contain anaërobic infection were operated upon, other things being equal, in precedence to cases with simple septic organisms. By the autumn two outstanding facts were clear, that cases with gas infection who arrived within twenty-four hours of being wounded were usually saved, and that those who had been detained on the journey for three days were usually lost unless amputation could remove the infected area.

By the end of July I had definitely concluded that the X-ray plate was of great value to the surgeon, both in revealing the presence of gas and its extent and situation. But, unfortunately, it was not until September that I learned that the radiologist could also yield valuable aid, especially in the absence of a bacteriologist, by reporting in many cases the variety of gas infection. The surgeon can rely on the positive finding of the radiologist in much the same way that the physician can rely on the bacteriologist when diphtheria is found in a suspicious throat. The causes of failure to obtain good plates from over or under-exposure, or from over and under-development, are so numerous that no reliance can be placed on a negative skiagram which is contrary to clinical evidence. When a skiagram reveals the presence of gas there are but few fallacies (readily eliminated) and the surgeon can gauge the extent, the depth, and also I believe, in most cases, the variety of anaërobe at work.

I gratefully acknowledge that I owe the recognition of the possibility of forming the opinion as to the variety of invading germ to Dr. Pech, that enthusiastic and original radiologist who was in charge of the X-ray department of the Military Hospital at Creil. He showed me a large number of remarkable plates with a fine striation which demarcated the individual muscles, so that they resembled a sketch made in black and white. He had found that such an appearance was always associated with the *Vibrion septique*, and that the outlook for the patient was of the gloomiest. In all the cases at Royaumont I could

only find two with the same fine striation. Both died within twenty-four hours, but the *Vibrion septique* was found in only one of these. As Dr. Pech's X-ray room is situated close to the evacuation station, probably he received many more of the men taken from the train, who were too ill to travel even the few miles in an ambulance to our hospital. Dr. Pech is shortly to publish his careful and unusual observations in the French journal of radiology.

As soon as I had seen these plates at Creil I made an analysis of the plates at Royaumont, noting the X-ray appearance, the bacteriological report and the surgical finding.

Of 100 plates taken there were sixty-seven on which I base this report. Some had been lost, some broken, others were too thin (from under-development and hard tubes) to yield information adequate to form an opinion. Others were in such situations as the trunk and the hand; in the former the density of the part, in the latter the amount of bone present, concealed the gas outline in these particular cases in such a way that one could not give any reliable information. Three totally distinct appearances were observed in the plates which gave positive evidence of gas, and with few exceptions these corresponded to three distinct forms of infection. Whether this is always true future observations can alone decide. The first two appearances were common, the third rare.

(1) Simple *swelling*, with a pale misty outline which sometimes fades a little in indefinite areas in much the same way that a white fog looks thin in some parts of a landscape. The degree of the swelling gives some evidence of the amount of infection present. This form is found when *Bacillus perfringens* is the chief organism, and is probably due to the œdema which is usually associated with *perfringens*.[1] A woman with œdema of one side of her arm showed just the same pale, misty hue in the situation where clinical evidence of œdema existed.

(2) *Swelling*, and in addition there is a *cloud-like* outline, an appearance as if the flesh were replaced by dark woolly clouds. This aspect is due to infiltration of the tissues by the gas. In some cases there are seen rounded or oval-shaped dark outlines, where the gas is sharply demarcated. The diffuse cloudiness is the more common variety by far. For a long time, when a group of gas bubbles about the size of a three-

[1] Of course it is understood that in most cases there is a multiple infection present, and that septic organisms probably play an important part in promoting the progress of a gas infection.

penny piece occurred in close proximity, I believed the picture was due to a plate flaw. The *diffuse cloudiness* was in the great majority of cases associated with *perfringens* and *sporogenes* together. In gas *bubbles* frequently *perfringens* is the only anaërobe.

(3) *Striation.*—This third appearance was exceedingly rare. There were two types—coarse and fine. The fine striation occurred in only two of our cases, but Dr. Pech has a large collection of plates illustrating this rare condition. The dark lines of gas infiltration map out the individual muscle fibres in such a definite manner that the plate resembles a drawing of the muscles of a limb.

The coarse striation was also rare. It was associated with considerable swelling, whereas the fine striation cases were not so swollen at the time of the skiagram as their serious clinical condition would have led one to expect. The coarse striation occurred in limited areas, the lines of division were straight and always were seen in a longitudinal direction. The intensity of the infective process seemed to cause dissociation of groups of muscle fibres in an area which was usually about 2 in. long, but I show prints of cases which present the striation along the greater part of the limb (figs. 5 and 6).

In one of the cases of fine striation, fully detailed below, the *Vibrion septique* and *Bacillus perfringens* were found in the depth of the muscles and in the blood. In the other *Bacillus histolyticus* and *Bacillus fallax* were the uncommon germs present. Both cases were fatal. In the cases with coarse striation *Bacillus œdematiens* and *Bacillus Hibler IX* were the most frequently occurring organisms. *Perfringens*, of course, was present in every case.

Fallacies to be avoided: (1) Actual loss of tissue, which is frequent with the extensive wounds of present-day warfare, causes a dark irregularly outlined aspect which may mislead the radiologist who has not seen the wound, but cannot possibly mislead the surgeon. (2) I have seen an extensive ecchymosis give the swollen, misty and fading away appearance exactly resembling the common œdematous *perfringens* infection. I have not seen enough cases of ecchymosis to be in a position to state whether it can be distinguished from the *perfringens* invasion, but there could not be any clinical difficulty in the differential diagnosis. (3) I believe abscesses may simulate gas bubbles. As *perfringens* is so all-prevalent in military surgery to-day it is difficult to say whether in these abscesses some gas was not present. In any case the indications for the surgeon are similar in both cases.

Perhaps when speaking of fallacies it is well here to emphasize the fact that no reliance must be placed on negative plates. Two cases occurred which forcibly impressed this truth on my mind. Both men had been wounded in the region of the hip, and the skiagrams revealed nothing abnormal in the dense area of flesh in the neighbourhood of the joint. Both developed gas a few days later and died. On the other hand it is only fair to say that two plates without abnormality, plates of patients who afterwards developed gas, were taken about a week before death. In most of the fatal cases the positive skiagrams had been taken within two to four days of death. The negative skiagrams, therefore, may have been negative because at the date they were taken only gas infection, without gas formation or gangrene, was present.

Prognosis and Bacteriological Analysis.

These paragraphs must only be regarded as a tentative contribution to a very complicated subject. So many points have to be taken into consideration with each individual that the province of the radiologist is only one of the factors which have to be duly weighed by the surgeon. In brief, the first type, that with simple swelling, is usually the most favourable for recovery provided correct surgery is resorted to immediately. Except where the swelling was very extensive—as for example, along an entire forearm or leg—the course after free incisions and adequate after-care was in most cases uneventful. Where the general condition is not good on admission, owing to lack of resistance to the toxin, the surgeon watches the case, on the alert to amputate if need be.

Of twenty-six plates showing only simple swelling *perfringens* was the only anaërobe present in twenty-five cases; *sporogenes* accompanied it in one.

Where the second type is present, with swelling and cloudy tracing, the course towards recovery may be steady after free incisions have been made. Where the cloudiness indicates deep-seated gas, unrecognizable clinically, the outlook is exceedingly serious, and when in such a situation that amputation or other operative measures cannot reach the infection to stem its advance, death ensues.

Of twenty-four plates showing both swelling and cloudy tracing, *perfringens* was the sole anaërobe in three cases; *perfringens* and *sporogenes* were associated in twenty-one cases; and in four the dangerous organisms, such as usually caused some degree of striation,

were detected, *Bacillus œdematiens* in three, and *Bacillus histolyticus* in one.

When striation is present, fine or coarse, in my limited experience the prognosis is very bad. Our two cases with fine striation died with rapid massive gangrene. Of fifteen with coarse striation six died in spite of amputation and every care; six required amputation; three had extensive removal of gangrenous muscle. In those three and in one requiring amputation (fig. 6) no other anaërobes than *perfringens* and *sporogenes* were found. In the other cases with striation dangerous anaërobes such as the *Vibrion septique*, *Bacillus œdematiens* and *Bacillus Hibler IX* were present.

NOTES OF CASES WITH FIGURES.

Fig. 1 is an example of a simple *perfringens* infection. The skiagram was taken the day after the wound. It shows the considerable degree of swelling and the pale misty appearance which is probably due to the yellow œdema so usually associated with *perfringens*. The pale aspect of these plates often resembles the skiagram produced by a hard tube. Where the swollen area is not too extensive, free incisions, without amputation, bring about cure, as in this case.

Fig. 2 is typical of many cases with a mixed *perfringens* and *sporogenes* infection. It was taken on admission the day after being wounded. The swollen muscles protruded from the foul wounds, and the general condition was serious. The torn gangrenous muscle was cut away and free incisions were made from elbow to wrist on both flexor and extensor aspect. Two and a half months later, assisted by galvanism after the wounds had healed, there was practically perfect movement of the arm. In this print the swelling and cloudy tracery are typical of the double infection. In such cases the prognosis was good when admitted before a general poisoning of the system had taken place, and when the local gas was not too deep-seated.

Fig. 3 shows the same infection, but deep-seated, as far as the bone, extensive, gas being diffused along the entire leg. The appearance is characteristic of the double infection. There was crepitant swelling and yellow œdema and gangrenous muscle. In spite of amputation the gas spread upwards. Reamputation high up in the thigh could not arrest its progress and death followed. A skiagram taken of the amputated thigh muscles showed no striation present.

Fig. 4 is characteristic of the fine striation which I believe heralds the most serious condition. A. S., a Senegalese lad, aged 20, was admitted with a temperature of 100° F., and a running pulse of 126. He had been wounded three days previously in the right leg, and at the field hospital the shattered

fragments of the right fibula had been removed the day after the wound. On arrival at Royaumont the leg was somewhat swollen and cold and the general condition aroused anxiety. Within two hours the leg was amputated high up in the thigh. The muscles in the position did not appear unhealthy. Professor Weinberg happened to be at Royaumont that day, and four hours after

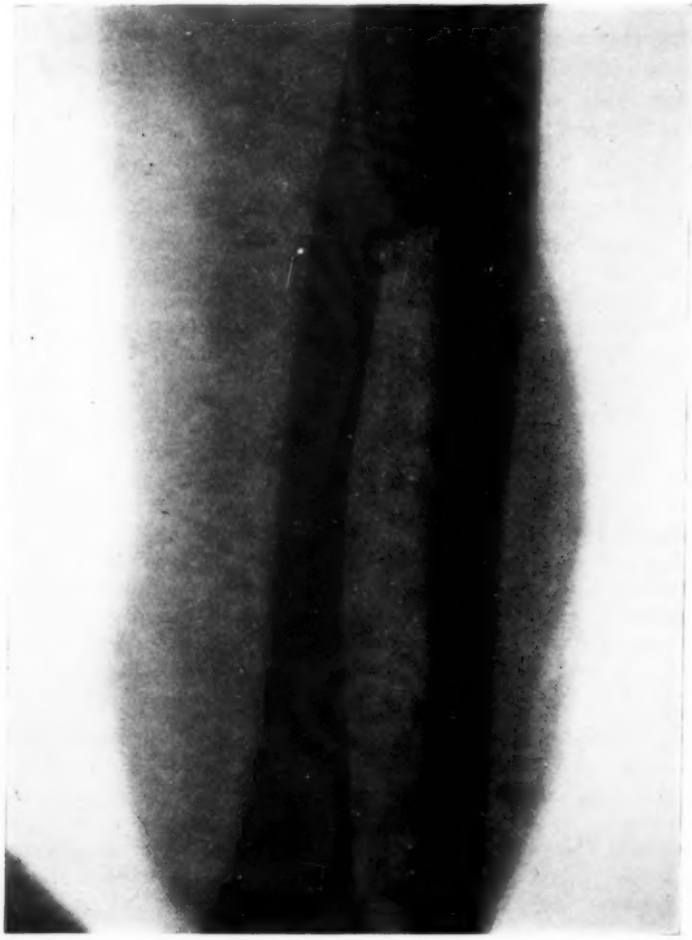


FIG. 1.

Pale misty swelling frequently found with *perfringens* infection. Free incisions; good recovery.

admission the patient received a dose of his serum which had been prepared at the Pasteur Institute, a mixture against *Bacillus perfringens*, *Vibrion septique* and *Bacillus œdematiens*. The pulse and temperature improved and next morning the stump looked healthy. The patient became restless during the day and



FIG. 2.

Swelling and cloudy outline. Position of metallic fragments shows extent of swelling at site of wound. *Bacillus perfringens* and *Bacillus sporogenes*. Recovery with complete use of arm in two and a half months.

the temperature and pulse began to rise. About six o'clock he seemed delirious, sat up in bed talking, then fell back in a syncope from which no stimulant nor even the intravenous saline could rouse him. Before seven he was dead. Then it was noted that the stump had swollen to thrice its size, and early the

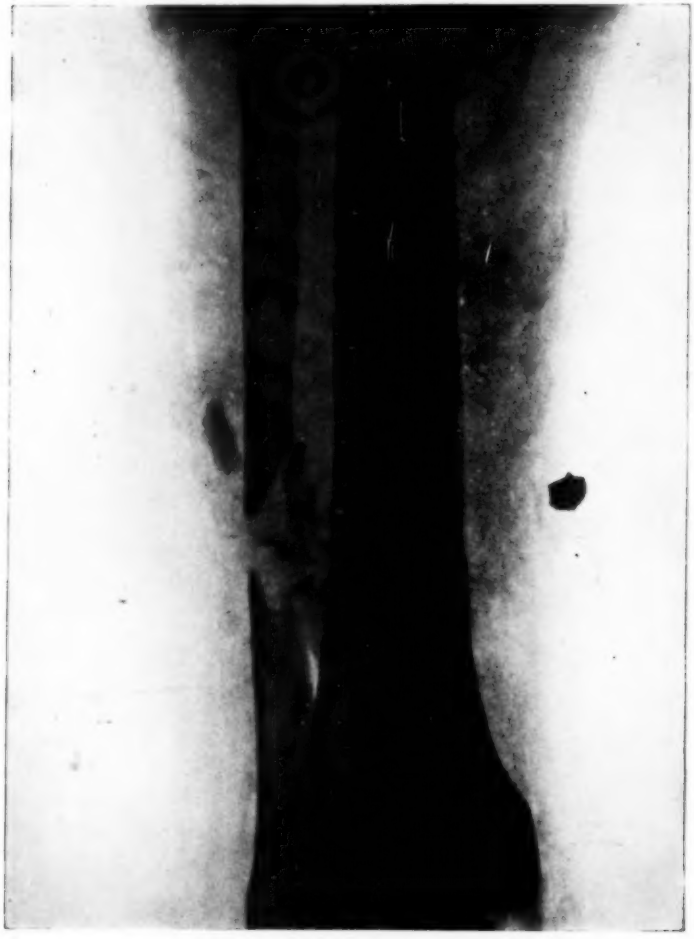


FIG. 3.

Cloudy tracing showing deep-seated and extensive gas infiltration. Extended upwards in spite of amputation; death ten days later. Clinically: crepitant, brawny swelling; yellow œdema and dark red gangrenous muscle.

next morning it was almost impossible to place the distended body into a coffin. Both in the muscles of the amputated leg and the swollen stump after death and also in the blood, there were found *perfringens* and the lethal *Vibrion septique.*



FIG. 4.

Fine striation. Skiagram taken three days after wound. Fatal issue next day. *Bacillus perfringens* and *Vibrion septique.*

Figs. 5 and 6 are excellent examples of coarse striation. There were only three of this type in which the coarse striation extended so far, from one joint to the joint above it.

Fig. 5: This patient arrived late at night in a very serious condition. Amputation was performed at once, but he succumbed in a few hours.



FIG. 5.

Coarse striation. Skiagram taken day after wound. Fatal issue same night.

Examination of the limb showed that the gangrenous muscle corresponded to the area of striation seen in the skiagram, and where the normal shadow is seen the muscle was not much affected.



FIG. 6.

Coarse striation. Skiagram two days after wound. (Artery injured and red degenerated muscle.) On third day arm blue, cold and swollen. Amputation saved life.

Fig. 6 is an example of a case in which the skiagram revealed serious trouble about thirty hours before the clinical signs became severe. The man had not much swelling of the forearm on admission. The wound was opened up freely and the foreign body removed. The artery was discovered to be injured and was ligatured. I was at that time (early in July) unaware of the importance of a skiagram showing striation, and therefore no account of it was taken. A day and a half later the forearm was found to be cold and discoloured and greatly swollen. Amputation was performed immediately.

The bacteriology of both these cases was simpler than in the majority of cases with striation. In the first there were found only *perfringens* and *sporogenes*. Yet the skiagram has none of the cloudy tracing which was so commonly found with these organisms (cp. fig. 3). In fig. 6 *perfringens* and streptococci were found. Possibly the injury to the artery had something to do with this unusual form of gas permeation in a condition showing *perfringens* and streptococci alone.

Another case may here be cited to prove the prophetic value of the skiagram. The patient arrived on a Saturday evening, and the plate taken on admission revealed a cloudy tracery over almost the whole area of the thigh which was visible on a 12 in. by 10 in. plate. In one corner there was slight but unmistakable coarse striation. Now the wound was three days old but apparently superficial. A piece of shell had glanced across the anterior surface of the thigh, tearing away a portion of skin. I did not then realize the importance of the skiagram, but when two days later I showed it to Dr. Pech he gave a gloomy prognosis. On the evening of admission the wound had been freely opened and pieces of gangrenous muscle had been removed, the discovery of which had put the surgeon on the alert lest danger should ensue. For two days all apparently went well. Then the whole thigh swelled up. A circular amputation was performed but the patient died next day with gas extending up the trunk. The bacteriological report mentioned *perfringens*, *sporogenes*, and *œdematiens*. Had we realized that the skiagram revealed deep-seated gas far from the site of the wound, stringent measures on admission might have saved life.

(*October* 20, 1916.)

The Recognition of Gas within the Tissues.

By H. Martin Berry, M.D.[1]

One of the many important subjects which the War has thrust upon the notice of the medical profession is that of the action of the various groups of anaërobic bacilli. A prominent effect of certain of these micro-organisms is the formation of gas within the tissues of the living body, and it is the detection of this gas, and therefore of the presence of the bacilli, which forms the subject of this communication.

The number of deaths which have already occurred as the result of anaërobic infection makes the subject of great practical importance, especially so since early diagnosis is the first step towards favourable prognosis, as is the case in all bacterial infections. If a wound merely be infected by anaërobes without the formation of gas bubbles, radiology, so far as we know at present, can be of no assistance towards diagnosis, but, if gas be present in the tissues, it can be demonstrated in a well-made radiograph. Not only its presence, but also its exact location can be depicted, and thus we can help the surgeon in deciding what course of treatment to pursue. In two at least of the following cases there were no symptoms suggestive of gas gangrene, as the process had not advanced to such a stage that either local signs or systemic disturbances had manifested themselves, but the early recognition of the presence of gas led to prompt surgical intervention, followed by rapid recovery.

When gas has been formed in the body as the result of fermentation of muscle sugars, consequent on anaërobic infection, it frequently happens that it has no method of escape, and therefore acquires a positive pressure which is sometimes considerable. This pressure tends to increase the infected area either, according to one theory, by limiting the blood supply of the part, or, according to another theory, by splitting up the tissue planes and thus laying open fresh avenues for infection. We need not concern ourselves with considering which of these theories is correct, since in either case the effect is the formation

[1] Radiologist, Royal Herbert Military Hospital, Woolwich.

of more gas resulting in a further increase in pressure, and thus a vicious cycle is established—perhaps the most potent argument for the necessity for early diagnosis.

In order to give some idea of the frequency of these cases, even at a hospital in England, those only have been included in which a diagnosis of gas formation has been made during a period of three months. Such cases, naturally, will be more common at our various hospitals in France. Many patients known to be infected with gas-forming organisms have been operated upon abroad and the gas liberated so that they show no radiographic evidence of gas formation on their arrival here. Despite this there are twenty-eight cases to be included. In practically all of the cases there is surgical or bacteriological evidence to substantiate the diagnosis. For the surgical notes I am indebted to Captain Jocelyn Swan and Captain Cecil Rowntree, and for bacteriological information to Mr. Kenneth Goadby. In most of the examples of diffuse infiltration clinical examination gave unmistakable evidence of the condition by tissue crepitation but, even in these cases, radiography was useful in localizing the area affected. In the cases with only a few discrete bubbles nothing abnormal could be felt. In a few instances limbs were dissected after amputation to check the diagnosis, and in each case confirmed it. The extent of tissue damage bears no necessary relation to the presence of gas; it was found in small punctured as well as in large lacerated wounds. In some cases the gas bubbles were not in the direct track of the wound and had apparently moved along the path of least resistance. Nor does the smell of any discharge which may be present act as a guide, as gas was found not only in those with foul discharge, but also in instances where it was quite sweet or absent altogether.

Radiographically speaking, there are two main types of gas formation:—

(1) Where there are a comparatively small number of discrete bubbles, though individual bubbles may be of large size.

(2) Where there is an extensive and diffuse gas infiltration.

In either condition the gas may be found under considerable pressure. Further experience may lead to extension of this classification.

There are certain fallacies to be guarded against in making a diagnosis of the presence of gas:—

(1) The actual wound may involve such a loss of tissue as to cause increased radio-transparency of the part. This is very common, but is easily distinguished and is hardly likely to lead to error.

(2) Bubbles of air may be trapped within the tissues. This is not so common, and the shadow outline is apt to be less defined. There is only one case in the series where this diagnosis was suggested.

(3) If the wound has been syringed with peroxide of hydrogen, oxygen bubbles may be present under considerable tension. Inquiries should be made therefore as to whether the wound is known to have been so treated and due caution exercised. This is particularly the case where the bubbles are few and discrete, and in the present state of our knowledge there is no certain means of differentiation, though, if the gas be situated at some distance from the track of the wound, it is more likely to be the result of the infection than of the treatment. In diffuse infiltration this is practically certain to be the case, and diagnosis can be made with confidence.

Case I.—The radiograph was taken four days after the wound was inflicted. It shows several discrete bubbles together with a fair amount of diffuse infiltration. As will be seen, there is extensive bone damage. Clinical notes:



FIG. 1. (Case I.)

Wound very foul, crepitation felt in the tissues, radial pulse normal on the affected side. Surgical notes: The part was freely opened up and drained, and the man improved for several days; later, the head of the humerus and the glenoid were removed along with much bone debris; the bone was soft, blackened, and foul; the patient is doing well. Bacteriological notes: A smear preparation taken at the first operation showed spore-forming bacilli of the malignant œdema type.

Case II.—The radiograph, taken on the fifth day, shows extensive and diffuse gas infiltration of the calf, without any bone damage. Clinical notes: The inner and posterior portions of the leg were distended, and crackled on

pressure, but the swelling did not extend into the thigh; the toes were just warm; there was no pulse in the dorsalis pedis or posterior tibial arteries. Surgical notes: A long incision was made in the leg in the region indicated and gas escaped under considerable pressure: the gas had lifted the subcutaneous tissues off the muscles, but the muscles themselves were not emphysematous; the smell was very foul and the condition so bad that amputation was performed. On the following day he had secondary hæmorrhage from another wound in his arm and the third part of the axillary artery was ligatured. The next day gangrene of the arm developed, with much emphysema, and the arm was amputated at the axillary fold, but the emphysema spread to the chest wall and he collapsed and died, mentally clear to the end. Bacteriological notes: A swab taken at the first operation showed a mixed infection of *perfringens*, malignant œdema, staphylococci, streptococci and *rodella*.



FIG. 2. (Case II.)

Case III.—The plate, taken eight days after the wound was received, shows diffuse gas infiltration around and behind the greater trochanter and extending down the thigh. Clinical notes: This man had had an operation in France; a drainage-tube had been inserted and the wound sutured around it; on admission the wound was very foul, the skin dusky and tense and emphysema was felt on pressure. Surgical notes: Operation the same day as the radiograph was taken; the deep fascia had been lifted off the muscles by gas, which was under great tension and spurted from under the ilio-tibial band, but the muscles themselves were not emphysematous; the patient appeared to be doing well after operation, but he suddenly collapsed and died two days later. Post-

mortem changes were very rapid: twelve hours after death the whole body was blackened and much swollen; in thirty-six hours distension was so great that the skin cracked in all directions and much fluid exuded. So great was the distension that it actually bulged out the sides and forced off the lid of the coffin in which he had been placed, so that a larger coffin had to be obtained. Bacteriological notes: The infection was a mixed one of *perfringens*, *lactis*, *aerogenes*, malignant œdema, *Hibler*, streptococci and staphylococci.

Note.—This and the preceding case are the only two deaths in the series of twenty-eight cases, and the similarity between them is very marked. In both there was a mixed infection; in both there was an extensive diffuse infiltration by gas, and the connective tissues were lifted off the underlying muscles; in both the smell was foul.

Case IV.—The plate, taken five days after infliction of the wound, shows a large metallic fragment in front of the elbow and a large single bubble of gas in its immediate region. Damage to the external condyle of the humerus was



FIG. 3. (Case IV.)

also observed. Clinical notes: There was no evidence of gas formation but the whole area surrounding the wound was very tender and the man complained of great pain on the slightest movement; in his other forearm he had a fracture of both bones, but this could be handled freely without pain. Surgical notes: On operation the gas was found under considerable pressure; the external condyle was found to be separated and damaged, and was removed, as was also the metallic fragment; he is doing well. Bacteriological notes: The infection was *perfringens*, *proteus* and streptococci.

Case V.—Radiographed three days after being wounded. The plate shows gas bubbles deep in the tissues of the back of the thigh. Clinical notes: The wounds were small and healing rapidly; the part was not markedly tender, and there was absolutely no suspicion of infection by anaërobic bacilli. Surgical notes: Operation was performed as soon as the presence of gas was

reported; a long incision was made and a cavity found which was full of recent blood clot: this clot was disintegrating and contained gas in its interior. Bacteriological notes: The clot showed a very extensive infection with *perfringens*. The surroundings were particularly favourable for the growth of anaërobic bacilli and, had the infection once passed beyond the limits of the clot, acute gas gangrene was inevitable; this was averted by prompt treatment. The patient made an uninterrupted recovery and has now been discharged from hospital.



FIG. 4. (Case V.)

Case VI.—Radiographed on the third day, and an extensive gas infiltration seen behind the knee. He did well after free incisions. Bacteriological notes: The infection was *perfringens* and diplococci.

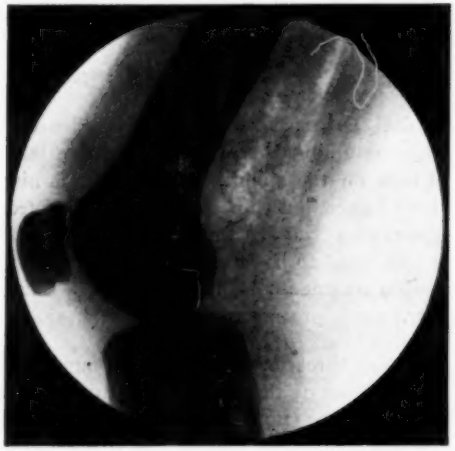


FIG. 5. (Case VI.)

Case VII.—Radiograph taken three days after the wound shows a large metallic fragment situate deeply behind the knee joint and a large single gas bubble in the same region. The gas was found to be under high tension. The patient did well. No bacteriological examination was made.

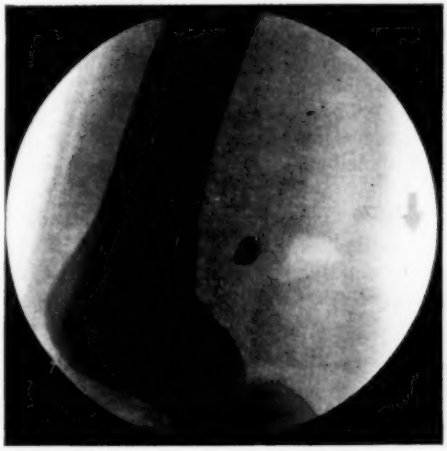


FIG. 6. (Case VII.)

Case VIII.—Radiograph taken on the ninth day showed a single large bubble in the tissues of the neck. Clinical notes: The bullet had entered at the back of

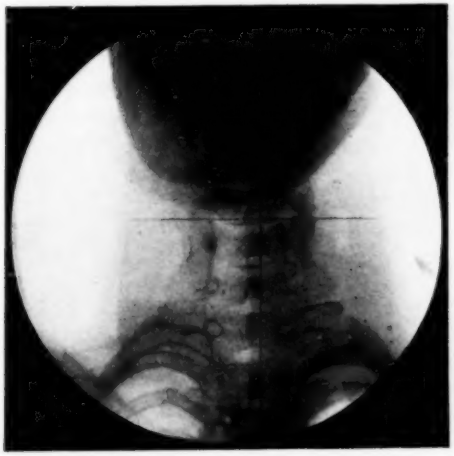


FIG. 7. (Case VIII.)

the neck, 2½ in. to the left of the spine of the seventh cervical vertebra, and had been removed from the right submaxillary region prior to his admission. He had symptoms suggestive of pharyngeal injury and this, taken together with the appearance of the gas bubble as seen in the radiograph, caused a tentative diagnosis of an air bubble to be made. On operation this proved to be correct; the cavity was found to communicate with the pharynx through a wound in its wall, and, as further proof, a bacteriological examination of the contents of the cavity disclosed the presence of yeasts, streptococci and a large coccus which appeared to be of mouth origin.

Case IX.—Radiographed on the sixth day and evidence of gas bubbles in the leg obtained. Clinical notes: The leg was not swollen; there was no emphysema and nothing to indicate the presence of gas, which was not suspected. Surgical notes: Operation was undertaken purely on the strength of the radiographic report and disclosed gas between the gastrocnemius and the soleus; there were small areas of black muscle digestion but no smell. Bacteriological notes: Infection was by perfringens and streptococci.

No useful purpose is to be served by mere multiplication of cases narrated. The foregoing have been selected as being representative and the remainder merely resemble one or other of them.

One suggestive fact was noticed throughout the whole series—viz., that, in each case characterized by the presence of much gas, *Bacillus perfringens* was found on bacteriological examination. From this there appears to be reasonable grounds to believe that the presence of this particular anaërobe may be diagnosed radiographically, though the absence of radiographic signs cannot be taken to imply the absence of the bacillus.

(*October* 20, 1916.)

Note on the Microscopic Histology and Bacteriology of Gas Gangrene.

By KENNETH GOADBY, L.R.C.P., M.R.C.S., D.P.H.Cantab.[1]

HAVING been associated with Dr. Berry for the last two years at the Royal Herbert Hospital, Woolwich, and having made bacteriological examinations in a number of cases of gas gangrene, many of which have been diagnosed from the X-ray photographs taken by Dr. Berry, he has asked me to supplement his remarks with a few slides illustrating some of the special points relating to gas gangrene from the purely bacteriological and histological side. I have therefore brought slides illustrating micro-organisms found in gas gangrene and the microscopic anatomy of the tissues in this acute form, as it may perhaps assist the radiographer to see the actual tissue changes which are taking place in the production of gas by the action of certain micro-organisms, partly aërobic and partly anaërobic, from the muscle sugar and glycogen of the tissues.

The photographs show that the bacterial infection is not limited to the area of gas infiltration, but exists at a considerable distance from the gas infiltrated area. The blood-vessels, and mainly blood-vessels, have carried the infected bacteria into the healthy muscle before stasis and coagulation of the tissue has taken place.

Two of the slides are from the actual cases of which you have already seen the radiographs, and the bacterial cultures are such as were obtained from the cases.

[1] Hon. Bacteriological Specialist, Royal Herbert Hospital, Woolwich.

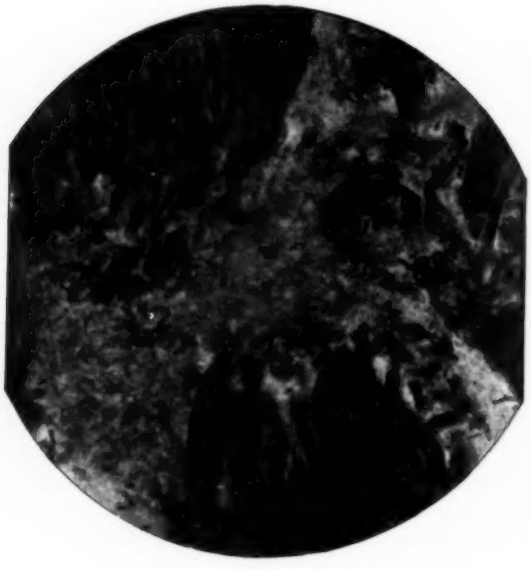


FIG. 1.

Section of muscle in a case of gas gangrene, with deep emphysema, showing destruction of muscle bundles and masses of bacteria closely surrounding the disintegrating muscle bundles.

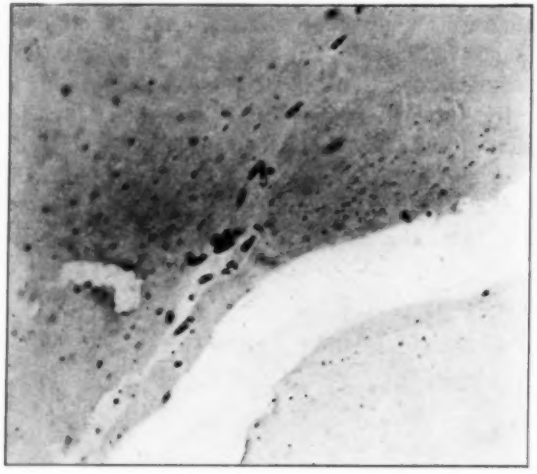


FIG. 2.

From the same case, showing spore-forming organisms of the *malignant œdema* type situated in the disintegrated muscle.

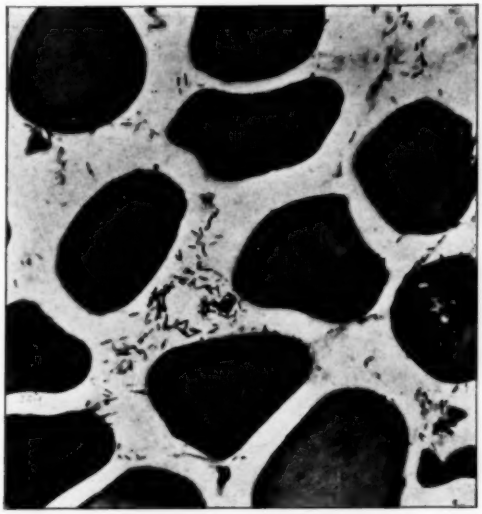


FIG. 3.

Gas gangrene from Case III (Dr. Berry), showing the separation of the individual muscle bundles; diagnosis of gas from the radiograph was made in this case. Interspersed between the muscle bundles are seen large numbers of Gram-positive bacilli (*Bacillus perfringens*).

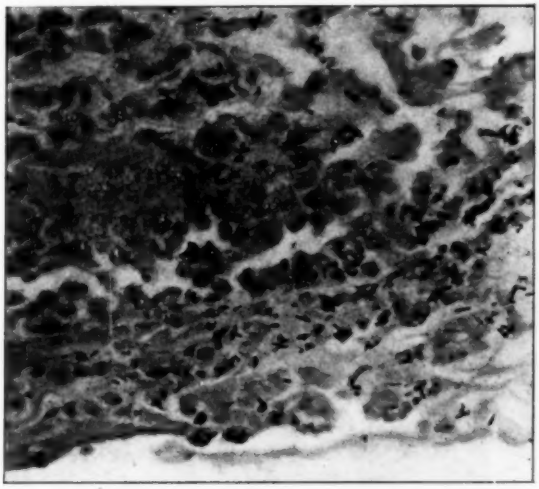


FIG. 4.

Gas gangrene, Case IV (Dr. Berry), showing the early spreading of the disease in the immediate neighbourhood of the area of gas infiltration; large numbers of bacilli are seen, many of them situated within the polymorphonuclear leucocytes, showing that tissue reaction is taking place and that the infiltration of bacteria is in advance of the actual areas of emphysema.

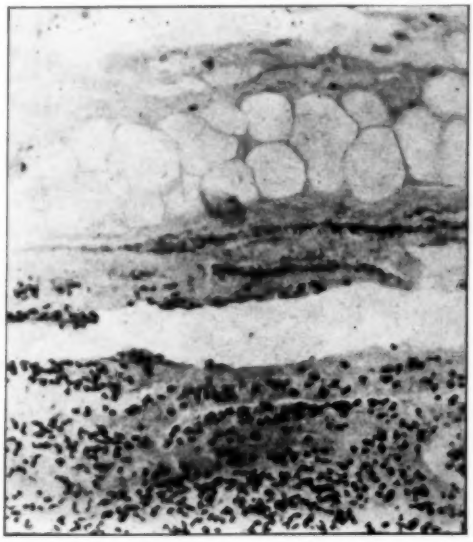


FIG. 5.

A portion of tissue from Case III, showing a small vessel just underneath the subcutaneous tissue, the walls of which are rendered specially distinct by the Gram-positive bacteria attached to them. This is taken from just inside the gas infiltration area, and shows a long crack in the tissue caused by gas infiltration.



FIG. 6.

Pure culture of *Malignant œdema* (*Koch*) obtained from the case of fig. 1.

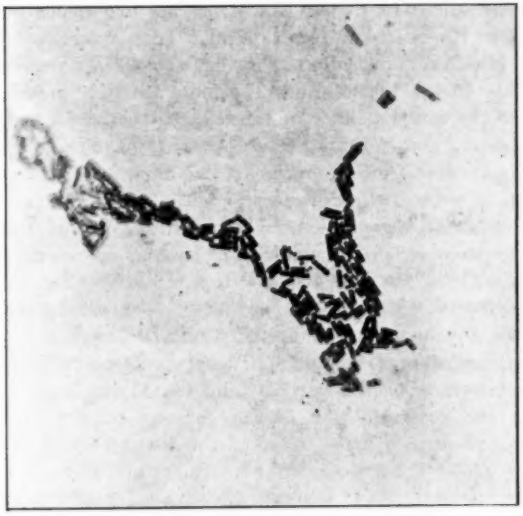


FIG. 7.

Pure culture of *Bacillus perfringens*, the organism actually found in gas gangrene with a large amount of tissue emphysema. The organism shows the curious granular staining frequently seen.

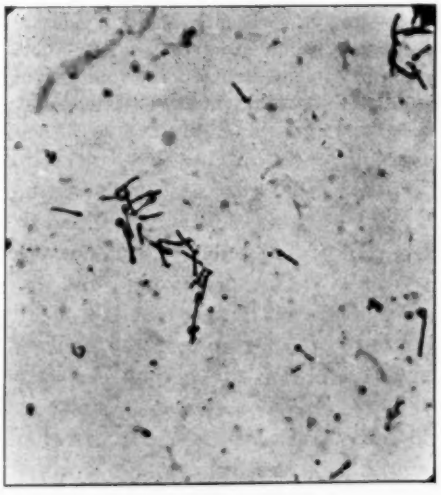


FIG. 8.

An impure culture of *Bacillus Hibler IX*, showing early spore formation, one of the three common organisms met with in gas gangrene.

Captain R. H. JOCELYN SWAN, R.A.M.C.: My experience of gas gangrene is that gained in England, but I have seen a large number of cases of it. I look upon the condition as (1) that of a very acute emphysematous cellulitis, rapidly spreading in the tissues, and (2) a more localized infection, limited to the area around the wound; it is in these cases that radiography assists the surgeon. In septic gunshot injuries the routine bacteriological examination reveals the presence of gas-forming anaërobic organisms in the deeper part of the wound in a large proportion of cases, but it is only in a small proportion of these that the infection becomes acute. In a critical examination of skiagrams in gunshot injuries there is often evidence of gaseous collections in the tissues, as has been shown in the series of slides exhibited by Dr. Agnes Savill and Dr. Berry, and I look upon these appearances as a strong indication to the surgeon to open up the wound thoroughly. In more than one case I have been led to proceed to operation on the strength of the radiographic evidence where the wounds have been apparently slight. At the same time there may be fallacies in the interpretation of the gaseous bubbles in the skiagram. Areas of decreased density may be due to the loss of tissue in the wound, to the presence of gas in the tissues after treatment of the wound with peroxide of hydrogen, or after injuries in which the air passages may be opened. I may mention a case in which a bullet passed obliquely through the neck, injuring the lower part of the pharynx, which showed a collection of gas in the side of the neck in the radiograph. On opening the wound some milky fluid was found with a wound at the side of the pharynx, and bacteriological examination showed the absence of any gas-forming organisms. Dr. Berry's observations on the X-ray examination of gunshot injuries are important, particularly where he showed that gas bubbles have been present in the intramuscular planes extending from the primary wound, and they supply another example of the close association which must exist between the radiographer and the surgeon, not only in the diagnosis of the exact injury sustained but as an indication of the treatment required in the individual case.

(*October* 20, 1916.)

Ultra-violet Radiation.

By W. J. Turrell, Major R.A.M.C.

You will recollect that at a meeting of this Section in January last Dr. Cumberbatch and Dr. Macgregor read notes on several cases treated by the so-called Simpson light. Most of these cases were either cured or showed marked improvement. Dr. Sequeira, at the same meeting, clearly demonstrated the non-penetrating character of this radiation, and its identity with the already recognized method of treatment by ultra-violet radiation. Dr. Russ also showed that, tested by the spectroscope, the radiation from the Simpson or wulframite electrodes was practically identical with that from metallic tungsten. It was, I think, perfectly obvious from the papers and discussion that evening, provided that tungsten was readily obtainable and burnt satisfactorily in an arc consuming a small amount of current, that the metallic electrodes would prove a far more satisfactory source of ultra-violet radiation than electrodes made from the impure ore.

I endeavoured without success for some months to obtain a supply of tungsten from the makers of electrical apparatus and from other sources. I was told everywhere that tungsten had always been very difficult to obtain, and that since the War it had become practically unobtainable. Discouraged by this failure I was compelled at last to have recourse to the Simpson electrodes, both in private and in hospital work. These electrodes I found burnt badly, always with much spluttering, and they frequently required grinding before they would burn at all. They were, moreover, very expensive, as they cost £2 the pair and were very quickly consumed. Subsequently I was able to obtain two short rods of tungsten, each about 1½ in. in length. These burnt very steadily, produced what I may call the erythema dose in a comparatively short time, and were very slowly consumed. Patients whom I had already treated by radiation from the Simpson electrodes and whom I subsequently treated by the tungsten radiation, all expressed themselves as having been more benefited by the radiation from the metal. This

increased benefit was I think due to the greater intensity of the radiation from the metal and the greater accuracy with which the dose could be administered owing to the more steady flame.

A short time ago I had the good fortune to get into communication with the British Thomson-Houston Co., and they very courteously and kindly supplied me with a number of tungsten rods of different sizes. They assured me that there would not be the slightest difficulty in supplying any quantity of these rods that was likely to be required for medical purposes. The price was £4 4s. per lb.[1] This price compares exceedingly favourably with that charged for the Simpson electrode, especially when it is borne in mind that the metallic electrodes last about ten times as long.

The Simpson lamp, costing £25 to £30, struck me from the first as a needlessly costly and bulky piece of apparatus, for there is no reason why a simple arc lamp with sufficient insulation to take a current of 15 amp. should not answer every purpose required. After some search I bought a No. 9 Empire Arc Lamp for £3 3s. from Messrs. Butcher and Sons. This proved such a satisfactory lamp in every way for the purpose that I have purchased a similar one for the hospital. At my house I work my lamp off my X-ray resistance, clamping the lamp to the tube-stand so that it can be readily raised or lowered. If one has not a suitable resistance this piece of apparatus will cost an additional £4 9s. 6d. The price of the lamp has recently been raised 12s.

In my hospital work I have been using an amperage of 9 to 11; in my private practice I use about 7 amp. With the present pressure in hospital work it is necessary to make the exposures as short as possible. Moreover, the heavier currents seem indicated by the researches of Cernovodaeno and Henri, who found that a twenty-four hours' culture of colon bacilli was completely sterilized in one second at a distance of 20 cm., with a current of 4·7 amp. at a voltage of 140, whilst with 2·3 amp. at 23 volts an exposure of 300 seconds was necessary to sterilize the culture.

The therapeutic value of ultra-violet radiation appears to be due to its destructive action on micro-organisms and to the active hyperæmia which it induces in the superficial tissues. The latter is probably of the greater clinical value, and within certain limits the intensity of the

[1] There are five 6½-in. rods in 1 lb. The price of tungsten 98 per cent. pure (quite pure enough for radiation purposes) has been fixed by the Government at 6s. 3d. per lb. A licence from the Minister of Munitions is necessary in order to obtain it at that price.

erythema produced in the skin in a given time may be taken as a measure of the clinical efficiency of the radiation from the different arcs.

In conjunction with Mr. Schunk, of Ewelme, near Oxford, I recently made a few simple experiments on this erythema dose. Different areas of the skin of the forearm were in turn exposed to the action of the following electrodes: Simpson electrodes, metallic tungsten, cored carbons containing uranium oxide, Ur_3O_8, and the powders of tungsten, wulframite and iron. The exposure was for five minutes at a distance of 12 in., with a current of 5 amp. The most intense erythema was obtained from the metallic tungsten, the next in intensity was that from the Simpson electrodes. None of the other electrodes produced any erythema in the five-minute exposure.

At Oxford we use the two methods of treatment by the ultra-violet radiation, that is by the use of a reflector and by the use of a quartz condensing lens. In using the reflector method the structure of the mirror is of considerable importance when dealing with wave lengths of the ultra-violet region. Silicon, which reflects 75 per cent. of the ultra-violet rays, is the most efficient. Nickel reflects 40 per cent., copper 35 per cent., and silver only 6 per cent.[1] In this method of treatment, the patient is placed about 12 in. from the arc and the reflector confines the radiation as nearly as possible to the treated area. We usually adopt the focusing method. In this the quartz lens is held in a wooden shield, and the rays are focused on the area to be treated. In the treatment of indolent wounds special attention is directed to the stimulation of the granulating edges. The shield holding the lens should be sufficiently large to protect untreated areas from the action of the light.

In administering tungsten radiation, special attention should be paid to the protection of the eyes of the patient and operator. Hooded frames containing No. 2 Crookes glass should be worn. I am showing a pair of such glasses made by the Oxford Optical Co. I think also if much treatment is done a mask or veil should be worn by the operators. Or it might possibly be more convenient to fit a shield to the lamp to cut off the extraneous rays.

In addition to the large lamp, which of course requires heavy wiring, I am showing a small lamp, also made by Butchers, which only takes $4\frac{1}{2}$ amp., and can therefore be used from the ordinary lighting circuit.

[1] "The Reflecting Power of Metals in the Ultra-violet Region of the Spectrum," *Astrophysical Journ.*, 1915, xlii, p. 205.

Smaller electrodes are used in this lamp; a finer point of light is obtained, which is very convenient for the focusing method.

In the selection of cases for ultra-violet radiation it is important to bear in mind the superficial action of their treatment. Indolent and extensive sloughing wounds have been much benefited, but the results obtained have been much better where one or two preliminary treatments by zinc or chlorine ionization have been administered. In barber's itch and pustular eczemas the treatment has been very useful, but here again preliminary zinc ionization conduces to a speedy cure. Certain cases of ulcerating lupus have improved. The nodular type, with little or no ulceration, have not been benefited, unless they have been previously fulgurated. Cases of eczema of the face and arms have done very well, some of them very old standing cases. Eczema of the hands, where, of course, the skin is thicker, has not done so well. A case of lupus erythematosus shows temporary improvement at any rate. The treatment of rodent ulcer by ultra-violet light has not been successful in my hands unless the case has been freely fulgurated; radio-therapy of these cases is far more satisfactory. The erythema set up by the radiation is sometimes useful in allaying the pain of neuritis.

It has been suggested that ultra-violet radiation might be more conveniently and more efficiently administered by means of a tungsten filament in a quartz globe. The British Thomson-Houston Co. very kindly constructed for me a 1,000 watt 100 volt tungsten filament lamp of 2,000 candle-power. On examining this lamp with the spectroscope Mr. Schunk found that its spectrum stopped at 3,700, and therefore gave no ultra-violet rays. The result was, of course, to be expected, for when a solid body is heated to incandescence the wave lengths stop at about 3,500, and a continuous spectrum is obtained. This spectrum is the same with different solids, and does not extend to the ultra-violet region. On the other hand when metals in an electric arc are used, the metal is heated to volatilization and the specific radiation and spectrum of the metal used is obtained. With mercury vapour, as in the Kromayer lamp, there are a few very intense lines in the ultra-violet region, but with the tungsten arc there is an almost continuous spectrum in that region.

To sum up briefly, the therapeutic value of ultra-violet radiation has been established for some years. The electric arc is the simplest and most convenient means for its production. The amount of ultra-violet

radiation obtainable from any metallic electrode appears to be directly proportionate to the melting point of the metal. Tungsten has the highest melting point of any metal obtainable, therefore tungsten arc electrodes would appear to be the most efficient source of ultra-violet radiation at present available.

I should like, in conclusion, to express my indebtedness to Mr. C. A. Schunk for much valuable information with regard to the physical properties of ultra-violet radiation.

Since writing this paper I have received the following letter from Mr. Schunk, from which it will be seen that the conclusions at which he arrives are in accordance with those stated above :—

"The following are the *tentative* results at which I have arrived, and of which, if of interest, please avail yourself on Friday :—

(1) The tungsten arc is the most intense source of ultra-violet light so far experimented with, the lines being so numerous and close together that they constitute almost a continuous spectrum.

(2) The Simpson electrodes give the tungsten spectrum with the addition of the iron lines, but on the photographic plate with the same exposure and amperage they are not so intense; no doubt this is due to the unsteadiness of the arc, compared to that from the tungsten rods.

(3) Half-inch cored carbon electrodes filled with wulfram (75 per cent. tungsten and 25 per cent. iron) burn very steadily, and give a spectrum identical with the Simpson electrodes, but the intensity as recorded on the photographic plate is not quite so marked.

(4) The iron arc is very rich in lines in the ultra-violet region, but compared to tungsten fails at about wave length 2,500, whereas tungsten extends to wave length 2,160 without much diminution.

(5) From the spectroscopic point of view, giving the Simpson electrodes and the carbons cored with wulfram an increased exposure compared with the tungsten arc, one would expect to get the same result from the point of view of ultra-violet light treatment.

Hyperæmia Experiments.—Using the tungsten arc and a quartz cylindrical condenser, forming an image 18 in. from the condenser, of about ½ in. wide, in the form of a strip on the skin :—

(1) *A Piece of Ordinary Thin Photographic Glass.*—No reaction in two and a half minutes. Spectroscopic examination 3,000 wave length recorded.

2) *A Piece of Microscopic Cover-glass.*—An appreciable reaction in two and a half minutes. Spectroscopic examination, 2,700 recorded.

These tentative experiments tend to show that radiations of 3,000 Angström units and upwards have no effect as judged by the hyperæmic effect, whereas from 2,700 wave length to 3,000 wave length there is an effect."

Sir J. MACKENZIE DAVIDSON, M.B., exhibited a Director for indicating the position during operation of foreign bodies localized by X-rays.

Section of Electro-Therapeutics.

President—Dr. Harrison Orton.

(*November* 17, 1916.)

DISCUSSION ON THE TREATMENT OF WAR INJURIES BY ELECTRICAL METHODS.

The President: We are having, to-night, a discussion on "The Treatment of War Injuries by Electrical Methods." In spite of its extreme importance, this form of treatment is, as a rule, referred to in a very perfunctory and off-hand manner; the whole matter, so to speak, is pushed into a corner, and nobody gains any further knowledge about it. The subject is, however, deserving of far greater interest, and I hope that this evening we shall hear some new facts and have a very interesting debate.

I call upon Dr. Turrell to open the discussion.

The Electrical Treatment of the Wounded.

By W. J. Turrell, Major R.A.M.C.

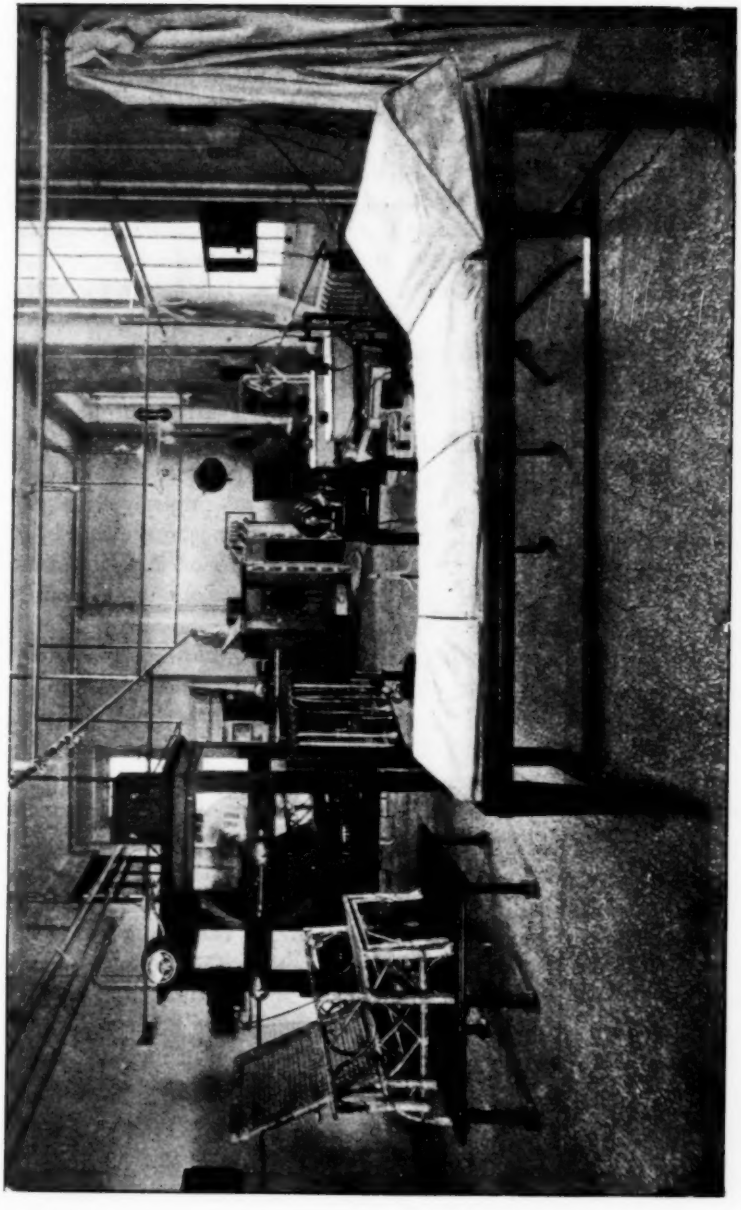
The treatment of wounded soldiers by electrical methods appears to have been first systematically practised by the French in 1907, during the Morocco War. Two very interesting articles dealing with this subject have appeared in the French medical papers. One of these, entitled "The Physio-therapy Department of the Military Hospital at Dey, in Algeria," by Dr. Hirtz, appeared in the *Archives d'Électricité médicale*, March 10, 1913; the other entitled "The Physio-therapy of those wounded in War," was read by M. Miramond de Laroquette before the Congress of the French Academy for the Advancement

of Science held at Tunis in 1913. It is interesting to note that Dr. Hirtz holds strong views on the importance of the independent control of physio-therapy departments. "The department of physio-therapy," he says, "forms a self-governing division in the same way as a department of medicine or of surgery. It was only by degrees that we obtained this independence, and we attach very great importance to it. It is only under such conditions that good and exact work can be done. Each department is thus rendered responsible for its own work."

The physio-therapy department comprised a subdivision for electro-therapy, one for radiology, and one for mechano-therapy. The number of patients treated during the year 1913 amounted to 323. Dr. Hirtz remarks that, as is usually the case, the greater part of these, that is 236, fell to the lot of the electro-therapy department. The cases treated by electro-therapy included: articular rheumatism (acute or subacute), neuralgia, neuritis, paralysis, functional troubles caused by wounds, synovitis, arthritis and hydrarthrosis. The 236 cases required 12,543 individual séances. The cases treated by radio-therapy included: cervical adenitis, arthritis, certain maladies of nervous origin and superficial tumours. Chronic effusions in the joints were treated by pastille doses of X-rays once a fortnight.

The success of the establishment led to the formation of similar departments in all the large French military hospitals. Both writers claim that without the aid of physio-therapy the number of those permanently disabled, and in receipt of state pensions, would have been much increased.

The fully equipped electro-therapeutic department at the Radcliffe Infirmary, Oxford, opened in November, 1913, was in full working order at the outbreak of war. We were therefore able to avail ourselves of its services on the arrival of the early convoys of wounded at the Third Southern General Hospital, of which hospital the Radcliffe Infirmary forms a section. It will probably facilitate discussion if I give a brief outline of the electrical methods adopted in our department. As regards results, it is, of course, absurd to compare those obtained at the physio-therapy department at base hospitals, which are regarded by some unenlightened medical officers as the dumping ground for all obstinate or incurable cases, with the results obtained at Command Depots, to which only those cases likely to be fit for foreign service within six weeks, are usually sent. We rely a good deal on ionization. Except when treating wounds or mucous membranes we do not attach any importance to the specific action of any special solution, but invariably



Electro-therapeutic Department, Radcliffe Infirmary, Oxford.

use a 2 per cent. solution of common salt. We make use of large pads, strong currents, and séances of as long duration as the time of the department will permit. Cases treated in this way include subacute and chronic rheumatism, some cases of neuritis, impetigo, sycosis, septic and indolent wounds, stiff joints, &c. Where the limitation of movement in a stiff joint is due to fibrous bands, or adhesions, ionization is often very useful; in these cases massage and passive movement should be performed as soon as possible after the conclusion of the electrical séance. Where the stiffness of a joint is merely due to cold and to rigidly contracted muscles, immersion of the limb in hot water for twenty minutes at a temperature of 115° F. will probably be all the treatment, preparatory to massage, that is required. If expense and space are of no consideration the water may be kept continually agitated by spouting jets (the "eau courante" system), or by a motor-driven propeller (the whirlpool bath system) and thus the molecules of water, cooled by radiation, are constantly replaced by hotter molecules. The consideration of this refinement, however, more properly belongs to another Section. The treatment of indolent wounds and ulcers by zinc ionization gives excellent results; after about two or three treatments by this method I now change to ultra-violet radiation from the tungsten arc and obtain more rapid healing. Ionic medication of foul extensive and sloughing wounds with the chlorin ion rapidly abates the odour, and leads to rapid healing, with smooth flexible scars and free movement of the neighbouring tendons. I believe that this class of case is far too rarely sent to the electrical department for treatment.

In addition to the electrical department, I have charge of sixty surgical beds. I have therefore had an opportunity of trying this method on early cases. The first case I treated in this manner was a patient with a gunshot wound of the forearm received at the range of only one or two yards. Captain Selous, the big-game hunter, has, I am told, pointed out that a bullet striking a limb at a very short range has a very similar effect to that of an explosive bullet. Anyhow this man had a small bullet wound over the interosseous space about the middle of the extensor surface of his forearm, and nearly the whole of the flexor surface of the forearm was replaced by a superficial foul sloughing and very offensive mass; there was no fracture and no nerve lesion. The wound was treated the day after admission by chlorin ionization. In twenty-four hours there was no offensive odour, and in two or three days the wound was quite clean, and, I think, in six or seven weeks the patient was discharged for light duty with the wound healed, a very flexible

scar and perfect movement in his wrist, hand, and fingers. Probably many of you have had equally successful results with this treatment, and I do not bring this case forward solely as a witness to the success attending this method, but because of an unaccountable rise of temperature which suddenly occurred about the eighth day with no other symptoms and no physical signs, the wound remaining perfectly healthy in appearance. The temperature rose to about 105° F. and subsided in about two or three days. This case occurred early in the War, and I regret that I am unable to show you the temperature chart. I have, however, had another very similar case with a higher range of temperature, rising to 106° F., the chart of which I will show you on the screen at the conclusion of my paper. This case was also a wound of the forearm similar to the one I have described, but not quite so extensive nor so foul. It was similarly treated, and there was nothing in the wound or its neighbourhood to account for the pyrexia. I shall be extremely interested to hear if any of you have experienced these alarming rises of temperature in the treatment by ionization of extensive, superficial, and sloughing wounds. I have not experienced any similar rises of temperature when treating deep suppurating wounds and sinuses, however septic they might be.

Many superficial wounds in my wards have been treated by hypertonic solution, but I cannot admit that the osmotic pressure of these concentrated solutions can be so effective as the ionic exchange excited by the application of the galvanic current, and I have not found in practice that the use of hypertonic is in any respect as effective as ionic medication. Hypertonic solutions are certainly more easily and readily applied than ionic medication, and therefore have their sphere of usefulness in the treatment of septic wounds.

Recently we have been trying Dakin's tubes with Dakin's solution in the treatment of deep cavities associated with bone lesions and necrosis, and have obtained good results by this method. The treatment, however, is only suited to a special kind of wound if the best results are to be obtained. It is ideal when the wound is a deep sulcus in which the fluid may collect with no sinus leading off and no counter opening. The sinuses are apt to develop an air block and so prevent the permeation of the fluid. Ionic medication is more penetrating in its effect and is not so much affected by these local conditions. I will show you presently a chart of a case of suppurating gunshot wound of the forearm which very much improved on the substitution of ionic medication for the Dakin method.

Nerve injuries, contusion, concussion, compression, and section, partial or complete, form a large proportion of the cases that are sent to the department for treatment. If the nerve reacts to the faradic current we utilize rhythmically reversed faradism from a coil of low co-efficiency, and in this we follow the generally recognized practice. There is not the same consensus of opinion in regard to the choice of treatment when there is no reaction to faradic stimulation, some preferring condenser stimuli, some the sinusoidal current, and others rhythmically reversed galvanism. Of these the last mentioned is the simplest and cheapest; it is, moreover, quite painless, and very efficient. We are told that when a nerve is severed, or otherwise rendered functionless, the muscles which it supplies hang flaccid like hammocks from their attachments, waste and toxic products accumulate within their substance, fatty degeneration takes place, and finally, if untreated, conversion more or less complete into fibrous tissue occurs, so that by the time the nerve has regenerated, the muscle has lost all contractile power. Our object in treatment is therefore to maintain the nutrition and contractility of the muscle while the nerve is undergoing the process of regeneration. The sudden sharp contraction elicited by the rhythmically reversed galvanic current seems to me the ideal method for the removal of these waste and toxic products, and for the maintenance of the muscular tone. We follow out the technique advocated by Bergonié for the treatment of infantile paralysis, and feel satisfied that we are in good company in following the lead of a teacher to whom electro-therapists owe so much.

This treatment is so simple in its application, the apparatus needed is so inexpensive, and the results obtained are so good, that it is deplorable that it is not more generally adopted at the smaller hospitals and convalescent homes. All of you must have seen limbs, permanently disabled by neglect or inefficient electrical treatment, the function of which could have been more or less completely restored, if the proper methods had been followed. At nearly all hospitals and convalescent homes, however small, there is usually some form of apparatus for the supply of the continuous current, and only the addition of a clockwork metronome, costing about £2, together with a little instruction and occasional supervision of those in charge, is needed to ensure the recovery of many cases, which if neglected, will become a permanent charge upon the State.

I do not intend to deal with the subject of muscle and nerve testing beyond laying stress upon the importance of associating the examina-

tion of the voluntary movements and sensation with the electrical reactions. Although we are all ready to admit the importance of this, in practice we are apt to neglect it, unless we methodically associate these procedures with the reactions. In order to co-ordinate these observations, and to keep a record of them, we make use of a card which I will show you on the screen presently.

Bergonié's apparatus for the production of electrically provoked exercise we find most useful in restoring the tone of muscles wasted from disuse or slight nerve disturbance. We have recently been extensively using it with great success for the development of the quadriceps extensor after surgical removal of the semilunar cartilage. Electro-therapy like other methods of treatment has usually failed in most cases of severe "shell shock"; many of these cases have very marked electrophobia, and electrical treatment tends to aggravate their symptoms. There is, however, one class of nerve shock in which the Bergonié treatment generally results in a speedy cure. These are the cases which are under the fixed impression that they have lost all power in their lower limbs and are unable to walk or even to stand up. One or two vigorous séances on the Bergonié chair are usually sufficient to convince them that there is still some contractile power in their muscles, and they are then soon able to stand, and to walk without assistance.

The chief indication for the use of diathermy in the treatment of the wounded is severe pain; unfortunately this is a frequent indication The hyperæmic changes induced in the tissues by the passage of the diathermic current are of great value in the treatment of conditions where the local nutrition is at fault. The high rate of oscillation of the electrons in the tissues promotes drainage and produces frictional heat. It has several times been pointed out how completely and efficiently diathermy fulfils the indications for treatment in trench foot; relieving the pain when all other means have failed, reducing the stasis and congestion of the parts by tissue drainage, and diminishing the loss of tissue to a minimum. The results claimed for diathermy in this treatment have now been confirmed by many workers. Volkmann's ischæmic contracture exhibits symptoms which seem to indicate the application of diathermy. I have treated three such cases by this method. Two were very advanced when they came under my care and were not in the least benefited. The other was an early case and the progress of the disease was arrested and very marked improvement took place.

Two interesting cases of local tetanus have shown distinct symptomatic improvement under the influence of diathermy. One of these patients was sent to me by Major Hurst to have his reactions taken. The muscles of the left shoulder were rigidly contracted, exhibiting an almost incredible degree of stony hardness (I will presently show you a photograph of this case). To take the electrical reactions of muscles in such a condition was clearly an impossibility. The patient had previously taken an anæsthetic very badly, so I decided to try the effect of diathermy. The application of this current resulted in a degree of relaxation far greater than that obtained from any other application, and even slightly greater than that obtained some days later by complete anæsthesia. The relaxation lasted for some hours and rendered the taking of the reactions quite a simple matter.

The other case occurred in the muscles of an officer's leg. Diathermy was regularly applied; definite relief was experienced after each treatment. Since writing this paper I received yesterday morning the following letter from this officer, in which he very clearly describes his symptoms and the effect of the treatment:—

November 15, 1916.

DEAR MAJOR TURRELL,—I have been meaning to write to you for some time, as I knew you would be interested to hear how I was getting on. Your letter has just been received, and I am only too happy to give you any information I can with regard to my leg. I was wounded in the left leg on October 13, 1915, by high explosive shell, and arrived at Oxford on October 22. There was no operation as the surgeon in charge did not consider it advisable to remove the pieces of shell; my leg seemed to be getting better, and after about a month I was able to hobble round with sticks. My foot at this time used to swell a great deal towards night, and the foot seemed then to gradually stiffen up with violent pains at intervals, this gradually spread up the whole leg to about the knee, and I was compelled to take to my bed again. The pain at times was very bad, similar to a very bad attack of cramps, and then my leg became rigid and stiff, and at other times used to get horrible jumps and it was impossible to keep it still, and whenever the doctor or nurse looked at it it used to stiffen up at once. The night seemed to be the worst, and consequently I got very little sleep. I often had to get up in the middle of the night on crutches to try and obtain relief, my leg was so cramped and sore. It was about this time that you first visited me and prescribed a course of electric treatment for my leg, and I shall never be able to thank you enough for the relief it gave me. I cannot remember the names of the different treatments, but the first one—diathermy, or heat pads—certainly relieved the pain, and after the first two or three visits to you I got immense relief. I never looked back after this, and, although the progress was slow, I gradually lost all pain

and was able to get sleep at night. The nervous jumps slowly disappeared and my leg became gradually normal except for contraction of the tendons. I was unable to straighten my ankle or knee, and it was thought at one time that my tendo Achilles would have to be severed. Gradually the knee straightened and I was able to get my heel to the ground. I was for some time on crutches, and was able to leave the hospital on February 5, 1916, walking with sticks. . . . I am now able to walk comfortably, but am unable to flex the ankle more than at right angle to my leg. The circulation is not very good, and I feel anything tight round my calf. I am still getting Boards, and have not been passed fit for overseas yet.

This officer was treated by ionization on December 6 and 7, 1915; by diathermy on December 7 to 22, 1915; also occasionally by static breeze, ionization, and chlorin ion, to relieve contraction, on December 29, 1915, to February 4, 1916.

Of course the treatment of such cases by diathermy is purely symptomatic, and in no way replaces the need for antitoxic serum or other specific treatment. The treatment is, nevertheless, a very important one in these cases, both on account of the immediate relief which it affords, and also on account of its tendency to prevent the permanent muscular shortening or contraction which has been found likely to result in these cases.

In relieving the pain of sciatica, neuritis, lumbago, and many like conditions, diathermy is of the greatest value. In dealing with sciatica I have practically abandoned all other methods of physical treatment; early cases are cured quickly, some of old standing require much perseverance and patience, and occasionally one comes across a case which shows no improvement. Electro-therapy would be an uninteresting and a tame proceeding if we had all successes and no failures. The mistake which I have often made in dealing with the obstinate cases has been in chopping and changing from one treatment to another. I believe that if sciatica will not yield to diathermy no other electrical method will benefit it, unless, perhaps, radio-therapy. As a preliminary to the passive movement of stiff joints the analgesic effect of diathermy and its influence in diminishing the spasmodic contracture of neighbouring muscles are of great service. In no other electrical modality is technique of such importance as in the application of diathermy. After four years' daily experience I am continually altering, and I hope improving, my methods.

The chief points to which attention should be paid are :—

(1) To apply the current slowly.

(2) To be careful to apply it in the right direction, and over the right area.

(3) To continue the application for a sufficient time. A minimum of 15 to 20 minutes.

(4) To obtain as great a heating effect as is consistent with safety.

(5) To select the size of pad and electrode suitable for the extent of area to be treated.

It is quite easy to treat cases by diathermy without producing any benefit, it is also possible by correct technique to cure these cases. Take, for instance, that most troublesome complaint coccyodynia. By following the routine method, and placing one electrode on the abdomen, and the other over the sacrum and coccyx, and giving the usual dose, probably no improvement will result. On the other hand, by adopting a more intensive method, most cases can be cured. The patient lies on his abdomen, a cylindrical metal electrode is placed in the rectum, and a small pad and electrode, about the size and shape of the coccyx, is placed over that bone, and the current slowly administered to the toleration of the patient. I have personally cured five or six cases in this way; the last was a coccyodynia, in a wounded soldier, following a contusion of the spine.

The high frequency vacuum tube we find useful in some conditions of trench foot, and other painful conditions. Ultra-violet radiation is to some extent replacing the use of this modality in our work.

One of the most useful and indispensable forms of apparatus in the treatment of wounded soldiers is the static machine. The equipment of no electrical department is complete which does not include an efficient instrument of this kind. The unidirectional current of the static machine, with its enormously high potential and its minute amperage, can be produced by no other form of electrical apparatus; and it is the possession of these specific properties which renders the static machine capable of producing results in certain cases which are unobtainable by any other means. We know from our condenser testing that the vigour and amplitude of a muscular contraction depends chiefly upon the voltage used. In a healthy muscle with the condenser, charged at 50 volts, we get a weak muscular contraction, with a condenser of similar capacity, but charged at 100 volts, a stronger contraction results, and a more vigorous contraction still is obtained from a charge at 200 volts. Now, with the Morton wave current of the static machine the muscles to which the electrode is attached require a potential of several hundred thousand volts before a discharge can take place across

the 7 or 8 in. spark gap which is often used. So vigorous and of such amplitude is the resulting muscular contraction that we are enabled by this method to free muscular fibres from involvement in scar tissue by the force of their own contraction. So readily and accurately can the force of this contraction be regulated that, by alternately widening and approximating the discharging balls, we can make use of this current as a form of electrical *arthro-moteur* for the movement of stiff joints, in the hands and feet; and as a means of breaking down slight adhesions. This method is especially useful for breaking down the adhesions, which persist in trench foot after the subsidence of the acute and painful symptoms. In the Morton wave current these vigorous contractions alternate with periods of complete relaxation, and thus, by a form of automuscular massage, the stasis and congestion of recent sprains are removed and the return of mobility and function is often hastened by several days or weeks. In the same manner, with suitable technique, the reabsorption of the fluid in synovitis of the knee-joint can be rapidly promoted.

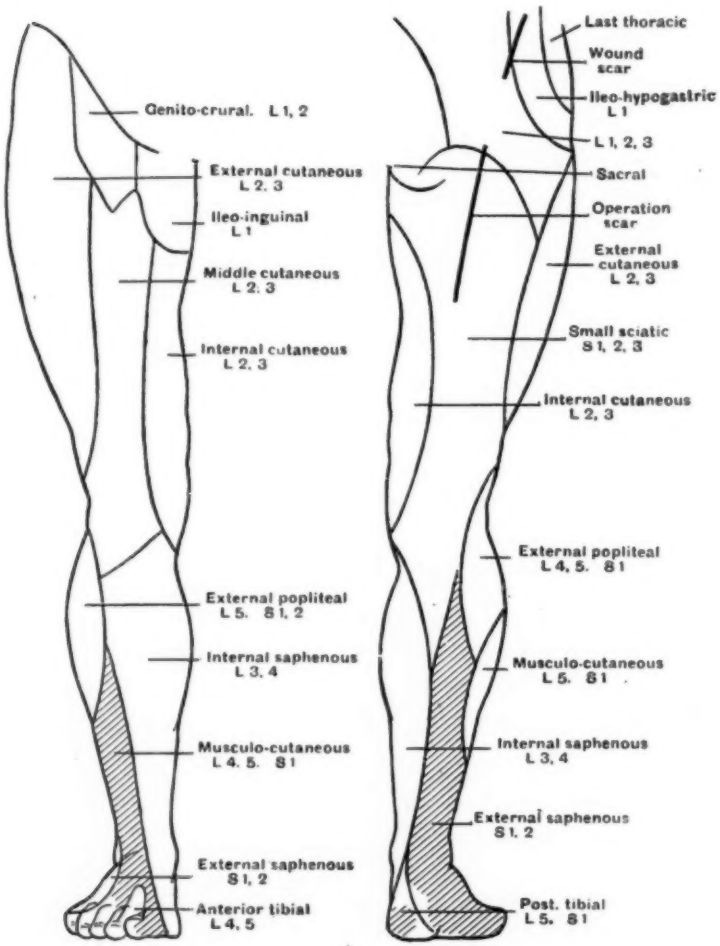
A full understanding of the modus operandi of this modality is necessary to enable us to appreciate in what a large number of cases, resulting from wounds and exposure, it is applicable. There is no form of electrical or other apparatus, by the use of which, quicker or more permanent results can be obtained. It seems to me surprising that no Command Depot and no military hospital, so far as I am aware, is equipped with an efficient static machine. The cost of the apparatus, about £200, is apparently regarded as an insuperable stumbling-block to its installation, and yet one successful case alone might be the means of recouping the State for this outlay by the expenditure saved on a single pension. Frequently at the very establishments where the need for economy is alleged as a bar to efficient equipment, one finds that, regardless of expense, pantostats, costing from £25 to £30 each, have been purchased instead of the equally or more efficient water resistances costing only a few shillings. At the Radcliffe Infirmary in three years, 8,000 static treatments have been given. With a charge of 6d. a treatment we should already have recovered the cost of the apparatus. The machine is still apparently equal to new. But I need not labour this point further: I hope that later on we shall hear Captain Humphris's remarks on this treatment; on account of his much longer experience he is able to speak with far more authority on these matters.

As I have so recently read a paper on "ultra-violet radiation" before this Section, I do not propose to detain you with any remarks

CHART FOR RECORDING ELECTRICAL REACTIONS USED AT THE THIRD SOUTHERN GENERAL HOSPITAL.

Lower Limb.

Reg. No.:
Name: Private Jones, T.
Regiment: 1/5
Injury: Gunshot wound left ilium, 25/9/15. Op. sciatic, 17/11/15.
Date: 25/7/16.



Nerve	Nerve root	Muscle	Condenser reaction
			micro-farads
Obturator	L 3, 4	Adductor longus ...	0·062
Anterior crural ...	L 2, 3, 4	Quadriceps extensor ...	0·062
	L 2, 3, 4	Sartorius	0·062
Superior gluteal ...	L 4, 5 ; S 1	Gluteus medius ...	0·062
	L 4, 5 ; S 1	Tens. fasc. fem. ...	—
Inferior gluteal ...	L 5 ; S 1, 2	Gluteus maximus ...	0·062
Great sciatic	L 4, 5 ; S 1, 2	Biceps	0·062
External popliteal :—			
Anterior tibial ...	L 4, 5 ; S 1	Tibialis anticus ...	0·33
	L 4, 5 ; S 1	Ext. long. hallucis ...	0·66
	L 4, 5 ; S 1	Ext. long. dig. ...	0·66
	L 4, 5 ; S 1	Ext. brev. dig. ...	2·0
Peroneal	L 4, 5 ; S 1	Peroneus longus ...	0·5
	L 4, 5 ; S 1	Peroneus brevis ...	Not taken
Internal popliteal ...	L 4, 5 ; S 1, 2, 3 ...	Semimembranosus ...	,, ,,
	L 4, 5 ; S 1, 2, 3 ...	Semitendinosus ...	,, ,,
	S 1, 2	Gastrocnemius ...	0·5
	S 1, 2	Soleus	0·5
Posterior tibial ...	L 5 ; S 1	Tibialis posticus ...	0·33
	L 5 ; S 1	Flex. com. dig. ...	Not taken
	L 5 ; S 1, 2	Flex. long. hallucis ...	,, ,,
Internal plantar ...	L 5 ; S 1	Abductor hallucis ...	,, ,,
	L 5 ; S 1	Flex. brev. dig. ...	,, ,,
External plantar ...	S 1, 2	Interossei	,, ,,

Diagnosis : Partial division of great sciatic ; regenerating.
Prognosis : Good ; improving.
Treatment suggested : Rhythmically reversed galvanism.
Result :
Range of voluntary movement.—Thigh : Extension, abduction, adduction, and rotation normal. Leg : (knee) flexion normal, extension normal ; (foot) flexion *nil*, eversion *nil*, extension *nil*, inversion *nil*.
Sensation : Much diminished over shaded area.

of mine, but we shall be very pleased to hear the experience of others on this subject. To summarize very briefly, the war services which electro-therapy can render to the State are as follows :—

(1) A considerable number of those, who would otherwise remain permanently unfit, can be rendered fit for general military service.

(2) The severe pain of many of those wounded or injured by exposure on military service can be completely arrested or greatly relieved by these methods.

(3) Electrical treatment is of great service in many of the less serious cases, often effecting a speedy cure and a quick return to the fighting line.

(4) Electro-therapy, fully and efficiently developed, will result in a very considerable reduction in the amount paid for State pensions, and, what is far more important, will restore function to many crippled limbs.

The last point we have to consider is the personnel of the staff of the electrical department; and this, though perhaps the most important point of all, I must deal with very briefly. A great deal of the electrical treatment administered at many of the convalescent homes and smaller hospitals serves no other purpose than to bring the treatment into disrepute. One frequently sees, as I saw to-day, a patient with complete reaction of degeneration of the musculo-spiral nerve, who had been treated in a perfunctory manner for five weeks with the faradic brush, with no other result of course than completely to destroy the patient's faith in all electrical methods. Under the present system, with about two months' training and the possession of a small faradic battery, anyone can become a so-called medical electrician, and practice as an expert the most specialized of all the medical sciences. I think that this condition of things could be largely remedied by appointing properly qualified electro-therapeutists as consultants to supervise and instruct in electrical methods, in the different hospitals and convalescent homes in their area. Of course the more important treatments, such as diathermy and static, would have to be conducted at properly organized and equipped departments. But, nevertheless, under proper supervision and control an amount of work, invaluable to the patients and to the State, could be performed in the auxiliary hospitals by means of ionization and rhythmically reversed galvanism. These suggestions are made in no carping spirit, but with a full recognition of the very valuable services which are being rendered by only partially trained nurses and masseuses, and with an earnest desire to increase the usefulness of these ladies, and to extend the benefits of efficient electro-therapy to regions which they have not yet reached.

With these exceptions, this Section cannot fail to be otherwise than much gratified at the development which has taken place in electro-therapy during this war, and at the increased recognition which it is daily receiving.

Dr. Grainger Stewart.

My own experience of electro-therapeutic methods has been mostly in connexion with neurology: and I am very willing to acknowledge to electro-therapeutics the great amount of success which, I think, everyone admits is due to it in the treatment of nerve injuries. But I am fully in agreement with what the speaker said in opening

this discussion when he remarked that in choosing the kind of electro-therapeutic treatment suitable for cases of nerve inju y, it is essential to correlate the clinical side with the treatment used, and further to supplement the electrical treatment with every other means of treatment which will aid in restoration of complete function to the affected limb.

One point which appealed to me very much was his reference to static treatment. I think that during the last few years electro-therapeutists have rather neglected static treatment. I know Sir William Gowers used to be a very strong advocate of static treatment in certain conditions of muscular atrophy. It is of use also in cases in which stiffness of joints is developing, not owing to the physical condition of the joint, but very often to a reflex or functional contracture of the muscles. Static electricity enables one to get contraction and complete relaxation: very often it prevents the joint becoming more fixed. Personally, I have seen, since the War commenced, a very large number of cases in which a stiff joint has resulted, perhaps partly because passive movement was not started soon enough, but partly also because the patient would not allow passive movement, either for functional reasons or on account of pain. I am certain that the easiest way of getting a slight amount of movement in these cases is by means of electrical stimulation. This can be carried out very gradually, and when once the patient sees and realizes that the joint can be moved without causing pain, it is astonishing what an amount of passive movement he will then permit. I am sure that if this method of treatment were employed earlier, we should have much less trouble with these cases of stiff joints.

With regard to the question of the treatment of muscles, I think faradic stimulation—certainly when you can get a reaction—is the best. Do not make it too strong, and always interrupt it. With regard to the cases which do not react to faradism, I have not used the condenser method at all, but I think the galvanic method as described is very serviceable. It is very simple, and if properly applied it gives very good results. I think that a great number of patients who are temporarily obliged to stay in bed, with perhaps a wound in one limb, an injured joint, or some other sort of temporary disablement, would be greatly benefited by having local electrical treatment applied to the unaffected parts of the disabled limb. An increase in the rapidity of recovery would take place if electro-therapeutic treatment were given earlier in many cases.

Captain F. Hernaman-Johnson, R.A.M.C.

Major Turrell's paper is a very able one. The subject is so wide that it will be possible for me to touch on one or two points only. In connexion with the treatment of muscle or nerve injuries, I do not think any mention has yet been made of the importance of what one may call the mechanical side of the matter. This is, perhaps, best illustrated by an example. A musculo-spiral lesion of moderate severity is very difficult to cure by electrical methods alone: treatment may, indeed, fail altogether. But if the fingers and wrist be properly supported by a relaxation splint of the type introduced—or, at any rate, strongly recommended—by Colonel Robert Jones, one finds that electrical stimulation immediately begins to take effect. Surgeons often make the opposite mistake—that of trusting to relaxation splints or massage alone, without using the electrical method. The proper course is to combine the two. When the muscles are very weak, it may be desirable to keep the part on the splint continuously. In such a case, if it be wrist-drop, one removes the bandages and leaves the arm on the splint, it being meanwhile steadied by a nurse. Then the muscles can be made to contract rhythmically without exerting a pull on the limb as a whole: they merely take up their own slack, so to speak. And we should be careful to see that the muscle is contracting as well at the end of the treatment as at the beginning. If at the end of the séance the muscle is responding less than at the beginning, the probability is that harm instead of good has been done. The best plan is to over-stimulate somewhat at the first sitting, and for some time afterwards only to give half the amount which previously caused fatigue. Owing to the pressure in military hospitals, much of this work has to be done by imperfectly trained men, and occasionally one finds that a case which had been doing well goes back. It is then generally found that there has been over-stimulation. This throws back the recovery for several days, and one must not recommence electrical exercise until after a period of rest.

This principle of relaxation should also be applied to certain cases of flat-foot. There has perhaps been a crush or blow at the ankle, and the arch of the foot is found dropped (I am not speaking of cases where actual fracture has occurred). You take the patient's foot in your hand, and mould it for a few minutes. If this will bring it back into shape probably it can be cured by massage, electrical, and, later, by voluntary exercises, combined with support. Colonel Robert Jones raises the

inside of the foot, sole and heel, ¼ in. (more or less), fills up the instep, and makes the patient walk on the outer side of the foot. That is practically applying a relaxation splint to the foot. These cases, if treated with this boot, by exercises, and by faradic stimulation of all the muscles of the leg, including those of the arch of the foot, are often found to be cured in a few weeks.

Injuries to the deltoid are common. Some patients object a good deal to an abduction splint, and in the case of the wrist it is often feasible to get sufficient rest for the deltoid by tying up the wrist at night to the top of the bedstead, and letting the patient sleep like that. That is also of value in the case of children with infantile paralysis.

It is essential that the massage and electrical departments should be under one head, and, for the sake of efficiency it must be autonomous. The rules we have at Aldershot are now satisfactory, but were not always so. If a stiff joint is being treated in the electrical department, it is not fair for a surgeon to break it down without reference to the officer who is treating it by physio-therapeutic methods. Formerly this was done, and often with disastrous results. Now, when a case comes into the electrical and massage department, it is primarily under the officer in charge of that department, and nothing of a serious nature can be done to that case without the consent of such officer. This, of course, throws corresponding responsibility for the progress of the case upon the officer-in-charge.

The only other topic upon which I shall have time to touch in this discussion is "shell shock." In this category we may place all the psychical cases. In some instances the psychical trauma may have been received at home, the man not having been in the firing line, but they are all much the same in essence. In the case of genuine "shell shock" ordinary electrical stimulation will not do much good, but cerebral galvanization, after the method of Leduc, is of great value. The polarity which is used is very important. I always find that in patients suffering from nervous irritability the positive pole placed over the forehead has a most soothing and beneficial effect. The other pole may be applied to the back of the neck, or, better still, a bar is held in both hands, so that the current is evenly distributed through the head, neck and shoulders: it goes through the medulla and cervical sympathetic. In these cases the memory is often bad, mental concentration is defective, and there is severe headache and sometimes sleeplessness. In many cases all these symptoms will disappear in a few weeks under cerebral galvanism. A 5-ma. current, or less, is

sufficient for this purpose. One of its advantages is, that if anything goes wrong with the circuit there is no violent shock; whereas if you are using a greater strength the resulting shock may be dangerous. Moreover, a patient who has had this experience will shrink from electricity in future.

Some patients display electrophobia without apparent reason. It is of great advantage to have in the department someone who is familiar with psycho-therapy. I am fortunate in this respect in having Dr. Burnett Rae, who is an accomplished hypnotist. He is frequently able to induce a restive patient to accept electrical treatment quietly; and therapeutic suggestion is in itself helpful in "shell-shock."

Cases of psychic spasm frequently find their way to the electrical department. A man gets an injury to his foot which is not serious; but when he gets out of bed he turns his toes in and walks in an extraordinary manner. In this condition he is useless for duty. These cases are often put into plaster—I have seen them after they have been in plaster six months—yet when the limb is taken out of it the spasm returns almost immediately. If such a case can be thoroughly hypnotized the man will walk quite normally while in the hypnotic state. There is some relapse after the first sitting, but in about a week the man can be returned to duty in a sound state. I think it is wrong to do tenotomy in any case, of however long standing, unless psycho-therapeutic methods have been tried in vain. The electrical stimulation of the opponents of the affected muscles is important as an adjunct.

In some cases of injury about the foot, the effect is to cause not spasm, but paralysis, perhaps complete. There may be dropped foot. Yet if you apply faradism to the dorsiflexor muscles you find everything normal. It is legitimate to treat such a case for a month by purely electrical methods; and, if caught early enough, it will recover by that means alone. But if you have a man who has been treated for months, but has not been encouraged to help himself, he is difficult to cure in that way, or in any way. Further electrical treatment is likely to make him worse. One has seen cases which have attended various electrical departments for eight or ten months when the muscles are, physically, perfectly sound. These patients can sometimes—though not always—be cured by the hypnotist; but they may be extremely resistant even to psychic treatment. My point in these remarks is to emphasize the danger of letting these cases run on in electrical departments. They become the most hopeless class of invalids, and the very elaborateness of the unsuccessful treatment they have received gives them a fixed idea of incurability.

I will conclude my remarks on "shell-shock" by saying that many of these cases are, on careful examination, found to be suffering from a certain degree of hyperthyroidism, and this condition should be treated. The application of X-rays to the thyroid, combined with cerebral galvanism (and occasionally hypnotism), will often rapidly cure these cases, though it is seldom desirable to send the men again to the Front.

Dr. Turrell did not mention X-rays in his paper, but this form of treatment is very useful in allaying pain, especially in the case of nerves surrounded by scar tissue. At Aldershot it is one of the most used means we employ in our department.

Major F. Howard Humphris, R.A.M.C.

I propose to confine my remarks to the subject of static electricity—not that I undervalue nor neglect other forms of electro-therapy, but simply because this particular form has so few supporters.

Major Turrell has kindly said that I have had a much longer experience of static currents than he has, but I think any one whose department has administered 8,000 treatments may be fairly well taken as a competent authority. Twelve years ago I urged the claim of static electricity in an article in the *Lancet* (Feb. 6, 1904, p. 362), and I have seen nothing since then which can replace it in certain of its therapeutic uses. It seems superfluous to say this, but I am afraid that it is not altogether so: that when we use electricity as a therapeutic agent, it is necessary to have a clear idea not only of the result which we wish to obtain, but also of the manner in which that result is to be accomplished. To many people electricity is electricity, and if a muscle is to be made to contract, it matters very little which form of electricity is to be used to make it do so. But to the physician who chooses wisely, it is clear that differentiation must be exercised, thus: (*a*) If mechanical effects are to be produced, static electricity is in most cases the best agent for the purpose. (*b*) If electrolytic effects, the constant current. (*c*) If diathermic effects, the D'Arsonval current, and so forth. It is only by keeping these effects clearly in view that the appropriate current will be selected, and a high proportion of success be attained.

But to come to some concrete instances in which static electricity is particularly useful in the treatment of war injuries. The currents which are chiefly efficacious in provoking muscular contraction are: (1) The

Morton wave current; (2) the static induced current; and (3) the static spark. The main feature of all these currents is the fact that they produce muscular contraction and local vibratory effect. Thus, in addition to stimulating and exercising the muscles, they will diminish local congestion and stasis where such exist.

In the paralyses caused primarily by damage to nerve, in stiffness of joints which has been caused by injury or by enforced immobilization, I have no hesitation in saying that the effects of these currents cannot be approached by any other known electrical means. In synovitis, in which we have a congested synovial membrane with inflammatory exudation, a stasis, possibly a faulty metabolism set up in the cartilage, and, perhaps, with an exudation in the tissues, we possess in the wave current an agent the mechanical action of which will disperse that congestion in the membrane, and so improve the circulation that these products are readily absorbed. As Erichsen says of synovitis, "If the disease progresses favourably, these products are completely absorbed"; so, conversely, we may say the disease will progress favourably provided we can absorb the products or cause them to be absorbed. In recent fractures and sprains the same argument will apply.

Another current which can be derived from the static machine, and one of great importance, is that known as the Brush Discharge, and was described as far back as 1786 as "an electric fluid coming from a wooden point"; and this description holds good to-day. It differs from the *effleuve* from the Oudin coil. The static machine should be powerful enough to give a discharge between the electrode and the patient of at least 10 in. It is very useful in relieving local congestion such as occurs in indurated margins of wounds. It may be used to advantage in the early stages of inflammations such as we meet with in military hospitals. It is of value in most forms of trench foot, even those severe cases in which gangrene has already commenced. In "shell-shock," that is to say, where we have a neurasthenia induced by prolonged exposure to the dangers, the noise, and other inconveniences of modern artillery, this current, when played up and down the spinal column until its rubefacient action is obtained produces a feeling of well-being in the patient which, repeated from day to day, aids in recovery in the most remarkable manner.

I know I have but very lightly and very inadequately touched upon a much neglected, and very often ignorantly condemned, electrical remedy, and I ask, even as John Wesley asked many years ago of the

profession, "That none of them would condemn they know not what; that they would hear the cause before they pass sentence."

Dr. Noel Burke.

It is stated by many authors, and we have already heard once or twice to-night, that the treatment of injured nerves should be carried out by the galvanic or faradic current according as reaction of degeneration is or is not present. I have looked through my records for the past year and a quarter, and find that I have a series of over 300 cases of nerve injury. The various forms of treatment they have received have been: Rhythmically varied stimulation by the faradic current, with the use of the stroking method; similar treatments by the galvanic current, and by condenser discharges; rhythmically varied sinusoidal baths, with the use of Lewis Jones's interrupter; similarly interrupted baths with the use of the combined faradic and galvanic current; occasionally the constant current without interruption, largely for the purpose of ionization effects. On the whole, most of these cases seem to have had faradic or condenser stroking. Nearly all had reaction of degeneration; nearly all seemed to improve equally well.

Apart from the ideal treatment which one would wish to apply, purely practical reasons exist necessitating the choice of another method when one is working in military hospitals. We have to use the machines that are supplied to us, and if there is pressure of work we have to use the most suitable machine available. Again, if a man has an open wound in his forearm or leg, one cannot deal with the muscles directly, but must give a bath and so get the current in by way of the foot or hand. If a man has a stiff knee one cannot always get him into a bath, and we must then use the stroking method.

So far as I can see, there is not much to choose between all these methods. So long as he gets a suitable electric stimulus—that is, one that rhythmically varies—for as long as possible and as often as possible, every patient is going to improve. Professor Langley contributed an article to the *Lancet* in July last,[1] in which he described the results of experiments he had performed with a view to settling this point. He concluded that the sole object of treatment of muscles which were wasting after injury to their nerves, was to maintain their nutrition, and said, "On the whole it appears to me probable that it would be better to obtain these accessory results of contraction without the contraction itself." This agrees with what I have found in my cases,

[1] *Lancet*, 1916, ii, p. 6.

and it agrees with the experience of St. Bartholomew's Hospital, where nearly all of these cases are treated by the sinusoidal bath.

The pain in these cases is sometimes very severe; we know that the nerve is involved in scar and that an operation is necessary to free it, but there is still an open wound which prevents interference. It is well worth while to treat this condition, as is shown by a striking case recently under my care. This man was wounded laterally through the thigh on September 3. When I saw him on September 27 he had bad pain from the calf to the foot, it was getting worse, and he could only sleep with the help of drugs. He was given a constant current, from the buttock to the foot, for half an hour daily, and immediately had relief, first during the application, then for about fifteen minutes after. After twelve days he had at least half an hour's daily ease, and could sleep. At the end of a month operation was possible, and the condition found was the following: A complete band of tissue passed from wound to wound, and constricted the sciatic nerve in the middle. On one side there was dense scar tissue; on the other a band of muscle fibres was deflected from the biceps and adherent to the nerve. Despite this ever present source of fresh painful stimuli, the treatment was effectual in supplying certain and regular relief.

I also have lately seen a case of rise of temperature after ionization of a wound. This was a sinus below the knee, and I think there was some dead bone at the bottom. I used salicylate, and there seemed to be some improvement, when the patient suddenly ceased to attend, and I heard he was in bed with a high temperature. I am afraid I can give no details of his illness, as I had nothing to do with his ward; the next I learned was that he had gone to a convalescent hospital. In Major Turrell's two cases and in mine the negative pole was being used, and it seems just possible that the cases can be explained in the light of Dr. Russ's theory of the movement of bacteria in a constant current. According to this theory, when we used the negative pole we drove bacteria inwards to the tissues, and this may have caused the temperature to rise. Dr. Russ would have used the positive pole, and have drawn the organisms out.

The organization of electrical work, particularly in military hospitals, is a very important point. One sees cases which have been through various auxiliary hospitals and have had electrical treatment, generally given by "one of the nurses," not even in many cases one who had learned anything about the work, simply the nurse who happened to be on duty. This deplorable state of affairs is largely due to the medical officers, who order that the patient is to have electricity and do not

prescribe the form or dose. I have heard of a case in which a patient was sent for ionization with the instruction—"but on no account use the galvanic current!"

Dr. E. P. Cumberbatch.

In the discussions which have recently been held, and the papers which have been written, on the treatment of injuries and disabilities produced in the War, electricity has received very scanty attention. Electrical currents, such as the sinusoidal and the Bergonié, have been mentioned and no records have been given of results following their use. Until to-night, we have heard of no results obtained in the treatment of such conditions as wounds, stiff joints, sinuses, paralyses, &c., following treatment by electrical methods. Major Turrell is to be congratulated upon the excellent results which he has obtained in the Radcliffe Infirmary. He is fortunate in having an elaborate department, fitted with all the modern apparatus, and he is still more fortunate in being able to select a large number of his own cases for treatment.

Some electrical apparatus is costly. A static apparatus will cost £200, a diathermic apparatus £40 to £80; in fact the cost of a static machine will be as great as that of a complete X-ray installation, and it is not likely that the powers-that-be will now allow the installation of such expensive apparatus for the treatment of injuries unless they can be assured that it is as essential for treatment as are X-rays for diagnosis.

During the past seven weeks I have been working in the electrical department of the First London General Hospital, and throughout that time we have had some 100 cases, and 1,500 treatments have been given. We have only two currents, the direct and the sinusoidal, yet in spite of that we have been able to obtain very good results.

I will make brief remarks upon two points. First, with regard to the method of administering these currents. We have three rooms in all—one is used as a waiting room. In the middle room the sinusoidal current is used. It is supplied by twelve plugs and twelve rheostats. The third room has twelve plugs and twelve rheostats for *direct* current. We use simple water resistance, and these work incomparably better than the more expensive wire and lamp resistance, and the cost is about one-eighth. With regard to results, I do not wish to mention anything more—at present—than the treatment of sinuses and wounds. When we want to treat wounds by ionization, we immediately think of zinc. In some of my cases I commenced by using zinc, but now I use the salicylic ion which I find more efficacious

and less painful. I should like to mention the case of an officer who had a gunshot wound of his leg: it measured 3 in. by 1 in., it had been in existence two months; it had neither increased nor decreased, but remained in an indolent condition. The first treatment by zinc ionization caused considerable pain, and had no effect on the wound. He was then treated by salicylic ions, which caused very little pain. With the second application a rich rosy crop of granulations sprang up, and in a short time the ulcer filled up and the skin had begun to grow from the edges. After three weeks' treatment the ulcer has now diminished to the size of a threepenny-piece. I have obtained as good results in the cases of other septic wounds and indolent ulcers.

In many cases of sinus we have a very difficult task. I have tried a number of methods and the one I am using at present, which seems to have yielded good results, is ionization with salicylic ions, applied in the way I am about to describe. I first syringe the wound clear of pus with sodium salicylate, 5 per cent., then get the wound plugged as much as possible by gauze which has been soaked in the same solution, immediately bring the patient to the electrical department and drive the current into the sinus and walls by way of a small portion of the plug which has been pulled outside. This plugging makes good contact with the interior of the sinus. Where sinuses are 1 in. or 2 in. deep, if they are not complicated by the presence of dead bone at the bottom, I have obtained good results in the way described.

Dr. Donald Baynes.

When reference was made to the relief of pain by means of the constant current, I noticed that it was not stated which pole was used. I suppose it was the positive pole, because owing to oxygen collecting round the positive pole the tissues adjacent to the positive pole are rendered acid, this resulting in a mitigation of pain and a sedative action; whereas the negative pole attracts hydrogen, which is an alkali maker, and sets up over-stimulation and irritation and will therefore increase pain. For ionization we should use the positive pole, with thyocyamine to dissolve and destroy scar tissue; and I find that putting the patient in a strong magnetic field has a wonderfully good and soothing effect in shock.

Dr. G. B. Batten.

I should like to emphasize a point which has not been touched upon, except indirectly. Dr. Hernaman-Johnson mentioned the danger we run, in many cases, of going on too long with electrical methods. I

would emphasize the point that whatever method we use, whether physical methods, such as compression by cotton wool for sprains, or electrical methods so as to make the muscles work—the Morton wave current, the reversed galvanic current, or the faradic current; all these should aim at enabling the patient to use active movement, not so much passive movement, because the active is so much the more valuable. If you can encourage a patient to use his own muscles, progress will be much faster than if you prolong purely electrical methods.

Dr. ETTIE SAYER.

I, too, should like to add an expression of my appreciation of Dr. Turrell's remarks, every word of which is confirmed from my own experience, except, perhaps, his observations on shock. I have always found the static machine extremely useful in every case of shock. In chronic cases of shock, I think its usefulness can be demonstrated by the rise of blood-pressure, as well as by the patient's enjoyment of the treatment: I have not experienced any exception to that. It seems specially acceptable to patients with wounds. One of the most tragic things in the whole War is that there is not more static treatment being applied, especially at very early stages; for its prevention of the formation of scar-tissue immensely diminishes the need for dissolving such tissue later on. My results have been much better in civil practice, because I get wound cases early. I have had two ladies who had met with serious face accidents, one seven years ago, the other three or four years ago. Each patient had been thrown out of a motor-car on to her face. Both had black eyes, and one had two fractures, and the face of each was severely contused. I used the static machine straight away, and there was so much satisfaction that it was asked for again and again at two or three hours' interval: indeed, one lady would not leave the machine-room, and I had to allow her to stay all night! I find the best way of treating the ionization cases is to give static treatments on the intervening days.

Major TURRELL (in reply).

Dr. Hernaman-Johnson dealt largely with the orthopædic treatment of the wounded; at the Third Southern General Hospital, the methods of Mr. Robert Jones, which Dr. Hernaman-Johnson advocates, are largely followed.

With regard to cerebral galvanism, we have not been able to carry out this treatment as I should like to have done. This is not a form of treatment which ought to be left to nurses, and with sixty surgical

beds to look after and seventy electrical treatments to supervise daily, there is not much time available to administer treatments personally. I have been much interested in Dr. Hernaman-Johnson's remarks on this subject and also in an article on cerebral galvanism which recently appeared in the *British Medical Journal*. I hope shortly to have an opportunity of giving it a trial in certain cases of shell shock.

The electrophobia which I have encountered in my work was of a very definite character in two cases. One was that of a patient who had been engaged in some large electrical works, the other patient had been in civil life a driver of an electric tram. I can understand that the somewhat formidable appearance of a powerful static machine is calculated to inspire with awe a nerve shocked patient who has previously received shocks from an electric trolley system.

I did not refer to radio-therapy, because at my hospital this belongs to another department. I am afraid that radiologists in general have been too busy during this War taking radiographs and designing numerous and ingenious localizing methods to pay much attention to radio-therapy.

With reference to the choice of method in the treatment of nerve injuries, I adhere to my opinion that the method which most efficiently excites the muscle to perform its function, that is, to contract, is the method of choice. The Morton wave current, moreover, demonstrates the efficiency of muscular contraction in promoting tissue drainage and the elimination of waste products.

I do not think that the temperature in my two cases of ionization is explained by the theory that it was due to the electrical transportation of bacteria into the tissues, because a blood culture proved negative after the lapse of several days. There was practically no constitutional disturbance in these cases, and I am inclined to regard the pyrexia as analogous to that which occurs occasionally in cases of large superficial burns as the result of the absorption of the products of tissue destruction. As I said in my paper, these cases only occurred in connexion with large superficial wounds, and not in connexion with the treatment of deep sinuses, however septic they might be.

I am much interested in Dr. Cumberbatch's ingenious method of securing the thorough treatment of deep sinuses, and hope to adopt his method of treatment by the projecting plug. In the treatment of superficial wounds, benefit is usually obtained by changing from ionization to ultra-violet radiation after the lapse of a few days. The same thing is seen in the surgical dressing of wounds, great improvement ensuing after a change of lotion or ointment.

Section of Electro-Therapeutics.

President—Dr. G. Harrison Orton.

(*December* 15, 1916.)

Pharyngeal Pouches.

By N. S. Finzi, M.B., Captain R.A.M.C.

Pharyngeal pouches, sometimes erroneously called œsophageal pouches, are diverticula or herniæ of the posterior and postero-lateral wall of the pharynx, behind the cricoid, into the loose areolar tissues behind the pharynx and œsophagus. Fig. 1 shows how the oblique and fundiform parts of the crico-pharyngeal muscle (inferior constrictor), which normally are quite close together, are in these cases separated, possibly congenitally, thus leaving the pharynx with no supporting muscular wall. A diverticulum forms which will tend to become larger and larger, its contents exerting an always greater pressure as they increase in amount. A general dilatation of the pharynx is also known. Fig. 2 shows the diverticulum in the early stage and fig. 3 in a later stage. Three of these slides are drawings from specimens at the museum of the Royal College of Surgeons and the four were kindly lent me by Dr. William Hill.

I am concerned this evening only with the *radiographic* examination, appearances and diagnosis of these cases.

The method used is the administration of a thick paste of bismuth oxychloride and water of such a consistency that, when heaped up, it has practically no tendency to flow back to its level. A bismuth meal,

bismuth and sugar, or barium sulphate and water are not so good, the first because it is not sufficiently opaque, and all of them because they do not stick to the œsophagus and outline it after the main mass of them has passed. The bismuth paste, however, has the property of giving quite a good outline of the œsophagus for a variable time after a bolus of it has been swallowed. The fluorescent screen examination of these cases is most important, as they may be very difficult to distinguish from stricture unless it is seen how the bismuth leaves the pouch. A pouch can be observed to fill and, when full, the bismuth passes into the œsophagus, going past the upper end of the pouch with ease but being held up lower down in those cases in which the pouch is

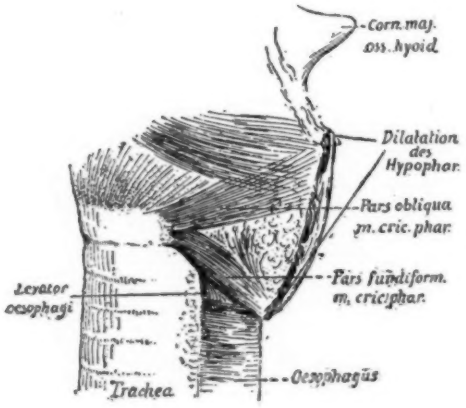


FIG. 1.

Dissection of the muscles of the pharynx (diagrammatic.)

big enough to press on the œsophagus (fig. 10): in some cases the pouch can be observed to empty into the œsophagus when the patient contracts the neck muscles. On the other hand in stricture the bismuth passes, if the stricture is not complete, from the lower end of the dilated portion of the gullet.

A plate examination always follows the screen examination and, if an intensifying screen is used, an instantaneous exposure may be given but this is not so important as it is in the lower part of the œsophagus, for if the patient refrains from breathing or swallowing there is no

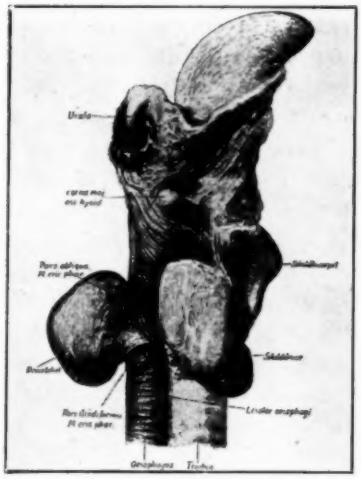


FIG. 2.

Pharyngeal diverticulum commencing to form.



FIG. 3.

Pharyngeal diverticulum in a later stage.

movement of this part of the gullet such as the heart and aorta cause in the lower part.

I find that when the pouch does not extend into the thorax a true lateral view gives a far better picture than the oblique antero-lateral view, though the latter is always useful. The plate may be placed close to the neck, with a cushion to keep it in place, but though this gives the clearest definition the lower end of a large pouch may be missed; but in some cases the whole pouch with the adjacent structures may be raised, and kept raised for a few seconds, by a tractable patient if he will keep them in the position they reach at the commencement of the act of swallowing. In many cases, and always when an intensifying screen in a casette is used, the plate will have to be placed some distance from the neck owing to the shoulder getting in the way; so that it is essential to have the X-ray tube a long way off in order to get parallel rays and avoid distortion. A posterior or an anterior view must also be taken, the former usually giving the clearer picture. I always examine the patients standing up.

I now show you a picture of the pharynx and upper part of the œsophagus, not quite normal it is true, for there was a polypus causing a slight partial obstruction, but it gives a general idea of the appearances in a normal case. Fig. 4 shows the appearance in the same position, right antero-lateral oblique, of a pharyngeal pouch, and fig. 5 of a typical carcinoma.

In the pouch a dark shadow is seen with a rounded lower border, and the bismuth which has already passed down the œsophagus has outlined it, showing that it is obviously unconnected with the lower end of the pouch shadow. In the carcinoma case the lower end of the shadow of the dilated part of the œsophagus is conical, with irregular projections into the gullet below the cone, these latter being fungations of the growth. The bismuth is obviously leaving the dilated part at its lower end.

Differential Diagnosis. — What a pouch generally has to be distinguished from is a malignant stricture, but a fibrous stricture may also simulate a pouch: I do not believe a spasmodic stricture will do so, as it is unlikely to occur in this position. The cardinal difference between a pouch and any form of stricture, however, is the fact that the former must empty from its upper and the latter from its lower end, and this can generally be made out by a careful screen examination on the lines I have indicated. A carcinomatous stricture is usually conical



FIG. 4.
Pharyngeal pouch: right antero-lateral oblique view.



FIG. 5.
Carcinoma of œsophagus: right antero-lateral oblique view.

FIG. 7.
Carcinoma simulating a pouch: right lateral view.

FIG. 6.
Typical pharyngeal pouch of medium size: left lateral view.

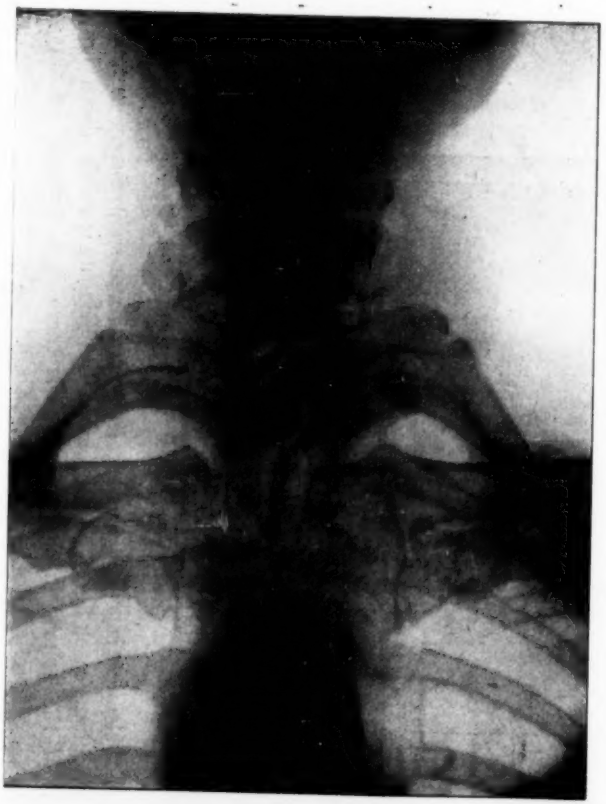


FIG. 8.
Pharyngeal pouch : posterior view.



FIG. 9.
Carcinoma of œsophagus, simulating pharyngeal pouch : posterior view.

in shape and has not the bulbous appearance seen in the case of a pouch. In the radiogram a carcinoma may, however, simulate a pouch, as shown by figs. 7 and 9. There are two points in the lateral view in which this case differs from the pouch. In the first place a little



FIG. 10.

Large pharyngeal pouch: left lateral view. The lower end of the pouch is pressing on and obstructing the œsophagus. The thin streak in front of the main shadow is the bismuth passing into the œsophagus.

point of bismuth is seen projecting from the lower end, whereas in the diverticulum case, fig. 6, the bismuth is obviously passing down in front of the pouch; in the second place there is a "waist" to the

bismuth shadow in the carcinoma case, about 1 in. above its lower end: neither of these by itself is absolute proof, though the former is most suggestive. In the posterior view (fig. 9) the real nature of the case betrays itself, for it is now seen that, on one side, the end of the shadow

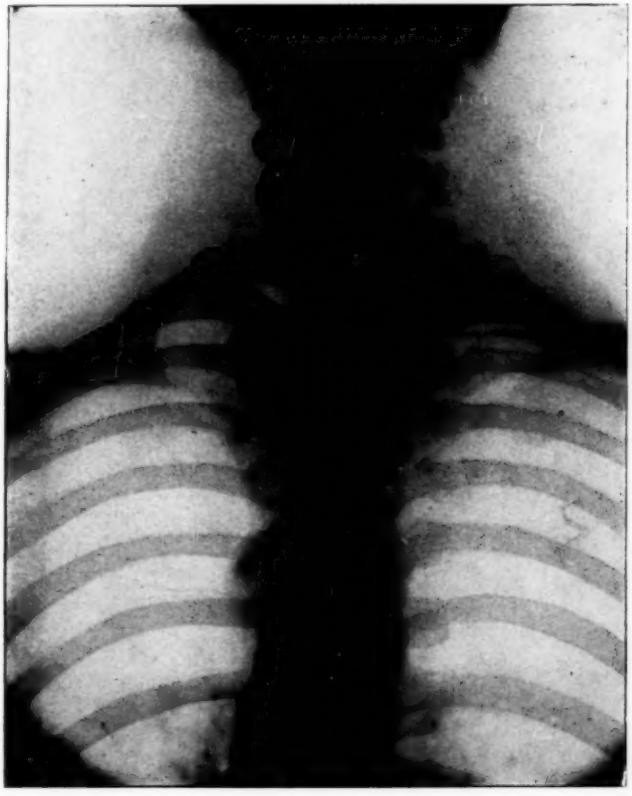


FIG. 11.

Large pharyngeal pouch: posterior view.

is irregular and does not extend so far down as on the other, while evidence is present of fungations both in the main shadow and below it. In the skiagrams of the pouch the lower end may be hitched up in the

middle (fig. 8), but it is quite smooth and rounded and there is no trace of fungations. The correctness of the diagnosis in both these cases was subsequently proved.



FIG. 12.

Small pharyngeal pouch: right antero-lateral oblique view.

A fibrous stricture may be even more difficult to distinguish though it is fortunately rare in this situation. Here one is not helped by the presence of fungations, though the stricture is likely to give a conical

lower end to the bismuth shadow. A screen examination will, however, distinguish.

If the pouch were so large as completely to obstruct the œsophagus, either directly or as the result of inflammation or adhesions, the diagnosis might be extremely difficult. Whether such a case can occur I do not know. One of my cases (fig. 10) was very near it and he had in fact had a gastrostomy for three years before his pouch was diagnosed and removed. In the lateral view of this case the bismuth is seen to be filling not only the pouch but also the pharynx and is seen to be escaping into the œsophagus, in front of the pouch. There is considerable obstruction of the œsophagus by the pouch and this had in fact led to a diagnosis of malignant disease at a previous X-ray examination.

Fig. 11 shows the posterior view of a very large pouch, and fig. 12 the right antero-lateral oblique view of a very small one.

These cases are supposed to be very rare, but all these eight cases have occurred in my private practice during four and a half years and I have also examined hospital cases during the same period. Their correct diagnosis is of the utmost importance owing to the radical difference between their treatment and that of the conditions which they simulate. I, therefore, trust that this paper may be of some use in pointing out some of their characteristics.

DISCUSSION.

Dr. DUNDAS GRANT: I did not expect to take part as a formal contributor to the proceedings of this evening, but I am very pleased to be permitted, as a casual visitor, to communicate my experience. In the first place I must express my admiration for the clear demonstration that Captain Finzi has given us and for his exposition of the anatomical points with regard to the weak spot in the pharyngeal wall just above the lower fibres of the inferior constrictor, for which we are indebted to Professor Killian. It was from the late Sir Henry Butlin that I first learned to look upon "the return of particles of undigested food many hours or even days after they had been swallowed," as the one constant symptom in the diagnosis.[1] The first case with which I had to deal was that of an elderly woman who complained of difficulty in swallowing, and return of food. I observed with the laryngoscope an extraordinary welling up of frothy mucus in the right hyoid fossa, which was increased by pressure on a soft swelling on the outer side of the neck just below the larynx. Sir James Mackenzie Davidson made a radiographic examination, and we saw the bismuth accumulate in the pouch: the skiagram was a perfectly lateral one and resembled very closely one of the smaller pouches shown us by Captain Finzi. In this case I removed the sac and complete recovery resulted. A subsequent case was that of an elderly man who presented similar symptoms, in which an antero-posterior skiagram showed a typical rounded shadow. In this case, owing to circumstances which were partially preventable, the patient died some days after the operation. An experience which I consider very instructive, was derived from a case at the Brompton Hospital—that of an elderly woman who for a considerable time had a difficulty in swallowing and brought up food containing particles which she had endeavoured to swallow some hours or even a day previously. Without further examination I referred her to Dr. Stanley Melville in the X-ray Department, anticipating that we should find evidence of a pharyngeal pouch; such, however, was not present, and a bismuth cartridge stuck in the lowest part of the pharynx: in this case there was not a pouch, but simply a tendency to pouching of the laryngo-pharynx. By means of an œsophageal bougie I made out a spasmodic contraction of the upper orifice of the œsophagus, and I repeated the passage on several occasions at intervals of a week. The symptoms then disappeared entirely and the conclusion at which I arrived was that this was a case of commencing pharyngeal pouch, the formation of which had been forestalled by the overcoming of the spasm of the mouth of the œsophagus. This is probably an element in the production and development of these pouches, the other factor being probably an unusual development of the weak spot in the pharyngeal wall. I may repeat what I have said elsewhere that we are sometimes twitted with

[1] *Proc. Laryng. Soc. Lond.*, 1897-98, v, p. 29.

depending too much on the resources of the clinical laboratory for the diagnosis of our cases, but we cannot do without the radiographer. Many clever persons have tried to do so and have regretted it.

Dr. STANLEY MELVILLE: I am exhibiting prints of a case of a large pharyngeal pouch which I have lately examined with Dr. Dundas Grant. Through the courtesy of Dr. Salusbury Trevor, of St. George's Hospital, I am able to show several pathological specimens illustrating this condition. One of these specimens shows a small but well-defined pouch; the patient came to the hospital under the care of Mr. Ewart, with a history of constant choking and difficulty in swallowing of some months' duration. In this, as in Dr. Dundas Grant's case, a thick emulsion of bismuth outlined a characteristic pouch, with well-defined, rounded base, and further quantities of the emulsion were seen to fill the œsophagus from the proximal end of the pouch and to pass along the tube without difficulty. I should like to emphasize the necessity for making a most careful and prolonged screen examination in these cases before deciding upon the condition. In several cases which I have examined and which have been proved to be caused by tight annular strictures, the dilated œsophagus has a well defined rounded base, and it was only after some moments that the characteristic funnel-shaped appearance of this condition showed itself. In one of the other specimens which I exhibit, the pouch itself, on account of its large size, actually caused obstruction to the further passage of food, and this is a point to be borne in mind. I warmly congratulate Captain Finzi upon his paper, which is most instructive. The number of cases which Captain Finzi has shown tend to confirm the opinion I myself have formed, namely, that pharyngeal pouches are less uncommon than they are usually considered to be.

Dr. IRWIN MOORE: I also should like to congratulate Captain Finzi on his very able demonstration. I am especially interested in these cases of pouches shown to-day, in view of the fact that amongst the series of eight, two of them are my private patients. In both these cases the question of diagnosis was very important, and it is a great triumph that confirmatory evidence can be obtained by X-rays. In the first case, a lady aged 64, it was suggested that she was suffering from malignant disease of the œsophagus, and I was consulted for the purpose of confirming or refuting this opinion. Under cocaine anæsthesia an examination was first made with the œsophagoscope before the X-rays were employed. As the endoscopic tube passed down the œsophagus under direct vision it entered a cavity the walls of which were covered by a slimy caseated material, giving off a faint sickly odour. Since no outlet could be found at the bottom of this cavity I came to the conclusion that the tube had entered a diverticulum or pouch. This was confirmed next day by Dr. Finzi with an X-ray examination, accompanied by a bismuth meal, which demonstrated not only the size but the position of the

pouch. It was possible, therefore, to give a definite opinion that there was no malignant disease present, which was a great satisfaction to the patient and her friends. In the second case no œsophagoscopic examination was made at the advice and expressed wish of the physician. The condition, which was first suggested by the clinical symptoms, was diagnosed by the X-ray and bismuth. Both these cases were in early stages, judging from the history, the size of the pouches, and the symptoms complained of, which were not of a very distressing nature. In neither case was any operation considered necessary. The patients were advised to eat and chew their food slowly, to avoid everything indigestible, whilst the first patient was to have the pouch washed out occasionally. She, however, refused, and she appears to have got on very well for over six years without this being necessary. It must not be forgotten that, owing to the great advance made in the method of examining the œsophagus by direct vision, these diverticula or pouches may be diagnosed without the aid of the X-ray. The valuable assistance however of X-rays as confirmatory evidence in ascertaining the size, position, and progress of the pouch, must not be overlooked and they should be employed in every case in combination with the œsophagoscope.

Section of Electro-Therapeutics.

President—Dr. G. Harrison Orton.

(*December* 15, 1916.)

The Changes observed in Cases of Osteomyelitis.

By R. W. A. Salmond, M.D., Ch.M.

The following are notes of three essentially different cases of osteomyelitis in a long bone, observed by repeated X-ray examinations and with their main clinical facts.

Case I: Infective Osteomyelitis of Right Radius of Staphylococcic Origin.—L. B., aged 9. Patient came to out-patient department with pain in right forearm, with the history that she was knocked down and fell with her arm under her in the school play-ground on the previous afternoon and had complained of pain in it the same evening. Her mother is quite definite that when she went to school that day her forearm was quite right. A radiograph (fig. 1), taken on this date, showed practically no change in the bone except, perhaps, a very small area of slight rarefaction at the outer side of the radius at its lower epiphyseal line. The next day, she was admitted to hospital under the care of Mr. Sidney J. Wareham with a temperature of 100° F., and the forearm was treated with fomentations. The following day, the forearm showed a condition of general cellulitis; under an anæsthetic, three long incisions were made down to the bone and pus evacuated. Eight days after this, the temperature had dropped to normal and remained so until the relapse, three and a half years later. One month from the onset, another examination (fig. 2) was made, and the lower end of radius seen to be largely disorganized. Here necrosis was seen alongside partially destroyed bone. There was a sharp line of differentiation between the affected and the unaffected portions of the shaft. New bone was being deposited by the separated periosteum and could be seen along the whole length of inner side of radius. The upper part was also affected, but to a less degree. Beneath the separated periosteum, the cortex of shaft was seen first to become eroded and made irregular and ultimately to

disappear. One month later, two months from the onset (fig. 3), an examination was made, and showed an extension of all the appearances of the previous month. The newly-deposited periosteal bone formation had increased in width and density and had become more regular in outline. Its width on either side of the shaft equalled that of the original shaft, the whole of which was now



FIG. 1.



FIG. 2.

surrounded, except at the middle third of outer side. Abscess cavities were seen at both upper and lower ends of the bone. On this date, an incision on the outer aspect again struck pus. One month later, three months after the onset (fig. 4), the original shaft had now been almost entirely absorbed. The

new bone formation had made a new wavy outlined and widened shaft, with slight traces of the original shaft to be seen at the upper end. There was an abscess cavity at the lower end involving also the epiphysis and containing a sequestrum. The carpal bones were not affected nor was the upper epiphysis. A drainage tube about the middle of the shaft pointed towards a sequestrum.



FIG. 3. FIG. 4.

Slight periostitis of the posterior aspect of the ulna was observed in the lateral view. Next month, four months after onset (fig. 5), the radiographic appearance was seen to be rather more quiescent. The width of new shaft was slightly less, and a piece of the shaft had been removed in an intervening

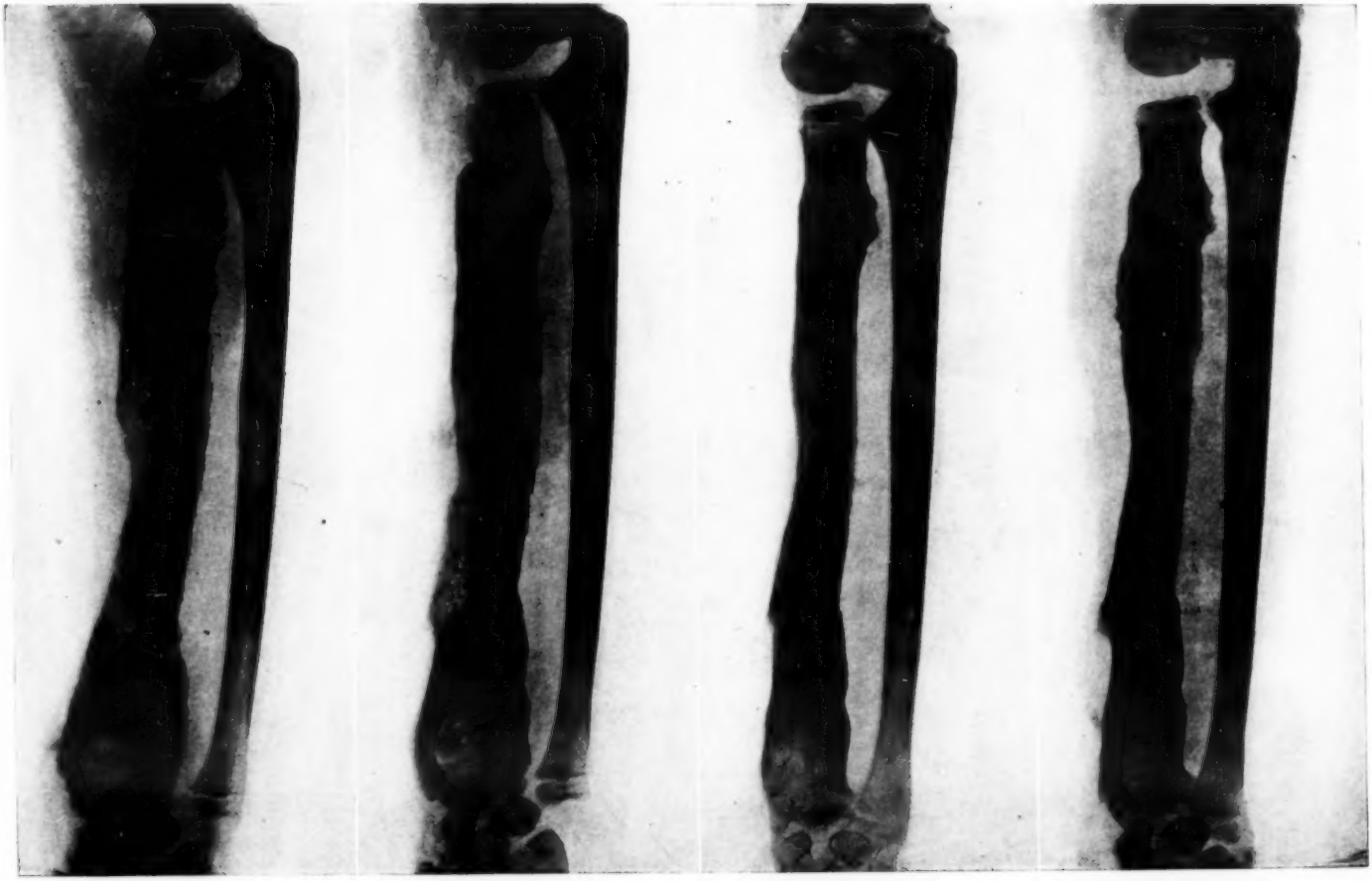


FIG. 5. FIG. 6. FIG. 7. FIG. 8.

operation. At least three sequestra were to be seen: one in the abscess cavity at the upper end, another in the middle of the shaft, and a third in process of extrusion from the abscess cavity at the lower end. The upper epiphysis remained unaffected. The following month, five months after the onset (fig. 6), examination showed that the sequestrum seen the month previously in the upper abscess cavity had been discharged. Others were still present. On the whole, there has not been much change from the plate just shown. A note made shortly after this examination, says that three small sinuses were still present



FIG. 9.



FIG. 10.

over lower half of the radius, with the remains of a healed sinus near the elbow-joint. The patient was now sent to a convalescent home at Reigate. She remained there for three months and then returned with a healed forearm, and an examination, nine months after the onset (fig. 7), showed considerable changes. The new shaft looked much more solid, its width was nearer the normal, its trabeculæ had a more normal appearance and its outline was more regular. The upper abscess cavity had been largely filled in but not so the lower. The upper epiphysis had remained unaffected. Two months after this, eleven

months after the onset (fig. 8), new periosteal bone deposit was found in the upper third of the shaft, with an area of rarefaction in its interior at this position. This was interpreted as a slight recrudescence of the disease, and if so it is interesting, as it caused no subjective symptoms, and it is

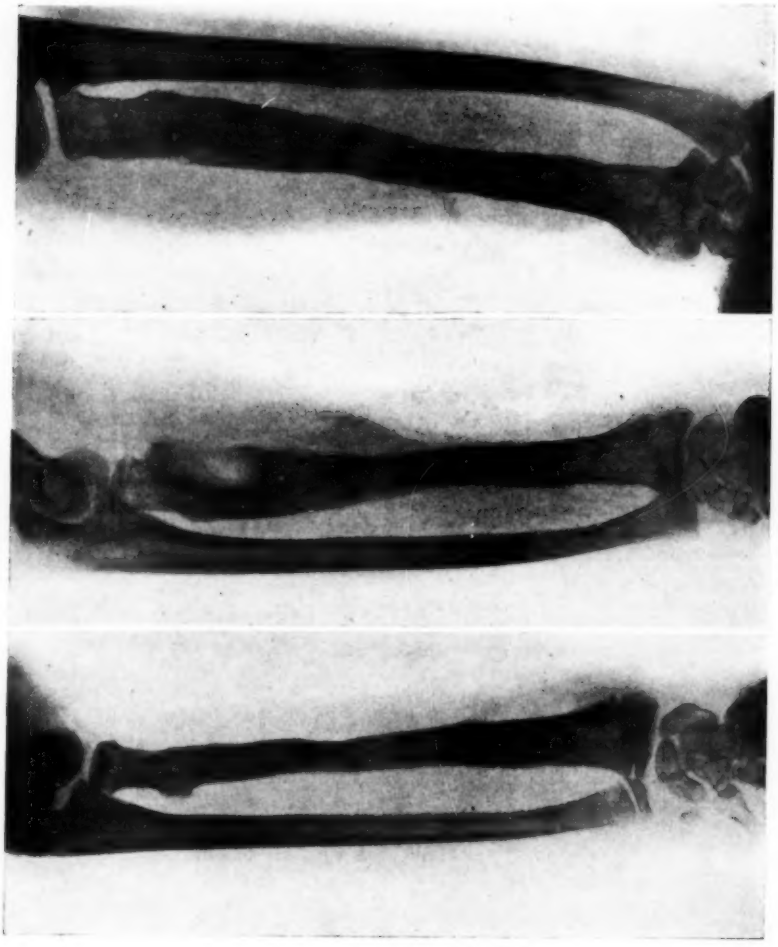


FIG. 13.

FIG. 12.

FIG. 11.

striking that some two years later a definite recurrence with abscess formation took place at this same position. Three months after the last examination, fourteen months after the onset (fig. 9), the recrudescence had quietened down without any sequestrum having been formed, but the area of rarefaction still

remained. The shaft was nearer the normal in width and the cavity at the lower end remained unobliterated. Six months later, twenty months after the onset, an examination showed the lower cavity to be rather less but still unobliterated. The upper rarefied area had gone. Three months later, twenty-three months after the onset (fig. 10), the cavity was still unobliterated. During the next ten months I unfortunately lost touch with the case as the War had begun and duties kept me away from London; but at the end of this time an abscess had developed at the wrist, and on examination, thirty-three months after the start (fig. 11), the lower cavity was found to be still unobliterated, with distinct swelling of the soft parts over it corresponding to the pointing of the abscess. Eight months afterwards, and while I was away again, the patient was readmitted to hospital, now three and a half years from the time of onset, with a temperature of 102° F. Part of the shaft of the radius and some sequestra were removed by operation, and at the end of four months she left with a forearm healed again. Four months afterwards, forty-nine months after the onset, an examination (fig. 12) showed that the lower cavity had at last become obliterated, but that there had been a relapse at the upper third of the diaphysis where this was widened and where the remains of an abscess cavity could be seen. A new shaft is in process of formation here. This corresponds to the seat of operation just mentioned and to the place where a slight recrudescence was observed two years before. The upper epiphysis is not affected. Periostitis was observed on the posterior surface of the ulna in a lateral view. Two months later, fifty-one months from time of onset, the remains of this new abscess cavity were seen to be smaller and to be filling up. Now, three months afterwards, four and a half years after the onset (fig. 13), the cavity is almost obliterated, and the newly-formed shaft again appears more normal. The lower epiphyseal cartilage having been involved has apparently lost some of its bone-forming functions, for the ulna is seen to be growing faster than the radius and pushing the hand into a position of slight abduction. Clinically, however, the patient has a very useful forearm with movements in every direction. This brings the radiographic observations of this case up to the present date, and as there seems the likelihood of further developments, it is hoped to continue them further.

The chief points of interest in this case are the following :—

(1) Observations were begun within twenty-four hours of the definite exciting factor.

(2) The clinical crisis taking place before hardly any radiographic changes were to be seen.

(3) The escape of the upper epiphysis, adjacent joints and, except for an occasional slight periostitis, the ulna.

(4) The formation of a complete new shaft in three months, the old shaft meantime having been absorbed, detached as sequestra or removed at operation.

(5) The occurrence of two relapses, one not causing any symptoms and discovered quite unexpectedly, the other taking place some two years later at the same position and much more acute in nature.

(6) The non-obliteration of one of the abscess cavities in the bone for three years. This seems due to the remains of some focus of disease, as the subsequent extrusion of a sequestrum or pus, or both, led to its disappearance.

X-ray examinations, as just illustrated, present a pathological record of the course of destruction and reconstruction of the bone in such a disease unobtainable in any other way.

Case II: Syphilitic Osteomyelitis of Right Radius.—N. A., aged 13. June 6: Patient admitted to hospital under Mr. Wareham with the history of a swelling on the outside of the right knee for the last three years, and with other smaller swellings on the right forearm and cheek of more recent date. On examination the lower end of the femur was found to be practically fractured and in bad position. There were also gummata to be felt on the right forearm and cheek. Definite signs of syphilis were present in the teeth, eyes and frontal region. The temperature was subnormal. An X-ray examination (fig. 14) showed an osteitis and periostitis of the upper two-thirds of the shaft of the radius with disappearance of the cortex. The upper epiphysis was also affected. Treatment: Pot. iodide 30 gr. and pil. hydrarg. 8 gr. *per diem.* June 14: X-ray examination showed no change. June 18: The gummatous deposits were smaller and much less inflamed. June 28: An X-ray examination, three weeks after antisyphilitic treatment was commenced, showed very little if any improvement. Pot. iodide was now increased to 45 gr. *per diem.* July 12: Iodide of pot. was being well tolerated and the gummatous deposits in forearm and cheek were slowly diminishing in size. An X-ray examination after five weeks' treatment (fig. 15) showed a slight improvement. July 31: Discharged as out-patient and directed to continue mercury and iodide treatment. August 16: Another X-ray examination after ten weeks' treatment (fig. 16), showed more definite improvement, the periosteal thickening having been largely absorbed. November 27: A further X-ray examination, after nearly six months' treatment (fig. 17), showed a still greater improvement. The periosteal thickening had almost entirely been absorbed while the trabeculæ had a nearly normal appearance and the cortex had been reformed. Clinically, no trace of any gumma could be felt in the forearm which seemed quite normal.

This was a case of bone infection, undoubtedly syphilitic in nature, and while it did not present the appearance from the beginning as in the former case, it showed the effects of mercury and iodide on one of the affected long bones. Under this treatment, the bone showed little improvement radiographically until after about two months, when the condition gradually returned to the normal. As in the previous case,



FIG. 14. FIG. 15. FIG. 16. FIG. 17.

the adjacent joints were not involved and the clinical improvement took place long before the radiographic.

Case III: Tuberculous Osteomyelitis of Lower End of Left Femur.—F. K., boy, aged 10. This patient was admitted also under Mr. Wareham for increasing swelling of the left knee and with the history of three weeks' pain. On examination, distinct fluctuation and patellar tapping were elicited. Temperature was normal and remained so throughout. An X-ray examination, taken on day of admission, showed an abscess cavity at the lower end of the femur involving both diaphysis and epiphysis. It was surrounded by an area of condensed bone and contained a sequestrum in its lower part. The outlines of the articular ends of the bones of the joint did not appear to be altered. There was little rarefaction, but it should be added that the plate was taken with a very soft tube. Treatment: Aspiration, immobilization and Scott's dressing, and at the end of two months the patient was sent home in a plaster of Paris knee-jacket. Four months later (six months after the first examination) the patient reported for observation. A Thomas's splint was applied. The X-ray examination showed that the abscess cavity had increased in size together with the appearance of a "fuzziness" and rarefaction of the bones. The articular ends of the bones, especially the femur, now showed irregular outlines. The joint appeared distended and the surrounding soft parts distinctly infiltrated. Six months later, another examination showed further rarefaction of the bones and more irregularity of their ends. Shortly after this, the joint was again aspirated, pus found and iodoform emulsion injected and the following month the patient was sent to his home in the country wearing a Thomas's splint. The subsequent history is that the boy has now been wearing this splint for the last two years, has a healed knee and quite a fair amount of movement at this joint. A note from the patient's doctor says that he anticipates complete normal use of the leg after the splint has been removed.

This case shows clearly defined differences as compared with the other two cases: (*a*) The ready spread of the disease to the adjacent joint, the epiphyseal and articular cartilages being no barrier, while in the infective case, the epiphyseal cartilage at one end, and the articular cartilage at the other, strictly defined its limits; (*b*) the extensive infiltration of the surrounding soft parts. The rarefaction typical of tubercular disease cannot be taken much note of as the immobilization of the limb could account for most of it.

The value of systematic and repeated X-ray examinations, in recording the course of changes taking place in these cases of osteomyelitis, are of sufficient interest to justify their publication; and, in conclusion, I wish to thank Mr. Wareham for the clinical notes of all three cases and for his ever-ready co-operation in these observations.

Section of Electro-Therapeutics.

President—Dr. G. Harrison Orton.

(*January* 19, 1917.)

The Electrical Department of St. Bartholomew's Hospital.

By E. P. Cumberbatch, M.B.

As a subject for this evening's meeting of the Section at St. Bartholomew's Hospital, I thought that a short account of the Electrical Department, its history, the work done, the types of cases sent there for treatment, and the treatment given, might be suitable.

The Electrical Department at this hospital was founded in 1882, and Dr. Steavenson was appointed medical officer in charge. Before the year mentioned some electrical treatment, such as faradism and galvanism, had been carried out in the wards by house-surgeons and dressers. The department founded in 1882 was close by the King Henry VIII gateway of the hospital, on the site now occupied by the Pathological Laboratory. It consisted of a waiting-room, a room for electrical treatment and testing, containing also an operating table for treatment by electrolysis. There was also a bath-room containing one electric bath. For many years this was the only electric bath in London. The department contained also a workshop. The current was derived from Leclanché cells. There were three batteries, each containing fifty cells.

During the first year fifty-five in-patients were treated in the department besides many more out-patients. The cases included paralysis, hysteria, ankylosed joints, sciatica, also epithelioma and caruncle, for destruction by electrolysis.

During the next three years, 1883-84-85, 269 in-patients and 876 out-patients were treated.

In 1891 Steavenson died, and Lewis Jones succeeded him. In April, 1896, the first skiagram was taken in this hospital, and since then the amount of X-ray work has increased year by year. In 1907 the Electrical Department moved into its present quarters. Dr. Walsham was appointed Assistant Medical Officer to the department in the same year and took charge of the X-ray work. Lewis Jones devoted his attention to the electrical work of the department until 1912, when he retired. After his retirement the department was divided into two separate and independent departments, one taking charge of the X-ray work, the other of the electrical work.

The progress of medical electricity in this country from 1891 is closely interwoven with the teaching of Lewis Jones, and it is not too much to say that its elevation from the lowly position it occupied in the early nineties to the position it holds to-day as a recognized and valued branch of medicine is due mainly to his influence. The introduction of sinusoidal currents for use in arm baths and full-length baths into this country, the scientific study of the output of induction coils, the introduction into England of the ionic method of electrical treatment, and of diathermy and of the condenser discharge method of testing electrical reactions—all these were due to Lewis Jones. By teaching that electricity when used as a therapeutic agent acted by means either of the chemical (ionic) or of the thermal changes which it produced in the tissues, he cleared the subject of medical electricity of much of the obscurity which formerly surrounded it, and which was mainly responsible for the sceptical attitude assumed by physicians and surgeons towards it.

I shall now refer to the work that is at present done in the department, and the types of case that attend for treatment.

During the year that has passed 497 new patients were admitted to the department. Of these, 284 were cases of disease and injury of the nervous system, mostly of the peripheral nerves, for testing and subsequent treatment. For testing purposes, when time permitted, both condenser discharges and the faradic and galvanic currents were used on

the same case, so that the relative value of the two methods could be determined. For treatment of paralysis the sinusoidal current was generally used, the paralysed parts immersed, when possible, in water in baths. This current has been used in the department for many years, and has given very good results. Its administration in baths is not only an effective method but much labour and time is saved—an important consideration in busy hospital practice.

Sixty-four cases of nævus, nearly all in infants, have been treated, and 186 operations, under anæsthesia, have been performed upon them. For cases of cavernous nævus I obtained the best results by means of electrolysis. The skin receives the least possible damage when this method is used, whereas the use of CO_2 snow or diathermy, if it is to be effective, destroys the skin and the underlying tissue *en masse*, and when the cavernous nævus is situated on the scalp, a large slough is produced which frequently becomes septic. I reserve CO_2 snow for capillary nævi, and use it when they are situated on the trunk or limbs over soft parts.

Forty-nine cases of disease of the skin and accessory structures have been treated, including lupus (eleven), rodent ulcer (eight), epithelioma (three), sluggish ulcers and wounds (seventeen), scars (four), corns (one), warts (one), eczema (two). Two cases of rodent ulcer of the cornea are under treatment by means of zinc ions, applied by a zinc electrode to the edges of the ulcer, where they stain with fluorescein.

Forty-three cases of joint disease were treated. Most of these were cases of rheumatoid arthritis or osteo-arthritis. The rheumatoid arthritis cases did not derive much benefit from electrical treatment. When in a quiescent stage the patients felt stronger and gained more freedom in their limbs. Cases of osteo-arthritis, when one or two joints only were affected, responded better to electrical treatment. As a rule the joint cases were treated during their first few visits with diathermy. For the next few visits we tried the effects of ionization, and continued with that treatment which suited the patient best. If it seemed desirable to prolong treatment for a further period than two months, we gave the sinusoidal current in the Schnee bath. I think that the last mentioned is the best. Many of our patients like to come at intervals for a few applications, and they say that they feel increase of the pain and stiffness if they omit treatment altogether.

Of diseases of the bones, fasciæ and fibrous tissues we had thirteen cases. We have had very few cases of disease of the alimentary,

vascular, and genito-urinary systems. There were three cases of the oral cavity, two being malignant growths of the floor of the mouth. Temporary improvement was effected by diathermy. The other was a benign papilloma of the buccal aspect of the cheek, which was successfully removed by the same method.

Of diseases of the respiratory system the cases were asthma (two), pulmonary tuberculosis (one), chronic rhinitis (one), tuberculous laryngitis (one). These were sent for the purpose of trial of the effect of inhalation of the tungsten arc vapour. The treatment did little good except in the case of tuberculous laryngitis, which benefited considerably. The ulceration and swelling diminished to one half of the former size, and the patient, a drill sergeant, recovered his voice, resumed his work, and did not come for further treatment.

Of diseases of the vascular system (seven in all), most were cases of "chilblain circulation" of the extremities.

One case, only, of disease of the genito-urinary tract was treated—viz., a case of spermatorrhœa—and the result was a failure. No gynæcological cases were sent.

The above-mentioned constitute the majority of the cases seen and treated during 1916. Most of these were suitable for electrical treatment, and derived benefit from it. Many of the joint cases—e.g., those of rheumatoid arthritis, were unsuitable. On the other hand, many maladies for which electrical treatment is especially suitable, were not sent. Cases of disease of the cutaneous system—e.g., warts, corns, papillomata, moles, simple chronic ulcers, &c.; diseases of the vascular system—e.g., chilblains, congestion of various parts; affections of the alimentary system, such as chronic and obstinate constipation, are particularly suitable for electrical treatment. The treatment of chronic infection of certain parts of the female genital tract, which give good results in the hands of French electro-therapeutists, has received very little attention in this country.

In conclusion, I should like to say a few words on the state of medical electricity at the present day. I think that all present will agree that medical electricity does not everywhere occupy the position which it should hold. One reason for this is that some of the most important electrical methods of treatment are of recent or comparatively recent introduction. Thus the ionic method was not introduced into England until 1906. Diathermy was introduced in 1910. Only in recent years has the mode of action of electricity been understood.

Another reason is that the field and scope of electrotherapy are known to so few. Medical students do not study electrical methods of treatment as questions on this subject are not asked in examinations, and they leave hospital with no knowledge other than they may have obtained from the not very inspiring references to the subject in general text-books. Another reason is the number of medically-unqualified practitioners in electro-therapeutics. This must surely make the public believe that for the practice of this subject the only essential is the knowledge of the apparatus and the methods of administering electricity. In a large number of cases for which it is suitable, electricity alleviates one or more of the *symptoms*. If no attention is paid to the disease itself, electrical treatment will lose much of its value. Electro-therapeutics is no more the blind administration of electricity than is medicine the simple prescription of drugs.

The question as to the best way to bring about a wider recognition and a more general practice of medical electricity may be briefly considered. The best way would be to teach the student. The other branches of medicine occupy the largest share of his time, and he is not likely to study seriously any subject on which he knows he will be asked no questions in the examination. A method which I would suggest is to furnish those who send cases for electrical treatment with lists, suitably printed and arranged, of the diseases or symptoms for which electricity is a valuable therapeutic agent, and, wherever possible, send the case back when the treatment is completed. I have adopted this plan at the First London General Hospital, where I receive only cases suitable for treatment with the currents there available. I am also trying the plan at the Miller Hospital. A cause which, I feel sure, hampers the development of medical electricity is the combination of the department devoted to it with the X-ray or massage department. Medical electricity, radiology and massage are large subjects, and the combination of two in one department in a large hospital must be detrimental to one or the other. The remedy is obvious.

DISCUSSION.

Dr. ROBERT ARMSTRONG-JONES: Upon the advice of the late Dr. Lewis Jones I used electric baths at the Claybury Asylum pretty extensively, and my results are published in Dr. Lewis Jones's text-book. The patients were immersed in an earthenware bath with water just above the body temperature, 100° F., and an electrode was placed behind a wooden support at the head and foot of the bath, and a current passed through the water for about twenty minutes. I recorded the cases in a special register, and took the weights of all patients weekly. In certain cases of adolescent insanity in young people, I found there was a decided improvement both medically and physically. The altered metabolism of the body was certainly marked, as most of the patients gained weight, anæmia was lessened, and the patients also improved mentally. The type of patient was that in which mental symptoms had occurred in consequence of bodily reduction, where nutrition had been impaired, and where insomnia and failure to assimilate nourishment had caused anæmia, and in girls irregular menstruation. I did not use a transformer, but wheeled the electric battery to the bath side. I was unable to attribute the improvement to any one factor of causation; possibly all the results—viz., the even pressure of water upon the vessels of the skin, the warmth which dilated the surface vessels and so withdrew blood from the internal organs, including the cerebrospinal nervous system, and the tonic effect of the electrical current upon muscular and nervous conditions, all helped to produce the physiological and therapeutic effects. We know very little of the influence of electricity upon the sympathetic system, but we must realize the great influence of this system upon the general economy of the body.

Dr. JAMES METCALFE: Dr. Cumberbatch's excellent paper shows the great value of the work as carried on in the electrical department of St. Bartholomew's Hospital. It is most necessary that the scientific aspects of the treatment of disease by these methods, and the satisfactory results obtained, should be given as wide a publicity as possible. There exists a want of knowledge of electrical methods and of their utility in treating disease, even in the medical profession, that is truly deplorable. A consequence is that much of this class of treatment falls into the hands of empirics and charlatans, and this largely discredits its character. All sorts of persons without any medical training, possessed of a few electrical instruments, pose as electrical specialists. It is obvious that no individual without a thorough and complete medical training, to which is superadded a special training in elec-

trical methods, is qualified to undertake the work. Efforts should constantly be made to bring before the medical profession, the licensing bodies and the General Medical Council, the importance of this branch of medicine. Medical students should be compelled to attend a few lectures and might be asked some questions on electro-therapeutic subjects in their final examinations.

Dr. NOEL BURKE: There is now, I believe, some organization of this work in Command Depots and convalescent camps, but little or none in other forms of military hospitals. The result is that one too constantly sees cases that have wandered along from Base Hospital in England to V.A.D. or other Auxiliary Hospital, thence to convalescent camp, and at last to a hospital which possesses an electrical department, where, on examination, there is found to be a nerve injury, stiff joint, scar, or other condition which requires treatment, and could have been dealt with much earlier had means been available. Often the patient says he has had electrical treatment but that it did no good. On asking "What kind of treatment?" and "Who gave it?" the reply shows that it was frequently a wrong treatment, and that it was applied by "one of the nurses," not by a nurse who had been trained for the work but by one who happened to be on duty in the ward. This is most unfair to the soldier and to the science of electro-therapy. In civil life it is just the same. I am sure the whole trouble is due in the first place to the lack of instruction given to the profession. Students learn to send cases of fracture and foreign bodies to the X-ray department, but any ideas they may have of the possibilities and principles of treatment by this agency are picked up casually. Their instruction in the capabilities of electricity and other physio-therapeutic agencies is even more meagre. It is an urgent matter, therefore, and we should work for the education of the profession, and then educative and possibly legal measures will be possible to protect the public from the dangers of treatment by unqualified and inefficient people.

Dr. G. B. BATTEN: I recently asked the principal medical officer of a large military hospital what they were doing for trench feet, and referred to electro-therapeutics. His answer was brief: "Electro-therapeutics! what good are they, simply swank." As a general practitioner as well as a specialist in electro-therapeutics I hear both sides of the question. When I suggest to my patients that they should be treated by an electro-therapeutist who has either some special apparatus or some special skill, I find they kick at the fees for a course of treatment, although quite willing to pay two guineas or even more for a single consultation or treatment, and later on I find these same people are going to unqualified men who advertise, simply because their fees are much less. This is an aspect of the question that ought to be seriously considered.

Section of Electro-Therapeutics.

President—Dr. G. Harrison Orton.

(*March* 16, 1917.)

On the Origin of Electric Currents led off from the Human Body, especially in Relation to "Nerve-leaks."

By W. M. Bayliss, M.A., D.Sc., F.R.S.

During the last two or three years certain theories propounded by Mr. Arthur E. Baines have received considerable attention, and have been applied in practice by a number of medical men who have accepted these views. I propose to examine the scientific basis on which they are founded.

The basis of the system lies in the fact that, by leading off from different points of the body by means of silver electrodes, currents of various degrees of magnitude can be detected when the electrodes are connected to a very sensitive galvanometer of high resistance. In a case of shell shock, for example, a particular area of skin may give a large deflection compared with that normally obtained by holding the electrodes in the hands. This is called a "nerve-leak," and is interpreted as an escape of "neuro-electricity," or "nerve energy," from a place where the insulation of the nerve fibres has broken down, similarly to what happens in a submarine telegraph cable.

In the first place, we must remember that the animal body does not contain any metallic conductors; the conduction of an electric current is of a different kind from that in a wire. We must examine, therefore the possible sources of potential difference, giving rise to currents, in such methods as those used by Mr. Baines.

The human body is a system of solids and liquids intermixed. The solids are, when dry, non-conductors, but become conductors when soaked with the solutions in which they are immersed. These liquids contain salts, and it is by means of the salts that the current is conveyed. Hence they were called by Faraday "electrolytes."

How is this done? When salts are dissolved in water they are split up into two or more atoms or molecular groups which have electrical charges of opposite sign. These products of dissociation are known as "ions," and the unit electrical charge carried by an ion is called an "electron." Electrons do not exist in such solutions free from the chemical atoms with which they are combined. In Mr. Baines's book, "Electropathology," ions and electrons are said to be identical, and this fundamental error runs through the whole theory. Ions and not electrons are the source of electrical differences in the body and serve as the only means of conduction of electrical currents. This they do by their actual movement, conveying electricity much as a horse and cart carries stones. Being actual material existences and non-volatile, they do not escape from the surface of the body to the air, as we are told that "nerve-leaks" do. Mr. Baines holds that electrolytic dissociation of salts in water is accompanied by an actual disintegration of the atoms themselves, by which free electrons are formed, as in the case of radium. No proof has ever been given that the salts actually present in the body do this. If it were so, the astonishing equivalence between the amount of energy supplied as food and that given out in various forms by the organism would not be obtained experimentally. This fact is one of the most significant results of modern physiology.

The body therefore is full of ions, but not of free electrons, and any explanation of electrical differences must be on this basis.

As a first step, let us see what happens when two places on the surface of such a moist conductor as the body is are led off by silver electrodes. We will suppose that the two places are at the same potential, and this is most easily obtained experimentally by taking a dilute solution of sodium chloride and immersing the electrodes in it. Now, unless the two electrodes are not only of the same metal, but also have their surfaces in precisely the same physical and chemical state, there will be found to be a potential difference between them. The exact origin of this potential difference would take too long to explain, and it is a somewhat difficult question, depending on the tendency of the ions of the electrode to pass into the solution or those of the solution to

pass into the electrode; but in practice I have found it impossible to get two silver electrodes which are equipotential in salt solutions. I have taken the greatest care and spent much time in the attempt, but have never obtained less than 0·006 volt. This potential difference would give, with Mr. Baines's galvanometer through the ordinary dry skin, a deflection of just about the order of that regarded by him as the normal hand-to-hand effect. Since the deflection obtained by the use of such electrodes on the skin depends on the resistance of the circuit, if one of them be moved to a place where the skin is moist the deflection will be greater, and the greater deflection indicates no more than that the resistance is less. This is the first kind of "nerve-leak," and I would emphasize the fact that no test of the equipotentiality of the electrodes is made in Mr. Baines's practice. In any normal person it is quite easy to find areas where the skin is moist and to compare these with drier places, and it is an omission that so few tests for "nerve-leaks" have been made in normal cases.

The simple state of affairs just described, although always present, is, however, not the whole story. Since the source here is in the electrodes, it is plain that if they are reversed in position, so that the one previously on the right hand is changed to the left hand, and vice versa, the direction of the galvanometer deflection is unchanged, whereas if the galvanometer connexions are changed, without altering the position in the hands, the direction is reversed. This, indeed, is usually the case, but not always. In the latter case it is clear that there must be a source of potential difference in the skin itself, which overpowers that of the electrodes. This can only be investigated by the use of electrodes which do not themselves produce a potential difference when in contact with the skin; in other words, the so-called non-polarizable electrodes must be used. The contact with the skin is made by a salt solution similar to that of the tissues themselves. It is familiar to all physical chemists that such phenomena as those with which we are concerned can only be investigated by the use of such electrodes, but it is unnecessary to describe here the details of their construction. For the present purpose their more important property is that they can be made equipotential, or rather the one electrode has a potential exactly equal and opposite to that of the other, so that in use they give rise to no current. Mr. Baines gives a series of extraordinary objections to their use, based on his view of the importance of static charges and inductive capacity in phenomena of electro-physiology. I shall have occasion to discuss this point presently, and will merely refer here to the instructive photographs

taken by Dr. Thomas Lewis with the string galvanometer, showing the distortions produced by metallic electrodes. They were published in the Proceedings of the Physiological Society in the *Journal of Physiology*, 1915, vol. xlix, p. li.

The most obvious source of electrical changes in the skin is the activity of the sweat glands, long known to be accompanied by such effects. Moreover, Tarchanoff showed that all kinds of mental states produced localized activities of these glands. Shell shock and neurasthenic states are undoubtedly conditions of mental instability and easily induce activity of sweat glands. There may be also other sources of electrical changes in the skin in addition to this.

The explanation of electrical changes in cells is beyond the limit of this article; the most acceptable view is that they are due to the separation of the positive from the negative ions by the interposition of a membrane through which only one kind of the ions is able to pass. Thus we have a Helmholtz "double-layer," which can be best realized by the comparison of a field containing ewes and lambs separated from another field by a fence through which only the lambs can pass. Just as the attraction between oppositely charged ions prevents either leaving the membrane to any great distance, so the attraction of food and parental care prevents both the ewes and the lambs from wandering very far from each other, although on one side of the fence the lambs will be in the majority, on the other side the ewes.

At one time I was inclined to think that these areas of localized sweating might be connected in some way with Head's areas of referred pain, but further investigation showed that it was impossible to refer them to any definite lesions in the nerve centres.

We have, in any case, another kind of "nerve-leak" of a more distinctly physiological origin. It is, when metallic electrodes are used, associated with the first kind, and the result on the galvanometer is the algebraical sum of the two together. Without any further hypothesis, they are capable of accounting for all the results. But Mr. Baines holds that these results are to be explained by static charges and "inductive capacity." I have made numerous attempts to detect the production of currents by giving charges to insulated bodies connected by electrodes to the galvanometer and have failed altogether. Indeed, since the points led off must be equipotential whatever the magnitude of the charge given, it is difficult to see how currents could be produced. The charge, as Faraday showed, is situated on the surface and cannot

affect processes going on in the interior of the body tissues. Static charge may, I hold, be rejected. What about "inductive capacity"? I found some difficulty in discovering how Mr. Baines imagines this to play a part. But, ultimately, it turned out to be this: The galvanometer deflection obtained from a patient is found to be very sensitive to the passing of electric trains, but, instead of looking for the cause in action upon the magnetic system, Mr. Baines attributes the deflection to currents induced in the human body which are being led off. This seems almost inconceivable in a skilled electrician, but no other evidence is given.

In Mr. Baines's theories there is a flow of what he calls "neuro-electricity" along nerves. This is stated to be a kind of ordinary electricity, so nearly the same that it passes as such around the coils of the galvanometer. If this be so, nerves, when cut and the ends placed in contact and insulated from the exterior, should conduct impulses, not of course giving rise to co-ordinate movements, but to movements of some kind. In point of fact, they do not, as is common knowledge. The source of this "neuro-electricity" is supposed to be the brain, and by atomic disintegration. No proof of any kind is given, and, whatever may be the nature of the nerve impulse, it is certain that there is no continuous stream of energy, and all that we know points to its being connected with the concentration of certain ions at membranes.

I pass on to the method of treatment founded on the theory of neuro-electricity. This is the application of "dielectric oil," or similar preparations, which are supposed to act as insulators and thus stop the leaks. Not only are the nerve-leaks of shell shock cured, but also all inflammatory states, such as pneumonia and so forth. All living cells are stated to lose their "vitality" if deprived of their supply of nerve energy. That this is incorrect is shown in a striking way by the experiments of Clara Jacobson, who showed that wounds in a denervated limb heal quite as rapidly as those in a normal limb.

What is this "dielectric oil"? It is merely a good specimen of ordinary liquid paraffin; in fact, its insulating properties are rather inferior to those of a preparation which I obtained from Messrs. Hopkin and Williams. I have made a detailed investigation of the chemical and physical properties of various paraffins, but was unable to find any possessed by the "dielectric oil" beyond those of other commercial samples. More especially is it easy to show that it does not penetrate the skin. If it did so, the electrical resistance would rise. The electrical resistance does not change. Further, the dielectric oil loses the greater

part of its insulating power by contact with salt solutions, as it must be in the skin and tissues.

There is no doubt that liquid paraffin is a pleasant dressing for raw wounds; it is protective, and to some extent excludes infection by micro-organisms. But it has no effect on the results of general infection.

In the attempt to explain the curative results obtained, apart from those obviously due to suggestion, it is necessary to bear in mind the great difficulty of avoiding the fallacy of concluding that a phenomenon which follows another in time is the direct result of the former one. Thus, when the temperature in pneumonia falls after the application of "dielectric" to the front of the chest, how do we know that the temperature would not have fallen without the dielectric? In any case, I cannot accept it as a proof of penetration *through* the chest that the bed-clothes beneath the patient were found soaked with paraffin three hours after its application to the front of the chest. It is rather remarkable that, in Mr. Baines's list of the important properties of his "dielectric," that of low surface tension is omitted.

In conclusion, I may say that it would be unjust to blame those medical men who have adopted the theories of Mr. Baines. They deal with a very complex branch of physiological science, although it is not so complex as Mr. Baines would have it.

The net results of my investigations may be summed up thus:—

(1) Currents led off from various parts of the body by metallic electrodes are due to inequalities in the electrodes, together with differences in the activity of the skin glands.

(2) Neither static charges nor induction play any part.

(3) "Nerve-leaks" are merely places where the skin is moist, and they give no indication of lesions in the nerve centres.

(4) The view that "neuro-electricity" is generated in the brain and escapes from nerves owing to breakdown of insulation is devoid of evidence and contrary to the knowledge we possess of physiological processes.

(5) My examination of Baines's "dielectric oil" leads me to conclude that it is ordinary liquid paraffin; that it does not pass through the skin and cannot reach any nerve or other internal tissue.

(6) Treatment of open wounds by liquid paraffin has some justification in excluding air and perhaps bacterial infection. But it is not new. The results obtained have no relation to insulating properties, and the "dielectric" has no superiority in this respect over commercial samples.

DISCUSSION.

Dr. G. B. BATTEN: We must not entirely condemn a therapeutic agent or method simply because the theory on which it is based or explained is quite erroneous. Many useful agencies have survived despite mistaken ideas of the cause of their usefulness. I have had very little experience of the insulating agent advocated in the books mentioned, and I believe that if this paraffin is useful then castor oil should be more useful, as it is decidedly a better dielectric.

Dr. HERNAMAN-JOHNSON: At the present stage rather too much attention is being given to theoretical considerations in connexion with so-called "nerve-leaks." It is quite possible that all Professor Bayliss's destructive criticism is in this respect justified—but, if this were so, it would not necessarily invalidate the clinical utility of the method. As regards treatment, the value of paraffin as a wound dressing and as a preventative of "trench feet" is well known; but that this value depends upon electrical phenomena cannot be considered proven. That "leaks" can be stopped by the inunction of "dielectric oil," and that this stoppage lasts for at least some weeks after inunction ceases, I have had proof; but this stoppage is by no means always associated with clinical improvement. The precise therapeutic value of the oil treatment in neurasthenia accompanied by "leaks" remains to be proved. So far as diagnosis is concerned, answers, I think, are on firmer ground. I took two cases to Dr. Horne Wilson—one a patient with chronic dysentery, the other suffering from amenorrhœa. There was nothing characteristic about the appearance of either, and no questions were asked. In the dysentery case, abnormal swing of the nerves occurred as the mid-dorsal region was reached; in the amenorrhœa patient, violent deflexion occurred at a somewhat lower level. Dr. Wilson in the one case suggested "liver and bowel trouble," and in the other, "derangement of the pelvic organs." The skin was carefully rubbed with a hot dry rough towel before the testing was done. I understand that Dr. Horne Wilson thinks he has reason to believe that he can pick out cases by his galvanometer in which certain forms of electrical treatment are likely to succeed—e.g., where the galvanometric deflexion is much below normal, galvanism or X-rays may help, whereas he says they will not assist when the reverse phenomena are formed. At present, one often finds it impossible to account for the failure of electrical measures in apparently suitable cases; and it seems to me that any method which even claims to enable us to avoid wasting time and energy should be carefully investigated by electro-therapeutists.

Dr. AGNES SAVILL: I have had very little experience with the "dielectric oil," but it has been good. I may mention the case of a lady who had eczema of both feet, to an equal degree in each, and the foot on which the dielectric oil was used healed more quickly and was less irritable than was its fellow.

Professor W. M. BAYLISS, F.R.S. (in reply): I agree with Dr. Batten that a method should not be condemned because the theory is erroneous. Except in so far as a false theory may lead to the method being tried in inappropriate cases. Should such cases improve, the conclusion is apt to be drawn that the theory was correct. As to the use of castor oil, I pointed out that the value of liquid paraffin does not consist in its dielectric properties, and the odour of castor oil is a disadvantage. Since metallic electrodes can give no trustworthy information as to the electric state of a patient, unless their own potential difference has been previously determined, and not even then should the skin be acid or alkaline, the results obtained in the case of Dr. Hernaman-Johnson's patients can have been merely fortuitous. In Mr. Baines's method the precaution of testing the electrodes is not taken. I cannot agree that the method is at all likely to result in the avoidance of waste of time and energy. It may possibly turn out that electrical states associated with local activity of sweat-glands have some significance with respect to nerve lesions, but the use of metallic electrodes is inadmissible, and a large amount of research would be necessary before any results of value were obtained. So far there have been none. Dr. Agnes Savill would find that ordinary liquid paraffin is quite as good as "dielectric oil" in the treatment of eczema.

Section of Electro-Therapeutics.

President—Dr. G. Harrison Orton.

(*April* 20, 1917.)

Further Note on a New Method of Bullet Extraction.

By A. E. Barclay, Captain R.A.M.C.[1]

A year ago it was my privilege to read before this Section a note on a new method of bullet extraction by means of a specially designed and electrically connected pair of forceps, to which a fluorescent screen was attached, with which bullets and other metallic foreign bodies could be removed under guidance of the X-rays.

If a preliminary note is made, it is only fair to supplement this in the light of further experience, whether the results are all that were expected or not. In this particular case it is particularly difficult to assess the facts—war alters so many things. The instrument, when perfected, performed its extractions well and with extraordinarily little damage to tissues, but, apart from the surgical considerations, there were limitations:—

(1) It could only be used in the direct line of the rays.

(2) It required experience and a knowledge of working with X-ray shadows to perform the extractions quickly and safely.

(3) The dangers of prolonged exposure to X-rays are not appreciated by a very large proportion of Army X-ray workers (who have had no experience in X-ray therapeutics), and such an instrument as this is therefore unsafe in their hands.

[1] Read before the Section by the Honorary Secretary at the meeting on April 20, 1917.

My own experience with the apparatus was successful, apart from slight, if annoying, deficiencies in the instrument, which were eventually overcome. When once the push of last July started, however, there was no further time available for this work and in consequence it has had to be laid aside. Perhaps when the War is over and we have a properly trained staff of radiologists to send out in charge of X-ray departments, the instrument may come to its own, but in the present state of affairs I have no hesitation in saying that its use, in the vast majority of military hospitals, would be accompanied by very grave risk not only to operators but also to patients.

Section of Electro-Therapeutics.

President—Dr. G. Harrison Orton.

(*February* 16, 1917.)

Trauma in the Ætiology of Arthritis.

By Martin Berry, M.D.

Since the days when all affections of the joints were grouped together under the term "articulorum passio" much work has been done in separating the various lesions from each other and in allotting each to its proper class. Many classifications have been made, at first on clinical and later on pathological bases: one early classification was that of Heberden who, in 1804, distinguished between acute and chronic articular rheumatism; and another and later one, that of Charcot, who pointed out the differences between osteo-arthritis and rheumatoid arthritis.

Some writers have contended that osteo-arthritis is merely a later development of the rheumatoid variety, but Llewellyn has pointed out the essential dissimilarity of the two conditions, though acknowledging that they may exist simultaneously in different joints of the same patient. He draws an analogy between this co-existence and that of the presence of gout and osteo-arthritis in the same individual.

A modern classification which is generally accepted is that of Goldthwait, who divides the condition into chronic villous arthritis, atrophic or rheumatoid arthritis, hypertrophic or osteo-arthritis, infective arthritis and chronic gout.

Each of these groups has its distinctive radiographic signs so that the classification we are able to make is essentially pathological. It is my purpose here to allude to only one group—osteo-arthritis—with particular reference to trauma as a factor in its ætiology. This point has been noted fairly frequently and is mentioned by Adams so far back as 1857 in his work on "Chronic Rheumatoid Arthritis," but

I know of no collection of cases specifically arranged to demonstrate the point at issue.

Goldthwait says that the ætiology is not known and merely includes trauma amongst a list of possible factors of importance. Billroth, in the New Sydenham Society's translation of his work, emphasizes the association of osteo-arthritis and arterio-sclerosis, and the same point has been noted by other observers. Hoffa and Wollenberg, in their book on arthritis deformans, report a case in which the blood-vessels of the synovial membrane on the cartilaginous border showed endarteritis obliterans, and a similar connexion is noted by Painter and Erving in the *Medical News* for 1903[1] in a case of osteo-arthritis of the knee following injury and complicated by marked villous hypertrophy.

Hoffa remarks that osteo-arthritis often follows joint injuries, but Llewellyn considers that the injury is probably a determining rather than an actual cause.

In each of my cases the history of trauma was obtained from the patient by asking him to what he attributed his symptoms and noting his reply: in no single instance was any attempt made to bring theory and fact into harmony by endeavouring to discover some remote injury to account for the condition. All cases of joint injury caused by bullet wounds are excluded from the series, also cases in which the changes resulted from sepsis complicating a wound of any sort.

To a certain extent the condition is most common in those parts which are most prone to accidental injury, the knee showing a well-marked preponderance, followed by the ankle and elbow. As the wrist is very liable to be injured it would be expected that osteo-arthritic changes would be seen in it frequently, but this is not the case. In the whole series there is not a single instance of typical osteo-arthritis of this articulation. In some cases the appearances have been those of rheumatoid arthritis, but even this is not common. I am unable to offer any explanation of this, so I merely state it as an observed fact in the hope that some explanation may be offered by others. Cotton, in his book on joint injuries, illustrates a case of arthritis following injury to the wrist, but again it is of the rheumatoid type.

The changes in the shoulder and hip take rather a different form and those in the vertebræ are fairly often seen, especially in the lumbar region, and they probably account for a large proportion of cases of so-called chronic lumbago or sciatica.

[1] *Medical News*, 1903: November 14, p. 912 *et seq.*; November 21, p. 973 *et seq.*; November 28, p. 1022.

Case I.—The first slide is an antero-posterior view of a knee-joint which was injured by a blow on the inner side seven years previously. The injury was not severe enough to stop the patient working, but he has had pain and stiffness in the joint ever since. The condition is one of considerable osteophytic proliferation and it is worthy of note that it is most advanced on the inner side of the joint at the exact spot where the injury was inflicted. In the next plate (fig. 1), which is a lateral view of the same knee, loose foreign bodies are seen, also irregularity of the anterior part of the articular surface of the femur. The loose bodies were taken to be thickened and degenerated synovial fringes. At a subsequent operation these fringes were found and removed, together with a fragment of the articular surface of the femur which had been detached, leaving the irregular area already noted. Osteophytic proliferation



FIG. 1. (Case I.)



FIG. 2. (Case II.)

Fig. 1.—Degenerated synovial fringes with osteophytic proliferation.
Fig. 2.—Irregular outgrowths with ruptured and calcified ligament.

and lipping of the bones were also observed when the joint was opened. Subsequent examination of the fringes which had been removed showed commencing calcification in some of them.

Case II.—The next case is that of a man who fell down a lift shaft three and a half years ago and injured his knee. This view (fig. 2) shows irregular osteophytic outgrowths and in the antero-posterior view some lipping is seen, together with a shadow on the inner side of the joint which I take to be a ruptured and calcified internal lateral ligament.

Case III.—This shows a very advanced stage of osteo-arthritis, general throughout the joint (fig. 3). The history is that of injury to the joint in 1902 and a further blow on it in 1915. The age of the patient is 44.

Case IV.—This slide shows a moderate degree of arthritic change in a knee-joint, following an injury received five years earlier. The same patient had also had an injury to his shoulder and arthritis was present in this joint also.

Case V.—The next pair of slides are of particular interest. The history is that of a horse kick on the outer side of the knee three months before; constant pain and swelling have been noted in that part since then. This lateral view does not show any marked abnormality but the antero-posterior plate (fig. 4) shows much lipping on the outer side of the joint exactly at the injured region. The appearance is suggestive of a much longer history than

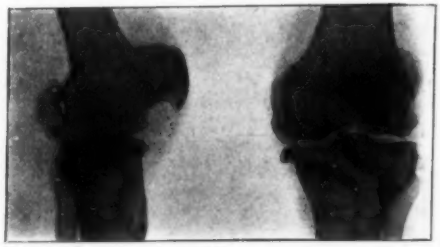


FIG. 3. (Case III.)



FIG. 4. (Case V.)

Fig. 3.—Advanced osteo-arthritis without diminution of cartilage.

Fig. 4.—Injury to the outer side of the knee followed by osteophytic proliferation at the same point.

three months, and I very carefully questioned him as to the possibility of a previous accident. He denied any other injury and declared that his knee was perfectly well up to the time of the accident. The other knee was also radiographed for comparison and found normal.

Case VI.—This is that of a man aged 27 who received a blow on the outer side of his knee eight years ago. He was in bed for three weeks after the accident and has been under a doctor on and off ever since. He now has much pain and can only walk a few yards with much limping. The plate shows a moderate degree of osteo-arthritis which is most marked on the outer side of the joint, where the injury was. It is also seen in this case that there is no necessary connexion between the degree of pain and the amount of structural change.

Case VII.—This is another instance in which the osteo-arthritic changes are especially marked at that point of the knee-joint where the injury was inflicted. The accident was a mule kick on the outer side of the knee a year earlier and the osteophytic proliferation is most evident at that point, though it is seen throughout the joint in a slighter degree. The man is aged 31. (Fig. 5.)

This case and the two preceding ones show marked arthritic changes at the outer side of the knee-joint, associated with injury in that region, and should be compared with the first case shown, in which the injury was on the inner side of the joint and the osteo-arthritis was more marked on the inner side also.

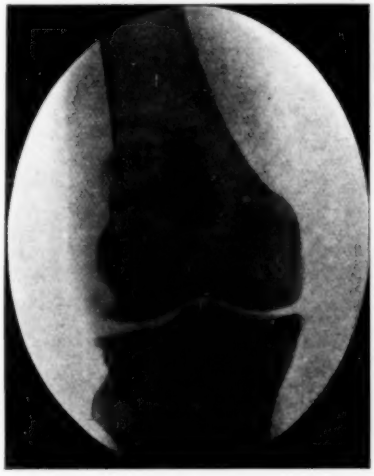


FIG. 5. (Case VII.)



FIG. 6. (Case VIII.)

Fig. 5.—Injury and proliferation on the outer side of the knee.
Fig. 6.—Large calcified loose body with lipping at the outer side of the joint.

Case VIII.—In this case I have not noted the exact date of the injury. The plate (fig. 6) shows a large calcified loose body in the upper and outer part of the knee-joint, together with a moderate amount of lipping at the outer side of the articular surfaces, without any diminution in the quantity of cartilage.

Case IX.—This case again illustrates the fact that there is no connexion between the severity or frequency of the traumata and the amount of change in the joint. There is a history of repeated injuries at football during a period of seven years, yet the arthritic change is only moderate.

Case X.—This is an instance of early arthritic change, both in point of time and of degree. There is a seven months' history and the plate shows general synovial thickening and early osteo-arthritis in the outer part of the joint. It may be suitable to remark at this juncture that changes are observed in the outer part of the articulation much more commonly than in the inner side. This is probably accounted for by the outer side being so much more exposed to injury. If the changes were brought about by fair wear and tear one would expect them to be more marked on the inner side of the joint, since that is the condyle which extends lower down and takes the greater part of the pressure of the body weight.



FIG. 7. (Case XIV.)



FIG. 8. (Case XVII.)

Fig. 7.—Fragment torn off femur and proliferation from injured area of tibia.
Fig. 8.—Synovial fringes and moderate osteo-arthritis.

Case XI.—Here is an instance of a strictly localized change five months after injury. The anterior border of the tibia is roughened at the insertion of the ligamentum patellæ.

Case XII.—Another example of changes at the insertion of the ligamentum patellæ in conjunction with considerable synovial thickening. The history is that of repeated football injuries during a period of twelve years.

Case XIII.—A further instance of injury in the same region is shown in this case. The injury was a kick at football three years earlier, giving rise to constant pain and disability ever since. A large bony mass has developed in the ligament.

The last three cases are not, strictly speaking, osteo-arthritis but are examples of osteophytic proliferation close to and not inside the joint capsule. They are a portion of a series illustrating this process in tendons and ligaments which forms the subject of a separate paper and are only included here for the sake of analogy.

Case XIV.—This shows the appearance of a knee three months after the infliction of a very severe injury. A fragment has been torn off the front of the internal condyle of the femur and lies loose in the upper part of the joint. The area from which it has been detached can be seen opposite the lower part of the patella. There has also been a contused fracture of the external condyle of the tibia and much osteophytic proliferation has taken place at this point (fig. 7).



FIG. 9. (Case XX.)

Lipping of edge of tibia.

Case XV.—In some respects this is similar to the preceding case. The history is that of a fall four months ago, and the synovial membrane is seen to be thickened. The point of resemblance is that of an irregularity on the front of the articular surface of the femur, suggesting that a fragment has been detached, but no trace of the fragment can be seen.

Case XVI.—This is a case of long-standing displacement of a semilunar cartilage. Early osteo-arthritis is present throughout the joint. The other knee was radiographed and found normal.

Case XVII.—As a comparison, here is another case of loose cartilages. Both cartilages were removed from this joint in 1910, but patient still has great pain and disability. The lateral view shows moderate osteo-arthritis and extensive formation of synovial fringes which are undergoing degeneration. This is confirmed by the antero-posterior view (fig. 8). It is interesting



FIG. 10. (Case XXI.

Old fracture of os calcis with advanced osteo-arthritis, most evident in the astragalo-scaphoid joint.



FIG. 11. (Case XXII.)

Osteo-arthritis originating from fracture of the internal malleolus.

to attempt to apportion the ætiology of the condition here seen between the original violence which caused detachment of the cartilages, the continuous irritation of their presence, and the surgical trauma incidental to their removal.

Case XVIII.—This is an example of a condition which I have seen several times. The patient is aged 24, and the history is that he twisted his knee ten years ago and it has been weak ever since. He twisted it again three months ago and now has pain on walking, referred mostly to the outer side of the knee. The plate shows osteophytic proliferation at the attachment of the crucial ligament to the tibial spine.

Case XIX.—This man had an oblique fracture of the tibia several years ago. Firm bony union had taken place in good position and with normal callus formation. Clinically, the ankle-joint was immovable, and is here seen to be choked with osteophytic growths which have mechanically locked the



FIG. 12. (Case XXIV.)



FIG. 13. (Case XXV.)

Fig. 12.—Patches of osteo-arthritis on tibia and astragalus.
Fig. 13.—Osteo-arthritis of ankle following repeated injuries.

joint, though articular cartilage is still present. There is very great deformity of the astragalus and, from the manner in which it is surrounded by osteophytes, it appears probable that it was injured at the same time as the tibia.

Case XX.—In this case again the ankle-joint appeared to be fixed, the history being that of "fracture" twelve years ago. In neither view is there any evidence of fracture, but osteo-arthritis is seen coupled with great increase in density (fig. 9).

Case XXI.—Here also the history was "Pott's fracture" six years ago. No sign of such a condition can be seen, but there has been a fracture of the os calcis, with union in poor position and extensive osteo-arthritic changes in all the bones of the posterior part of the tarsus, especially noticeable in the astragalo-scaphoid articulation (fig. 10).

Case XXII.—This patient had a history of "sprained ankle" three years ago. There is an advanced stage of osteo-arthritis which appears to have originated from a fracture of the internal malleolus (fig. 11). This man also had osteo-arthritis in the shoulder following an operation on the humerus.

Case XXIII.—In this case I have omitted to note the date of the injury. A local patch of osteophytic proliferation is seen on the front of the articular portion of the astragalus. This position is one in which arthritic changes are often seen, leading to great disability by limiting or abolishing flexion of the ankle-joint.

Case XXIV.—This is another instance of arthritis at the front of the ankle-joint. The man is aged 24, and was kicked by a horse on the front of the ankle nine months before this plate was taken. The plate (fig. 12) shows a patch of osteo-arthritis in the region of the injury, both on the front of the lower end



FIG. 14. (Case XXVIII.)

Osteo-arthritis of great toe six years after injury.

of the tibia and on the front of the articular surface of the astragalus. The tendency to osteophytic proliferation at this particular spot on the astragalus is explained by the fact that it is the anterior limit of the articular cartilage.

Case XXV.—In this case there is a history of repeated injuries to the ankle. The patient is aged 35. Considerable osteophytic proliferation is seen at the front of the lower end of the tibia, which seriously interferes with the function of the joint, though the articular cartilage is normal in amount (fig. 13).

Case XXVI.—In this case there is arthritis of several of the intertarsal joints, marked flat-foot, and a commencing calcaneal spur, together with advanced calcification of all the arteries around the ankle. The arthritis is most marked in the astragalo-scaphoid joint, which also shows considerable

alteration of its normal relations. There is a history of injury a year ago, but the flat-foot and arterial degeneration have probably contributed to the development of the condition seen. It is possible that all of the changes are consequent upon the trauma.

Case XXVII.—This man had a horse roll on his foot eighteen months ago. There is now much deformity of the foot and marked osteo-arthritis in the joints around the cuboid and cuneiforms.

Case XXVIII.—This man fell from a wagon six years ago and injured his big toe. He has had constant pain and difficulty in walking since the accident. Extensive osteo-arthritic changes are seen both in this view (fig. 14) and in the lateral plate. The joint is choked with osteophytic growths, but it can be seen that articular cartilage is still present.



FIG. 15. (Case XXXII.)

Extensive changes in head of femur two years after a kick.

Case XXIX.—This is a particularly interesting case. There is a history of injury to the big toe in childhood, followed at the age of 45 by pain in the joint due to the development of osteo-arthritis. It has been stated several times by various authors that injury to a joint in the early part of life may be a determining cause of arthritis in later years, and this case tends to support that opinion. In addition to the condition of the toe, there is a history of several falls in the hunting field, resulting in pain and stiffness of the back, and an examination of that part discloses a fairly advanced stage of osteo-arthritis of the lumbar vertebræ.

Case XXX.—Here is another case of lumbar osteo-arthritis with a history of a single injury caused by being crushed in a coal mine four years ago. There are fairly marked changes in the second, third and fourth lumbar vertebræ.

It has already been stated that the changes in the hip-joint appear to be of a somewhat different character from those already noted. The laying down of new bone is of a more massive variety, and is fairly often associated with considerable deformation of the bone, and what appears to be some absorption. In addition to this there is usually much thickening of the surrounding soft parts, which prevents really good radiographs being obtained; at all events, I personally have found that the appearance of the plates is disappointing.

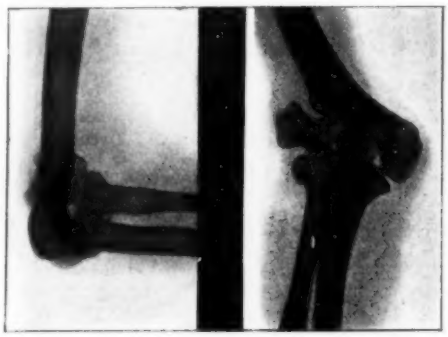


FIG. 16. (Case XXXVI.)

"Candle-guttering" on head of radius two years after severe injury.

Case XXXI.—In this slide it is seen that absorption of portions of the head and neck of the femur and much deposition of new bone have taken place. The history is of a kick on the hip twelve years ago. The joint was greatly swollen, and was aspirated soon after the injury. The articular cartilage is still unaffected, and the only definite change seen in the acetabulum is thickening round its lower border.

Case XXXII.—The appearance of this plate is similar to that of the preceding one. This is the case of a young man aged 20, who was kicked on the hip two years ago. Three months later he began to limp, and now limps badly. The radiograph (fig. 15) shows extensive osteo-arthritic changes in the head and neck of the femur. A large portion of the head has been absorbed, and there is great irregular osteophytic proliferation.

Case XXXIII.—This is the shoulder of a man whose ankle has already been shown (Case XXII). Ten years ago he had an operation on the humerus, and now exhibits marked osteo-arthritic changes in the head of the bone, pronounced lipping being seen at the lower margin of the articular surface.

Case XXXIV.—Osteo-arthritic changes in the elbow are of a similar character to those seen in the knee and ankle. In this case there is a history of five injuries to the elbow during the past sixteen years. In the lateral view there is evidence of osteo-arthritis, whilst in the antero-posterior view it is seen that there has been a fracture of the external condyle of the humerus, with separation of a fragment of bone. This case is a parallel to one shown by Sir Rickman Godlee in 1908, at a meeting of the Clinical Section of this Society.[1]



FIG. 17. (Case XXXVII.)

Advanced osteo-arthritis of elbow after injuries.

Case XXXV.—In this case also there was a history of "fracture" eight years ago. No evidence of fracture can be obtained from the plates, but osteo-arthritic changes are seen even in the lateral view, whilst in the next plate, taken from before backwards, very extensive lipping of the bones and increased density of the part are evident, especially on the inner side of the joint.

Case XXXVI.—This is a man aged 44, who sustained an injury to his elbow nearly two years ago. The extent of the damage will be seen on the slide (fig. 16), the whole of the external condyle being separated and displaced outwards, carrying the radius and ulna with it, so that the upper end of the ulna lies in a gap between the condyles, and does not articulate with any part of the humerus. The point of particular interest is the development of osteo-arthritis in the damaged area, and the typical "candle gutterings" are seen on the head of the radius in addition to other indications of arthritis.

[1] *Proceedings*, 1908, i (Clin. Sect.), p. 95.

Case XXXVII.—The age of this patient is 21, and the history he gives is of "fracture" of the elbow in 1913 and again in 1916. He now has pain and very great limitation of movement. The plates show no evidence of fracture, but advanced osteo-arthritis is obvious (fig. 17).

Case XXXVIII.—In this case the original injury was dislocation of the elbow nine months ago. He still has pain and disability in the joint, and the radiograph demonstrates osteo-arthritis of the head of the radius as the cause of the symptoms (fig. 18).

Case XXXIX.—This is the only hand included in the series, and is shown in order to demonstrate the different effect of trauma on this part. The history is of a sprain in October, 1915, and a dislocation in June, 1916. This



FIG. 18. (Case XXXVIII.)



FIG. 19. (Case XXXIX.)

Fig. 18.—"Candle-guttering" on head of radius nine months after dislocation.

Fig. 19.—Atrophic arthritis of wrist after injuries. To illustrate the different reaction of this joint to trauma.

plate (fig. 19) was taken in September, 1916, and shows loss of lime salts and of articular cartilage in the carpus. Later radiographs showed an extension of this process, resulting in the carpal bones becoming fused into a solid mass with complete solution of the cartilage—the case, therefore, belongs to the group of atrophic arthritis.

In conclusion, I should like to repeat that the history of trauma was in all cases volunteered by the patient, and assigned by him as the cause

of his symptoms. In a large number of cases a similar history was given, and radiographic examination did not disclose any evidence of arthritis. This makes it certain that trauma is only one factor in the ætiology of the disease, but it certainly is a most important one, and should be assigned a more prominent position than it has heretofore held.

DISCUSSION.

The PRESIDENT (Dr. G. Harrison Orton): The classification of arthritis at the present time is very confusing: we hear of osteo-arthritis, rheumatoid arthritis, gouty arthritis, rheumatic gout, and so on, and it seems to me that the adoption of a definite classification is very desirable. From an X-ray point of view, there are certainly two very distinct types commonly met with, which are not always easy to differentiate clinically—namely, one in which there is a very marked osteophytic formation, and very little change in the amount of joint cartilage; and the second, where there is practically no osteophytic formation, and a very marked diminution of joint cartilage. The former we generally call osteo-arthritis, the latter rheumatoid arthritis. That injury is very commonly a predisposing cause in the first class there can be no doubt. With regard to osteo-arthritic changes in the wrist-joint, they are certainly very rare, in fact I cannot, offhand, recall having seen a case in my own practice. The last slide shown by Dr. Berry does not appear to me at all typical of osteo-arthritis, and the changes suggest rather an old tubercular lesion.

Dr. H. A. ECCLES: Will Dr. Berry tell us the ages of the patients whose skiagrams of the tubercle (now called the tuberosity) of the tibia he has shown us, and whether their age would allow of the possibility of the presence of Schlatter's disease. The frequency of osteo-arthritis in the metatarso-phalangeal joints of the big toes is noteworthy, and that this is often caused by repeated frictional traumatism due to ill-fitting boots supports Dr. Berry's argument.

Mr. CECIL ROWNTREE: Dr. Berry's beautiful and convincing series of pictures represent a real attempt to differentiate a quite definite and important type of case from the conglomerate mass of chronic arthritis; and are valuable, not only from the point of view of ætiology and pathological anatomy, but also of treatment. A corollary to Dr. Berry's observations that naturally suggests itself to the surgical mind is the question of preventive treatment, and it seems clear that the only effective treatment of traumatic osteo-arthritis is its prevention—indeed, some of the cases we have seen to-night may almost be regarded as permanent certificates of surgical failure. The more one considers these cases, the more is one convinced that the pathological basis of the degenerative change is the outpouring and consolidation of osteoblastic cells. We are all familiar with the condition brought about by

the formation of osteoid masses in muscle—myositis ossificans. We know the similar formations at the insertion of tendons—tenositis ossificans, if we like to call it so. Dr. Berry has shown us that ligaments may become infiltrated with bone cells, and all these joints reveal strictly analogous changes. So strictly analogous are they that I have been tempted to suggest that we might bring all these conditions into line by describing traumatic joint changes not as osteo-arthritis, but as arthritis ossificans. I do not know if the term has been used before, if not I venture to suggest it as being more truly descriptive of the pathological processes involved than are any of the names in present use. One remark of Dr. Berry's that struck me particularly referred to the great rarity of osteo-arthritis of the wrist-joint, in spite of the frequency with which it is injured. Is it not possible that this may be due to the relative looseness of fit of this articulation? For although functionally so perfect, it is by no means a good mechanical fit—as compared, for instance, with the ankle-joint, where the fit of the opposing cartilaginous surfaces is marvellously close and accurate —so accurate that one may easily imagine a tiny mass of blood-clot or the slightest breach of the articular surface setting up such a degree of frictional resistance as seriously to incommode and ultimately damage the joint. If osteo-arthritis is a more common sequel of minor joint injuries than we supposed, and if, as I firmly believe, the escape of osteoblasts is a factor of supreme importance in its ætiology, then the view that it is dangerous to use early passive movement in the treatment of the injuries of joints receives strong support. To prevent the outpouring of osteoblastic cells should be our aim, and to attain this, anatomical and physiological rest, early and complete, is the only logical treatment.

Dr. MARTIN BERRY (in reply): The purpose of this series of cases is not so much to draw attention to a particular variety of radiographic appearance as to insist on the importance of injury as a cause of the condition. With special reference to the wrist-joint changes I am pleased to note that the President agrees with me as to their rarity. The case which I have shown as atrophic arthritis of the carpus following injury certainly resembles tubercle very much, but I believe that many cases classed as old tubercle are really atrophic arthritis. In the cases of ossification at the insertion of the ligamentum patellæ I have no note of the ages: they are all men of military age. In cases where lime deposition takes place in peri-articular structures, particularly tendons and ligaments, the form of deposition may be an amorphous mass, or may exhibit structure similar to that of bone. In one case I have been able to follow this process from the original injury through a stage of calcification to ossification. Captain Rowntree's nomenclature for the various conditions seems to me to be of assistance in drawing attention to the essential similarity of the processes. He suggests that a revision of existing treatment of minor joint injuries might prevent the occurrence of osteo-arthritis in some cases. If this be the case, the purpose of the paper will have been served.

Section of Electro-Therapeutics.

President—Dr. G. Harrison Orton.

(*May* 18, 1917.)

A Contribution to the Study of Dosage in Radium Therapy.

By J. C. Mottram, M.B.Lond., and S. Russ, D.Sc.Lond.

The information upon radium dosage contained in this paper, was derived from experimental observations which were made upon a patient admitted to the Cancer Wards of the Middlesex Hospital, suffering from carcinoma of the breast. The patient was under the care of Mr. Comyns Berkeley, who gave us every opportunity to pursue our investigations, and we should like to express here our indebtedness to him.

Part I.—Description of the Case.

The patient is a single woman, aged 53. Seven years ago, at the age of 46, she first noticed a small tumour under the skin of the left breast, close to the nipple, which soon became retracted. Three years ago numerous nodules appeared in the skin of the trunk and head. About this time she became a patient at Guy's Hospital, where she had X-ray treatment for some months, with no good effect. Soon afterwards the skin over the growth in the breast became discoloured and broke down, leaving an ulcerated surface upon which a dry crust formed ; the nipple was involved in this ulceration. Cutaneous and sub-cutaneous nodules continued to appear upon the trunk and head.

Eighteen months ago, she was admitted to the cancer wards of the Middlesex Hospital. On admission, she was found to be somewhat wasted and pale. The left breast presented at the site of the nipple a small puckered ulcer covered with a dry crust; the surrounding skin was discoloured and the tissues invaded with growth forming a mass of superficial area 1½ in. by 2½ in., somewhat adherent to the chest wall. Numerous cutaneous nodules of growth were found scattered over the head and trunk, ranging in size from ½ in. in diameter to minute nodules only palpable with difficulty. Upon the chest and abdomen in front thirty-nine nodules were found, upon the back forty-two. Several were present on the face, of which the patient was especially conscious as they interfered somewhat with the contraction of the facial muscles. Several large nodules were found on the scalp, giving rise to bald patches. No enlarged lymphatic glands were found, nor any evidence of other secondary growths.

Since the patient has been under observation, her general condition has much improved. The growth in the breast remained unaltered until radium treatment was applied: First application, 3 mg. for sixteen days; second application, 3 mg. for twenty-seven days; screen 1 mm. lead, area of application 40 sq. cm. After the second application a transient erythema was observed; this was followed by a falling off of the crust which covered the ulcerated surface. The skin over the growth was then found to be intact; the growth appeared to have diminished somewhat in size, in which condition it has since remained.

Since admission, many of the cutaneous nodules have disappeared under experimental applications of radium; on the other hand, many fresh nodules have appeared, and some of the original nodules have increased in size. A few nodules have appeared on the limbs, more especially on the thighs. No evidence of secondary growths in other situations has been found.

From the above description, it is seen that the case presents unusual features. The progress of the disease is very slow; the primary lesion, after seven years, remains very localized in the superficial tissues of the breast: numerous secondary growths are present, and these are entirely confined to the skin.

This type of cancer of the breast, though rare, has been several times described under such terms as "acute miliary carcinosis" and "disseminated scirrhus."

Pathologically the condition is said to be due to the early invasion of the lymphatics of the skin by a growth arising in the super-

ficial parts of the breast, and the subsequent spread of the growth over the skin of the whole body by means of the cutaneous lymphatics.

Histology —A small subcutaneous nodule from the back, on section, presented a scirrhus type of glandular carcinoma. Scattered through a moderately dense, fibrous, connective tissue were small elongated cell masses. These consisted of pencils of undivided and undifferentiated or very finely granular protoplasm, containing numerous large, oval, not very granular nuclei (*see* fig. 1). In many cases these cell masses were seen to be lying in lymphatic channels (*see* fig. 1). The connective tissue contained few cells, chiefly fibroblasts and tissue lymphocytes.

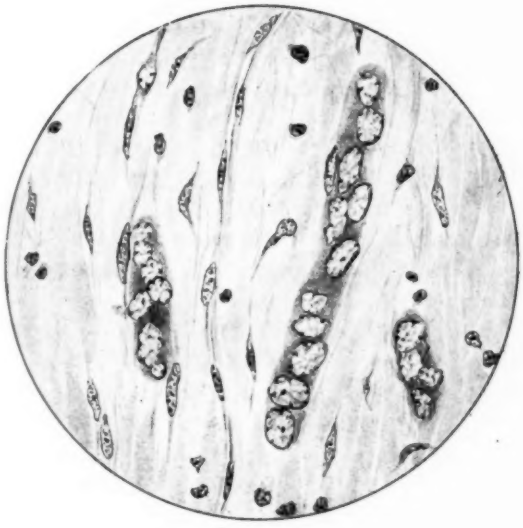


Fig. 1.

The Action of Radium Rays upon the Skin.

The skin changes resulting from exposure formed a progressive series, and are conveniently described under the following headings, given in the order in which they made their appearances: Erythema, œdema, scaliness, pigmentation, blister formation, dry exudation, moist surface, hæmorrhage, healing, hair destruction, leucoderma, and enlarged venules.

Erythema varied from a faint pink to a deep dusky coloration. It was often the only skin change following exposure to radium, and in no case have other skin changes been observed not preceded by erythema.

Œdema.—In a few cases a transient œdema was observed six hours after exposure, more especially when intense sources of gamma rays were employed.

Scaliness and Pigmentation commonly succeeded the primary erythema. In many cases, however, these changes were masked by the more profound changes, such as exudation, following large doses of radiation.

Pigmentation often persisted for many months. In a few cases the erythema was followed by the formation of a shallow *blister*, which either dried up or became a moist surface discharging serum. In a great number of cases the irradiated area became covered with a heaped up *dry exudation* or crust, resulting from the slow discharge of serum, which dried as quickly as it was discharged. In other cases the rate of discharge of serum did not permit of it drying locally, and thus a *moist surface* resulted. In a few cases in which the injury was more severe, the discharge of serum from the moist surface was blood-stained.

In all cases these skin reactions were followed by *healing*, but in many cases permanent alteration of the skin resulted: there was either partial or complete *destruction of the hair.*

If ulceration of the skin had occurred, accompanied by either a dry exudation or a moist surface, then, after healing, a permanent absence of pigmentation (*leucoderma*) was frequently observed; this was associated with a thin, glossy, hairless skin, as is often seen in the scars following other skin injuries. A final permanent change in the irradiated skin consisted in the formation of *dilated venules;* this was always accompanied by leucoderma, and only followed the severe ulcerations.

The Effects of Exposure to Radium upon the Cutaneous Nodules of Growth.

These varied from complete or permanent disappearance to *no* appreciable alteration. Many intermediate effects were noted: for instance, a temporary disappearance with subsequent recurrence; a temporary diminution in size; a temporary stoppage in growth. Owing however to the small size and varying depth of these nodules, it was

found impossible to measure their size from week to week, and thus to follow in detail the growth of each nodule. For this reason observations were confined to the estimation whether or not the nodules had disappeared (to palpation) after exposure to radium. In the tables in Part II the nodules are divided into large and small, and the time of their disappearance is given in days; but in view of the varying size and depth of the nodules, and the variation in the thickness of skin upon different parts of the body, these periods of time can only give a rough indication of the effect of the various doses of radium which were applied. The conclusions in this paper, as regards the effect of radium on the disappearance of these tumours, have been made with this reservation prominently in view.

Part II.—Details of the Investigation.

The study in dosage which has been made, both upon malignant nodules and upon the skin, has necessitated a certain number of

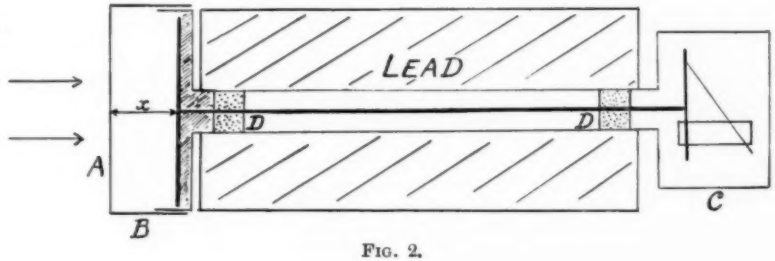


Fig. 2.

measurements of the radiation emitted by the various radium applicators which have been used, and of the absorption which such radiation suffers by the various screens employed. It has also been necessary to compare the amounts of radiation actually absorbed by the skin and subcutaneous tissues, when exposed to the different types of rays, beta and gamma, which have been utilized. For these purposes, a gold-leaf electroscope, which has already been described before this Section, was employed. The main feature of the instrument is that the depth of the chamber in which ionization is produced by the rays is varied to suit the particular requirements of the case.

The ionization chamber consists of a cylindrical box *B*, provided with a window *A*, covered with very thin aluminium (0·001 mm.).

When the rays from an applicator are allowed to enter this chamber, they ionize the air in it, and this is indicated by a movement of the gold-leaf in *C*, which is electrically connected to the electrode in the chamber *B*. From the rate of movement of the gold-leaf over a graduated scale, accurate measurements may be made of the intensity of the radiation employed, and of the extent to which such radiation is absorbed by filters or by tissues intervening. For such measurement the depth x of the chamber *B* is generally kept at about 1 cm. When filters are employed with an applicator, the radiation received by the skin is reduced, both by reason of the absorption which the rays suffer and on account of the increased distance of the applicator from the skin. The former is calculated from measurements which have been described. To determine the latter correction, the depth x is reduced to about 1 mm., and the applicator brought up flush with the window *A*; the ionization is measured, and the applicator now moved back 1 mm. at a time and the diminution of the ionization obtained for each distance.

It will be seen that measurements made in this way, with a very shallow ionization chamber, provide data which are applicable to the case of radiation incident upon, and absorbed by, the succeeding layers of the skin.

One of the objects of the present study has been to compare the effects produced upon the skin when it is irradiated in such a manner that equal amounts of beta and gamma ray energy are absorbed by it. Roughly speaking, we may say that the gamma rays from a radium capsule screened by 2 mm. of lead are about fifty times as penetrating as the beta rays from the same capsule unscreened; in order that the skin should absorb the same amount of beta and of gamma rays it is found that either the time of exposure in the latter case must be about fifty times that in the former if the same capsule be employed, or in order that the time of exposure may be the same, another capsule of about fifty times the strength must be employed. It will be seen from the results recorded that quite different effects are obtained when the time of exposure is very prolonged to those resulting from a short exposure using an intense source.

Fig. 3 represents diagrammatically a section of skin with a nodule situated a few millimetres below the surface. On the right are circles, the areas of which correspond to the amount of beta and gamma ray energy absorbed by each succeeding layer; on the left are the corresponding circles in the case of screened radiation—i.e., of gamma rays

only. In both cases it is arranged that the same amount of energy is absorbed in the first layer, 1 mm. in thickness; this, for convenience, we may take to be 100 units.

When the attempt is made to adjust the dose of radiation administered so that the skin shall absorb the same amount of beta and gamma

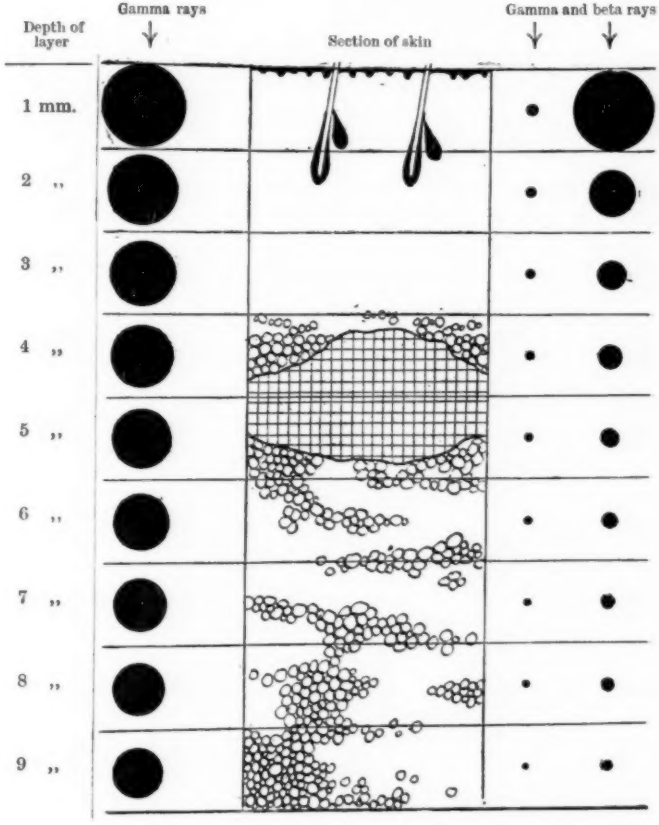


FIG. 3.

ray energy, it will be seen from the diagram, fig. 2, and the data in Table A, that this can only be done for a single layer, owing to the different degree to which the two kinds of rays are absorbed by the tissues. If adjustment to equality is made for rays absorbed by the top

layer of the skin, then the layer, situated at a depth of 5 mm., will absorb about twenty times as much of gamma ray as beta ray energy. It is found, in fact, under these conditions, that rather more profound changes are produced upon the skin by gamma rays than by beta rays, when steps are taken to ensure the equality of radiation absorbed at the surface.

It may well be that disturbances which are seen at the surface, are the sum total of the effects produced at the different layers, so that this more marked reaction to the gamma rays is rather to be anticipated if such reaction is simply a question of the amount of energy absorbed by the cells.

TABLE A.—RADIATION ABSORBED BY FIRST LAYER=100 UNITS IN EACH CASE.

Depth of layer below surface of skin	Unscreened rays		Screened rays (gamma rays only)
	Beta rays	Gamma rays	
1 mm.	98·00	2·00	100·0
2 mm.	31·20	1·40	70·0
3 mm.	13·10	1·27	63·5
4 mm.	7·56	1·12	56·2
5 mm.	4·71	1·01	50·8
6 mm.	3·24	0·92	46·0
7 mm.	2·25	0·83	41·8
8 mm.	1·57	0·77	38·5
9 mm.	1·18	0·70	35·5
10 mm.	0·94	0·66	33·4

On the basis of these and similar data for the various capsules employed, a series of quantitative studies (Nos. 1 to 4 in what follows) has been carried out, each of which will receive separate and detailed consideration.

Dosage Study No. 1.

"*Comparison of the Effects of Beta Rays, according as a Large Quantity is used for a Short Time, or a Small Quantity for a Long Time.*"—For this purpose two capsules of the same area were used: one contained 7 mg. and the other 0·018 mg. radium bromide (hydrated). Subcutaneous nodules were exposed to the two capsules and observations subsequently made of the effect of the radiation upon the nodule and upon the superimposed skin. The data are collected in Table I. Attention may be drawn to two findings: (1) The *time* taken for the reactions does not vary appreciably whether the exposure be two or as much as six hours, but the *degree* of such reaction corresponds with the

length of exposure; (2) there is much less effect upon the skin when it is exposed for a long time to a weak source, than when a strong source is used for a short time. It will be seen that no permanent alteration in the skin occurs when it is given an equivalent of four hours' exposure, using a weak source for a long time; with the strong source, however, this exposure results in loss of hair, leucoderma and the formation of

TABLE I.

Quantity of radium bromide in milligrams	Exposure	Days required for Reaction										
		Erythema	Scaliness	Pigmentation	Exudate	Moist surface	Healed	Hair	Leucoderma	Venules	Nodule	
											Small	Large
7·0	1 h.	22	—	—	—	—	—	No permanent alteration			N.P.	
	1½ h.	3	—	—	—	—	—	No permanent alteration			N.P.	
		9	33	—	—	—	—				37	
		14	—	—	—	—	—					N.P.
	2 h.	1	21	—	+	—	37	Absent	—	—	36	
		3	—	—	30	—	41	Absent	+	—		47
	2½ h.	3	—	—	29	—	57	—	—	—	57	
	4 h.	1	14	—	21	—	37	Absent	+	—	36	
		5	25	—	29	—	46	Absent	+	+		67
0·083	3 d. 12 h. equivalent 1 h., 7 mg.	14	—	100	—	—	—	No permanent alteration			N.P.	
		19	—	100	—	—	—					N.P.
	7 d. equivalent 2 h., 7 mg.	6	24	+	—	—	—	No permanent alteration			42	
		7	23	35	—	—	—					N.P.
	14 d. equivalent 4 h., 7 mg.	3	44	31	—	—	—	No permanent alteration			46	

Area of capsules, 3·14 sq. cm.
Screen, 0·1 mm. aluminium, 1 layer of lint.

N.P. signifies "nodule persists."

venules. The effect upon the nodules, however, is very nearly the same in the two cases, which brings into contrast the behaviour of the malignant cells with that of the normal cells of the skin. The same contrast will be seen later under different experimental conditions.

Dosage Study No. 2.

"*Comparison of the Effects of Screened Beta Rays under Various Conditions of Irradiation.*"—In this series two capsules were used: one contained 7 mg., spread over an area of 3·14 sq. cm., and the other, 24 mg. over an area of 0·5 sq. cm., thus having an intensity 24·4 times greater than the first. The exposures were all based upon the experience gained with the 7 mg. capsule unscreened. Two hours' exposure with this capsule led to a definite clinical picture, the final phase of which was a loss of hair and leucoderma. When a screen of

TABLE II.

Quantity of radium bromide in milligrams	Exposure	Equivalent	Days required for Reaction										
			Erythema	Scaliness	Pigmentation	Exudate	Moist surface	Healed	Hair	Leucoderma	Vessles	Nodule	
												Small	Large
7	15 h.	2 h.	2	—	—	—	—	—	None	—	—	N.P.	
	15 h.	2 h.	—	—	42	—	—	—	Some hair	—	—		N.P.
	18¾ h.	2½ h.	—	—	—	slight 20	—	33	None	—	+	52	
	22½ h.	3 h.	—	—	—	21	—	35	None	—	—	60	
	30 h.	4 h.	4	—	—	19	—	39	Some	+	—	43	
	—	—	7	21	—	28	—	48	None	—	+		100
	48 h.	6½ h.	3	15	—	24	27	38	None	+	+		45
24	10 m.	½ h.	7	38	—	—	—	—	No permanent alteration			76	
	20 m.	1 h.	2	21	—	27	—	30	No permanent alteration			101	
	60 m.	3 h.	3	21	—	24	—	31	None	—	—	31	

Area of 7 mg. capsule, 3·14 sq. cm.
Area of 24 mg. capsule, 0·5 sq. cm.
Screen, 1 mm. aluminium, 1 layer of lint.
N.P. signifies "nodule persists."

aluminium 1 mm. thick is placed between such a capsule and the skin, the dose of rays received by the skin is reduced to about 13·5 per cent.; consequently in order to adjust the surface dose to equality, the time of exposure must be increased from two to fifteen hours, *vide* first observation, Table II. Although this dose gave rather a slighter

disturbance than when unscreened rays were used, generally speaking, the reactions observed, whether the rays were screened or unscreened, were essentially of the same kind when the dose absorbed was adjusted to equality. With the small capsule of high intensity, distinctly more marked effects were obtained upon the skin than with the weaker source.

This finding is analogous to that recorded in Study No. 1. The source of high intensity appears to be more effective in its action upon the nodule than the weaker source. Whether this be due to the shortened period of irradiation, or to the small size of the nodules which were under observation, it is not possible to say.

Dosage Study No. 3.

"*Comparison of the Effects of Gamma Rays over a Wide Range of Intensity of Radiation.*"—A capsule covered by 1 mm. of lead emits mainly gamma rays; while it is true that there are beta rays of sufficient penetrating power to go through this thickness of lead, such rays form a very small percentage of the beta rays, and in fact of the effects to which the screened radiation gives rise; the secondary radiation emitted by the lead, due to the passage of gamma rays through it, is of a soft type and is cut out by a few layers of stiff paper and lint.

For this series three capsules were used, the 7 mg. and 24 mg. ones already mentioned, with the addition of a third one of just the same dimensions as the 7 mg. one, but filled with radium emanation. As in previous studies, the exposures were all based upon the unscreened radiation series with the 7 mg. capsule: covering this capsule with 1 mm. of lead necessitates increasing the exposure from two hours to about five and a half days, in order that the top layer of the skin shall actually absorb the same amount of energy. Such an exposure, *vide* Table III, is just as effective in causing the disappearance of the nodules as the shorter exposure to unscreened rays but in an exactly analogous manner to that detailed in Study No. 1; the effect upon the skin is much less. When the quantity of radiation is increased so that the exposures are a matter of hours, *vide* 94·5 mg. acting for ten hours (this being equivalent to 7 mg. unscreened for two hours), the effects upon the skin are not less, but rather more pronounced when gamma rays are used than when unscreened radiation is employed. The reason for this is probably not far to seek: a reference to the diagram, fig. 3, shows that when surface equality of beta and gamma rays is established,

the layer at a depth of 5 mm. will be actually absorbing about nine times as much gamma ray energy as that of beta + gamma type. This enhanced reaction obtained with the gamma rays is well seen with an exposure equivalent to one hour. With gamma rays (quantity 32·9 mg., acting for fourteen and a half hours) a typical exudate was produced and the subcutaneous nodule disappeared. Unscreened rays for one hour produced no such effect (*vide* Table I).

TABLE III.

Quantity of radium bromide in milligrams	Exposure	Equivalent	Days Required for Reaction								Nodule	
			Erythema	Scaliness	Pigmentation	Exudate	Moist surface	Healed	Hair	Leucoderma	Venules	Small	Large
7·0	5 d. 9½ h.	2 h.	—	—	—	—	—	—	No permanent alteration			66	
	10 d. 19 h.	4 h.	24	57	45	—	—	—	None	—	—		73
24·0	2½ h.	1 h.	5	—	—	—	—	—	No permanent alteration			N.P.	
	5 h.	2 h.	—	—	—	—	—	—	No permanent alteration			35	
	10 h.	4 h.	3	—	19	48	—	+	None	—	—	61	
	15 h.	6 h.	5	26	—	32	39	+	None	—	—	54	
Radium emanation	30 h.	12 h.	2	15	—	29	31	+	None	—	—	46	
32·9	14½ h.	1 h.	7	—	—	25	—	42	No permanent alteration			46	
70·0	10 h.	1½ h.	1	26	—	29	33	50	None	—	—		60
94·5	10 h.	2 h.	2	—	—	20	36	48	None	+	—	52	
83·2	23½ h.	4 h.	2	—	—	19	27	47	None	+	+		52
58·0	2 d. 9 h.	6 h.	1	—	—	17	28	59	None	+	+		66
56·3	*17 h.	2 h.	1	—	—	26	33	60	None	+	+		61
70·0	*28½ h.	4 h.	—	21	—	27	34	61	None	+	+	69	
47 7	†2 d. 21 h.	4 h.	—	19	—	25	32	60	None	+	+	60	

Area of 7 mg. capsule and emanation capsule, 3·14 sq. cm.
Area of 24 mg. capsule and emanation capsule, 0·5 sq. cm.
Screen, 1 mm. of lead, 1 layer of lint.

* Screen, 1 mm. lead, 0·3 mm. aluminium, 1 mm. paper, 1 layer of lint.
Screen, 3 mm. lead, 0·3 mm. aluminium, 1 mm. paper, 1 layer of lint.

N.P. signifies "nodule persists."

Dosage Study No. 4.

"*Comparison of the Effect of Unscreened (Beta and Gamma) Rays upon the Skin and Subcutaneous Nodules, over a Wide Range of Intensity of Radiation.*"—A capsule of the same size as the 7 mg. capsule was filled with radium emanation and a series of applications made to the

TABLE IV.

Quantity of radium emanation, equivalent milligrams	Time in minutes	Equivalent	Days required for Reaction									
			Erythema	Scaliness	Pigmentation	Exudate	Moist surface	Healed	Hair	Leucoderma	Vesicles	Nodule
84·00	5·7	1 h.	1	19	—	—	—	—	—	—	—	N.P.
35·00	13·5	1 h.	1	—	25	31	—	48	—	—	—	N.P.
17·40	27·5	1 h.	1	38	56	—	—	—	—	—	—	N.P.
6·80	71·0	1 h.	23	40	33	—	—	—	—	—	—	N.P.
84·00	11·4	2 h	1	—	—	23	—	43	—	—	—	49
35·00	27·0	2 h.	1	18	—	42	—	49	—	—	—	49
17·00	55·0	2 h.	1	28	—	32	—	54	—	—	—	54
6·80	142·0	2 h.	5	17	—	37	43	50	—	—	—	50
1·12	900·0	2 h.	2	26	32	—	—	39	—	—	—	N.P.
84·00	22·8	4 h.	1	19	—	23	—	37	—	—	—	19
35·00	54·0	4 h.	1	—	—	18	—	36	—	—	—	42
17·40	110·0	4 h.	1	10	—	22	—	30	—	—	—	30
5·60	345·0	4 h.	4	—	—	22	26	39	—	—	—	39
1·60	1,380·0	4 h.	4	—	—	—	22	41	—	—	—	41

Area of capsule, 3·14 sq. cm.
Screen < 0·1 mm. silver, 1 layer of lint.
N.P. signifies "nodule persists."

nodules. As the emanation decayed, further applications were made, and a series was completed in which the quantity of radiation varied from that equivalent to 84 mg. radium bromide to as small a quantity as 1·12 mg. In this way, data were obtained both of the skin and nodule reactions; they are grouped together in Table IV, and as before,

the basis of exposure was made upon the one, two and four hours observations with the 7 mg. capsule. The general trend of the results is to confirm those which figure in Studies 1, 2 and 3. The times taken for the different phases of the skin reaction to appear do not vary very much with the dose administered, but the degree of such reaction does. Again, when a small quantity of radiation is administered for a long time, there is far less disturbance of the skin than occurs with a large quantity applied for a short time. We have not been able to apply a larger intensity of unscreened rays than 84 mg. over an area of 3·14 sq. cm.—i.e., 23·6 mg. per sq. cm. With an exposure of 11·4 minutes to such radiation, the skin became blistered and an exudate appeared in about three weeks, which soon healed.

It is a matter for further experiment to decide whether, if ten times as intense a source were applied for 1·14 minutes, there would be produced an equivalent reaction. We are not aware of any data dealing with such very intense sources of radiation.

Discussion of Results.

There are two facts which have been established, as a result of this study upon dosage:—

The first is that if the skin be irradiated in such a manner that neighbouring portions absorb the same amount of beta ray and of gamma ray energy, then the reactions are of the same kind, but they are generally of a more pronounced degree with gamma rays. This latter is probably to be explained by the fact that when the adjustment is made so that the surface layer absorbs the same amount of the two kinds of rays, the layers at a depth and which contribute to the reaction, absorb more gamma rays than beta rays.

The second fact is, that if the skin be exposed to a large quantity of radiation (beta rays or gamma rays) for a short time, a much more pronounced reaction is observed than if the same dose be so administered that the quantity is small and the time correspondingly prolonged.

The effects observed upon the malignant subcutaneous nodules, however, is not appreciably different in the two cases.

In endeavouring to find some explanation of these findings, it may be useful to state the experimental conditions in another way. If we look upon the radiation employed as an irritant, then it appears that the skin is able to cope with such an irritant, provided it be not too intense, so that when a weak source is employed over a long interval of

time the changes observed in the skin are very slight; the action of the irritant is counteracted by some defensive powers of the cells of the skin; in the tumour cells however, this is not the case within the limits of our experimental range of dosage. It appears not improbable that the different effects to which the same amount of radiant energy can give rise, even in the same variety of cells (for instance those forming the skin) according to whether such radiation be employed over a short or a long time, are very intimately bound up with the series of changes which form the cycle of the cells' life—viz., growth to maturity, division and growth once more.

It has been shown by several observers that the stage of cell-life represented by division, is an especially vulnerable one to radiation. Mottram has shown that the ova of *Ascaris megalocephala* are about ten times as vulnerable when in the dividing stage as in the growing stage. Such a factor we may term the "factor of vulnerability"; in view of such a finding and the possibility that it applies also in the case of malignant cells, it appears a desideratum in the clinical application of radium that in the process of irradiation all the cells of the tumour should be in the dividing stage, for in that case they would be irradiated when they were at the condition of maximum vulnerability. The difficulty of obtaining such a condition of things, is the comparatively long time it takes for such cells to complete the cycle of their life. For example, a rapidly growing sarcoma of the rat which we have had under observation for five years, goes through the whole cycle in about four days, say one hundred hours, and the division time is about one hour. This is rather a rapidly growing tumour, yet even in this case it is seen that an exposure lasting four days would be necessary, in order that all the cells should receive irradiation during the phase of division.

From a general consideration of the problem, it will appear that unless the "factor of vulnerability" be something comparable with the ratio of the times, which represent the two main stages of cell life (viz., growth and division), then little advantage is gained by prolonged exposure, except in so far as skin reactions are concerned. Hence it follows that in most cases, the main consideration is that the "growing" cells should receive a dose of radiation which is sufficient to prevent their reaching the dividing stage; such a dose should be more than enough to arrest the process of division in those cells which were in this stage at the time of irradiation.

We have pleasure in recording our thanks to the Radium Institute for kindness in giving us large quantities of radium emanation upon several occasions, for the purposes of this investigation.

DISCUSSION.

Dr. HERNAMAN-JOHNSON: May I call attention to what I term the "time-forms" of tumours. Tumours which appear clinically and histologically identical may differ greatly in the rate and periodicity of their growth. Some years ago I suggested that "time-forms" might be studied experimentally by growing portions of tumours *in vitro* by Carrel's method. It is well known that apparently similar tumours show wide differences in their response to the same treatment. But that such tumours have similar "time-forms" has not yet been shown. The experiments described in Dr. Mottram's and Dr. Russ's paper approach the same subject from a different angle, and show the varying reaction produced by altering the time factor when applying radiation to the same tumour. Gamma rays are agents producing chemical and electronic changes in the tissues at a certain definite rate per minute. If the body cells are regarded as inert, then it is a matter of no consequence how many minutes, hours or days are occupied in administering a given dosage of rays. But the cell is by no means inert. Chemical and other changes occur in it at a definite rate: and the time-ratios of cell-activity and gamma ray activity cannot fail to have an important bearing upon the results obtained. Briefly, it may be said that statical conceptions of tumours have hitherto held the field too exclusively. Every cell and, consequently, every neoplasm, presents a problem in dynamics; and, in calculating the outcome of opposing forces, it is well known to the physicist that *the relative times during which they act* must be carefully taken into account.

Dr. J. A. MURRAY: Vulnerability and dosage are apparently closely related. A curious parallel exists between the lethal action of radio-active agencies and the similar effect of incubation *in vitro* at body temperature, when studied in transplantable growths. In experiments carried out in the laboratory of the Imperial Cancer Research Fund, it was found that two separate growths, a carcinoma and a sarcoma, were not equally vulnerable when incubated at body temperature outside the body. The sarcoma, which has a rather higher energy of growth and very much higher resistance to artificially induced immunity than the carcinoma, was found to be much more vulnerable, and it was probable that the greater susceptibility in this case was an expression of interference with the whole metabolic activities of the cells, and not restricted to the single mechanism of cell reproduction.

Dr. R. KNOX: I have been greatly interested in the facts submitted to us by Dr. Mottram and Dr. Russ. I should like to show two lantern slides and give the details of technique employed in the treatment of two cases of rodent ulcer:—

C. M., female, aged 57. Rodent ulcer of left upper lip, extending from the margin of the lip to the outer limit of ala nasi, and inwards nearly to the middle

line. There was ulceration at the centre with thick crusts at the periphery. The history given showed a duration of nearly ten years, with rapid growth in the last few months. The condition was beginning to give rise to trouble, and it appeared that if something could not be done quickly the ulcer would soon get beyond the reach of treatment. Realizing that the treatment must be vigorous, and that accuracy in the estimation of the dosage was most essential, the following exposure was arranged for: The ulcer was roughly divided into four areas, care being taken to ensure that a good margin of healthy skin all round was irradiated—the central portion received the greater exposure. Sixty mg. of radium bromide of a high percentage of purity were used in two small platinum tubes of $\frac{1}{3}$ mm. thickness; these were placed side by side on a thin

FIG. 1, *a*. FIG. 1, *b*.



FIG. 1, *c*. FIG. 1, *d*.

sheet of lead of $\frac{1}{2}$ mm. thickness, and two layers of lint placed between it and the skin surface. Each of the four areas received three hours' exposure, twelve hours in all, the radiations being distributed equally over the ulcer, the central portion probably receiving a portion of the irradiation from all four exposures. A photograph, taken on June 8, 1916, just before the first treatment, showed the condition seen in fig. 1, *a*. The treatment was followed by a sharp reaction of almost equal intensity over the whole of the ulcer, with a redness of the healthy skin well beyond the edges of the ulcer. The crusts cleared off, leaving a slightly raised surface—shown in photograph (fig. 1, *b*), taken on July 17, 1916. The upper outer edge appeared to be more active than the other portions of the ulcer. A dose of two hours' duration was given to this

under the same conditions of filter as in the first treatment. Progress went steadily on in the right direction. On July 30 a photograph taken (fig. 1, *c*) showed a marked change: the ulcer was slowly healing; the upper portion showed a depressed area which did not look like healing. On July 23 a third exposure was given under the same conditions of three hours' duration. Progress was uninterrupted from that date onwards to September 4, when a fourth photograph (fig. 1, *d*) showed the ulcer completely healed. The patient has been under observation up to date, and remains well.

This case illustrates the importance of accurate dosage in radium treatment. Over or under dosage might readily have led to disaster. The condition when first seen strongly suggested that if a result could not be obtained quickly the ulcer would break down and extend rapidly. Such at least has been my experience in other cases. The method of overlapping the exposures and ensuring a thorough irradiation of a good margin of healthy tissue was carried out with the idea that if a fairly strong reactive change could be induced clear of the



a. FIG. 2. *b.*

growth it might help to localize the growth if by chance it tended to increase after the treatment. That is a point which does not receive the attention it deserves in both radium and X-ray treatment. It might be argued with a fair amount of reason that the whole of the healing was the result of an accurately calculated exposure. The other two exposures were given to areas of the ulcer which did not appear to be progressing so favourably as one could have wished. Another object was achieved by these succeeding treatments. Small nodules frequently persist for long periods after the larger portion of the ulcer has healed. These are always sources of fresh activity if they are allowed to remain untreated. They are also a source of anxiety so long as they persist, so it is sound policy to get them removed as soon as possible. I have frequently been surprised at the rapidity with which small ulcers clear up if they are given a sufficiently strong initial dose. It would appear that all growths have what might be called a critical dose, which is sufficient to induce rapid and

uninterrupted healing. If this can be estimated accurately, then all is well, but if the exposure be over or under done, the ulcer either fails to respond or goes on rapidly increasing as a result of the stimulation it has received from the irradiation. Then no two growths seem to possess the same critical dosage value. It is in this direction that the work of Dr. Mottram and Dr. Russ is so helpful to us, and I am certain their work has gone a long way towards clearing up some very difficult problems. I could quote a number of other cases which have surprised me by the rapidity of their progress towards cure, but I shall content myself with one more case, photographs of which you will see on the screen (fig. 2, *a*, *b*).

Corporal Dixon, aged 35. History: An obscure one of having cut the side of his face with a razor twelve months before he presented himself for treatment. A small punched-out ulcer on the right side of the face in front of the ear. He had had various treatments, including zinc and ionization, with no good result. Seen first on October 20, 1916. Treatment prescribed: Brine fomentations for two or three days to clear up the ulcer before giving radium exposures. October 24, 1916: Radium, 60 mg., in two small platinum tubes $\frac{1}{3}$ mm. thick, with 1 mm. lead and six layers of lint—three hours' exposure. Seen again on October 31, 1916: Crust had formed over the ulcer, there being now no discharge. November 7, 1916: Ulcer healed; several small nodules around the edges. Further treatment of one hour. November 21: Two hours to upper aspect of area. December 19, 1916: One hour. January 23, 1917: Two hours. March 20, 1917: Two hours. When last seen the ulcer was quite healed and the surrounding skin healthy. (Patient returned for treatment of a small crust on the edge of the ulcer, June, 1917.)

These two cases illustrate the importance of an initial dose of sufficient intensity to produce changes of a favourable nature. The improvement was brought about with filtered rays, chiefly the gamma radiation being used, but I have obtained equally good results with unfiltered radiations, using a flat applicator with the radium incorporated in a varnish, no filter other than gutta-percha tissue and lint being used. The radium was one-quarter the activity of that used in the cases described, 20 mg. of the weaker preparation being used. The exposures were about two hours to an area; this was sufficient to induce a moderate degree of reaction, followed by disappearance of the ulcer or recurrent nodule. I feel sure that when we have had time to absorb the tables given by Dr. Russ we shall be greatly helped in our practical work.

Dr. S. GILBERT SCOTT: Can Dr. Russ say whether in the course of his investigations on radium therapy there is any reason to suppose that cells in the various stages of development are destroyed by the various types of rays emanating from either radium or an X-ray tube? In other words, will a tumour made up of cells in various stages of development be more effectually destroyed by being bombarded by rays of various penetration, rather than by large doses of one particular type. This is an important point in the question of treatment, for if the former assumption be correct, better results should be obtained from

lightly screened radium doses, or by an ordinary X-ray tube, rather than by a Coolidge tube. My impression—from practical experience—is that a tumour disappears more rapidly by using massive doses from an ordinary X-ray tube than from a Coolidge tube.

Dr. S. RUSS (in reply): With regard to what Dr. Hernaman-Johnson and Dr. Murray have said concerning the many aspects of what may be termed vulnerability, I entirely concur that one variety of cells may be more vulnerable than another to one agent such as radiation and less so to another such as heat. Many more experiments are required before any kind of generalization can be made upon the subject. In answer to Dr. Scott, I know of no observations which throw any light on the important question as to whether the cell responds to monochromatic radiation in an essentially different way to a mixture of rays having the same average penetrating power.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

EDITED BY

J. Y. W. MacALISTER

UNDER THE DIRECTION OF

THE EDITORIAL COMMITTEE

VOLUME THE TENTH

SESSION 1916-17

SECTION OF EPIDEMIOLOGY & STATE MEDICINE



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1917

Section of Epidemiology and State Medicine.

OFFICERS FOR THE SESSION 1916-17.

President—G. S. Buchanan, M.D.

Vice-Presidents—

W. H. Hamer, M.D.
G. B. Longstaff, M.D.
J. C. McVail, M.D.
Sir Patrick Manson, G.C.M.G., M.D., F.R.S.
Sir Shirley F. Murphy, F.R.C.S.
A. Newsholme, C.B., M.D.
J. Niven, M.B.
Col. J. Lane Notter, M.D.
Sir Arthur Whitelegge, K.C.B., M.D.

Hon. Secretaries—

William Butler, M.B.
Major Greenwood.

Hon. Secretary for Navy—Fleet Surgeon P. W. Bassett-Smith, C.B., R.N.

Hon. Secretaries for Army—

Col. R. H. Firth, A.M.S., F.R.C.S.
Lieut.-Col. A. M. Davies, R.A.M.C.

Hon. Secretary for Scotland—A. K. Chalmers, M.D.

Foreign and Colonial Hon. Secretaries—

Sir Arthur Whitelegge, K.C.B., M.D. (France).
R. H. Saltet, M.D. (Holland and Belgium).
Frank G. Clemow, M.D. (Russia).
Arthur Shadwell, M.D. (Portugal).
Major F. A. Smith, I.M.S., M.D. (Baluchistan).
Sir Armand Ruffer, C.M.G., M.D. (Egypt and Red Sea).
Professor S. Kitasato (Japan).
R. Bruce Low, M.D. (South America).
J. A. Lowson, M.B. (China).
F. Tidswell, M.B. (New South Wales).
B. Glanvill Corney, I.S.O. (Polynesia).

Other Members of Council—

E. F. Bashford, M.D.
Edgar L. Collis, M.B.
S. Monckton Copeman, M.D., F.R.S.
F. J. H. Coutts, M.D.
F. G. Crookshank, M.D.
R. Dudfield, M.B.
K. W. Goadby.
E. W. Goodall, M.D.
Surgeon-General W. G. Macpherson, A.M.S., C.M.G., M.B.
Charles E. Paget.
Sir Ronald Ross, K.C.B., F.R.S
J. E. Sandilands, M.D.
F. M. Sandwith, C.M.G., M.D.
W. J. R. Simpson, C.M.G., M.D.
T. H. C. Stevenson, M.D.
C. W. F. Young, M.D.

Representative on Library Committee—E. W. Goodall, M.D.

Representative on Editorial Committee—William Butler, M.D.

SECTION OF EPIDEMIOLOGY AND STATE MEDICINE.

CONTENTS.

October 27, 1916.

C. G. Moor, Captain R.A.M.C. PAGE

Army Sanitation at a Base Camp 1

November 24, 1916.

W. H. Hamer, M.D.

The Epidemiology of Cerebrospinal Fever 17

M. Greenwood, Jun., Captain R.A.M.C.(T.F.).

The Outbreak of Cerebrospinal Fever at Salisbury in 1914-15... ... 44

February 23, 1917.

A. W. Bacot.

The Louse Problem 61

May 25, 1917.

Muriel Robertson, M.A.

Recent Researches into the Ætiology of Typhus 95

The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

LONDON:
JOHN BALE, SONS AND DANIELSSON, LTD.,
OXFORD HOUSE,
83-91, GREAT TITCHFIELD STREET, OXFORD STREET, W. 1.

Section of Epidemiology and State Medicine.

President—Dr. G. S. Buchanan.

(*October* 27, 1916.)

(Chairman—Sir Shirley Murphy, F.R.C.S., Lieutenant-Colonel R.A.M.C.(T.).)

Army Sanitation at a Base Camp.

By C. G. Moor.[1]

In the early part of the War the hurrying out of troops and materials and the making of camps occupied the energies of the regular units so completely that many details of sanitation could not be attended to as thoroughly as they were at a rather later date. Having taken out the First Section of the Sanitary Company in September, 1914, I find it interesting to compare the state of things then obtaining with the conditions which prevail at the present day. I believe that much the same may be said of the sanitary conditions at the Front, that is, that vast improvements have taken place, but as my experience has been entirely confined to Base work, all the remarks I have to offer you to-night must be understood to refer to Base sanitation only.

Before mentioning work done in France it is advisable to state that the two Sanitary Companies (the First and the Second London) were formed in 1908, at the time of the creation of the Territorial Forces, but were never at full strength. The training consisted of ordinary infantry drill without arms, stretcher drill, physical exercises, marching, and some lectures on general sanitation, water purification, the disposal of refuse, and the construction of the simplest types of field latrine and urine pits. Subsequent experience has shown that the stretcher drill ought to occupy less time, and a much more detailed training in water-work and general sanitation, practical as well as theoretical, is what is wanted.

[1] Captain, First London Sanitary Company, and County Analyst to Dorsetshire, &c.

In war-time the Sanitary Companies exist solely for the purpose of training men who go out, not as a Company but as a Sanitary Section. A Sanitary Section consists of an officer and twenty-five men. The officer may be a medical man, but he may also be a non-medical man with sanitary or engineering qualifications. Of the twenty-five men, three or four should be sanitary inspectors, five or six of the others should be men with trade experience, such as that of plumbing or carpentering, the others may be men of good general education. The chief point is that they should be adaptable, keen, possess a general knowledge of field sanitation, and be able immediately to sterilize water on a small or large scale, to carry out a practical campaign against flies, know how to build furnaces, be capable of burning camp rubbish, and fæces and horse manure, construct grease-traps, &c., and pick out and remedy on the spot all minor sanitary defects in a camp. Finally they must all have the power of making camp inspections with tact, they must be clear and definite in their reports to their O.C., and able to impress the sanitary squads of the units they are sent to inspect, so that their advice is attended to and carried out without causing friction.

The ranks that are authorized are as follows: The officer may be a lieutenant or a captain; of the men, one is a staff-sergeant and another a sergeant; there are two corporals and one paid lance-corporal, and any or all of the others who are privates may be given honorary rank as lance-corporals or corporals.

Many most capable and highly-trained men are still serving as privates, and while one would like to see promotion more rapid in many cases, it is not permitted to send for a man to come back from abroad to promote him, so that a very good man may be still a private, while another who joined much later is a staff-sergeant. On the other hand several who joined early now hold commissions. While some men may feel they ought to get quicker promotion, there is a very strong feeling of comradeship amongst the members of each section, so that men do not always feel desirous of leaving their friends. In addition it has been so generally recognized that every section has proved itself of real value in the Army that all men feel their work to be useful, and are more concerned to do it well than to think too much of their personal advancement.

On looking back at the first few weeks of our work, I see that with greater experience we might have accomplished much more in our sanitary work than we did, but our inexperience in military methods was not a serious obstacle, because the need for improvement in

sanitation was so apparent that we were welcomed and helped not only by the O.C.'s of units, but by the French civil authorities. Our first task was to clean up and keep clean a fair-sized railway station, where trains of wounded were arriving. One of our men who spoke French fluently took charge of a small party of civilians; these men brushed up the rubbish and burned it in two old steamer funnels which we set at an angle so as to promote a draught. The rubbish consisted of foul bandages, excreta, food thrown out of infected carriages, blood-stained clothing, &c. Others watered the platforms with cresol solution, and others cleaned out and scrubbed a large warehouse which in a few days was used as a receiving hospital. This place looked to us as quite hopeless on account of its filthy condition, but its position was ideal and when cleaned and fitted up it was extremely useful. Meantime we were stencilling notice boards directing men to latrines and urinals, burning horse-dung, and putting up brick incinerators in all the camps.

I inspected the water supply and finding the source, to say the least of it, very liable to pollution, I determined to sterilize all the drinking water possible. There were no tanks in which to do this, nor had we official filtering water-carts, so I decided to sterilize the water in the soldiers' own water-bottles, using a method which is applied to the drinking water in many towns where the water is of doubtful quality. It did not then occur to me to ask leave of the French to sterilize the whole town supply at the water-works, though I believe they would have agreed. The method we used was the following: The men's water-bottles were collected and half-filled with water and placed in rows on long tables. To the contents of each, three drops of bleaching powder solution (2 grm. of bleaching powder dissolved in a pint of water) was then added. After this was added the water-bottles were filled to within an inch of the neck, corked and shaken, and returned to the men after half an hour. This method is very simple, and very quickly performed.

Many cases must have occurred where the regulation filtering and sterilizing water-carts were not available, nor tanks either. In such cases, the officer in charge of each platoon could make up the mixture of 2 grm. of bleaching powder to a pint of water and use it to sterilize the water in his men's water-bottles as above described. I have used small pipettes which deliver three drops with which to add the solution, but a simpler and equally good plan is to carry some clean wood skewers such as butchers use. If these are dipped into the solution and the end quickly held over the neck of the water-bottle, three drops

can be run in off them, and if one pint jar of the solution is made, and four skewers are ready, 100 men can add the three drops to their water-bottles within two minutes. The addition of three drops to the water-bottle, which has a capacity of about 1,000 c.c., is equal to an addition of one part of bleaching powder to one million parts of water. There is no harm in letting four drops or five drops run in, but three are enough except for very bad water.

It is a curious fact that whereas a small excess of chlorine is unpleasant in water, it is much more so when the water is made into tea. There are a few people who are particularly sensitive to the taste of chlorine, and in order that the process of sterilization shall not be disliked and evaded, it is necessary that it should be carefully and exactly performed, using no more than is sufficient to ensure a certainty of sterilizing all the coli-typhoid group, and the water must be left sufficiently long for any residual chlorine to disappear.

I found in the beginning that there were many men who did not like the idea of a "chemical" being added to drinking water, and in teaching the method of sterilizing I have always taken care to explain that while it is quite true that in this process we *do* add a chemical to the drinking water, it no longer exists as such when the water is ready to be drunk, as it has been changed into lime-salts such as exist in most natural hard waters.

In the next town at which we were stationed I was equally convinced that the drinking water ought to be sterilized, and as water had to be carted from stand-pipes to the different units, I arranged to sterilize it in the water-carts (which were wine-barrels placed on carts) at the time when they were filled at the stand-pipes.

To ensure proper mixture and also to make sure that the right quantity is added, it is best to prepare a stock solution over night and bottle it off in Winchester quarts. The quantity I decided to add was 7 gr. of bleaching powder per 100 gallons of water. The water-barrels had a capacity of just 100 gallons. The stock solution was made up to consist of 1 per cent. (1 grm. per 100 c.c.), so that 50 c.c. of this solution were added to the contents of each water-barrel. Subsequently a piped supply was laid on to most of the units and the amount of carted water was then much less. Later on pairs of 500-gallon tanks were fixed at suitable places in the camp and the water was sterilized by adding 250 c.c. of the solution to each.

By arrangement with the O.C.'s of units a notice was placed on each tank prohibiting any one from filling the tanks except the men

in charge of the sterilizing. Each tank had a row of four or five taps for the convenient filling of water-bottles. Billets in the town were provided with small tanks, and the water was sterilized in these by the addition of an appropriate quantity of solution.

There were, however, a large number of troops (and all the townspeople) who were drinking the untreated and unfiltered town water—so I sought and obtained permission to suggest to the town authorities that it would be a wise precaution to sterilize the whole of the town supply. This suggestion was favourably considered by them and was adopted, and the regular sterilization is carried out by their medical officer at the waterworks in addition to the sterilization at the camps already described. It is advisable to continue both sterilizations in order to give real security.

In Base camps in the North of France flies have not been so troublesome as in many other places. The principal point in keeping a camp free from flies is the regular removal of horse-dung. This substance forms their chief breeding ground in temperate climates, and if it can be removed to a distance of a mile or a mile and a half from camp it is not necessary to waste much time on it afterwards. Except with a view of providing exercise for drivers and horses, the carting of it is not so good a method of transport as tramming it away on a narrow-gauge light tram-line such as is used in mining. A convenient gauge is 18 in., and the weight of rails should be 12 lb. to 18 lb. to the yard. Sleepers should be placed not more than 2 ft. apart, and properly packed.

In war time, manure cannot be sold in most places, nor even given away, so it is carted or trammed to a dump. Sometimes it is necessary to burn it, and this is not easy to do when large quantities have to be dealt with, and when the weather is wet. Numerous kinds of furnaces have been tried, but the best is a very simple one, consisting of a cross trench; the trenches are made 12 ft. long, 2 ft. wide and 2 ft. deep. This is dug on the top of the manure dump itself. The cross-trench is then covered with iron bars, such as rails or old piping, spaced about a foot apart. On this are spread sheets of expanded metal, of a mesh about 4 in. by 2 in. Over the part where the trenches cross and on top of the expanded metal is placed a cone or ball of any loose metal, such as bundles of the wires that come on the hay-bales. This cone is to keep the fire open. The best manure, consisting of the driest and longest pieces, is spread over the cone and the expanded metal; above this is placed other manure, and the whole is lighted by holding half a dozen old sacks, sprinkled with paraffin, one at a time on the end

of a manure fork under the surface of the expanded metal in the centre, where the trenches cross. The burning sacks heat the cone and set fire to the manure, which smoulders away. More manure is scattered on the cone and on the expanded metal about once an hour, and at the same time the cone and the expanded metal is knocked with a fork so as to cause the ashes to drop through into the trench. They are removed from the trench by means of a hoe. Manure seldom burns with a flame, and unless it is supported and kept open by some such means as above described, it settles down and packs into a tight mass that will not allow air to draw through.

Again, unless the ash is removed frequently, it deadens the fire by excluding the air. Where there is much manure to dispose of the tram-line should be laid in a circle on the dump with incinerators at intervals on either side of the tram-line. In this way as each truck of manure comes up it can be conveniently unloaded, wholly or partially, at whichever incinerator is ready to take more manure. If the tram-line is over half a mile long there should be points and crossing places to enable empty to pass full trucks. Nevertheless, it is a pity to burn manure at any time, and it would be much better to make a large garden and use the manure to produce fresh vegetables.

Another method of keeping down the number of flies would be to run a large flock of fowls on each dump, because they pick up undigested grains and a great many larvæ. The principal danger of flies in a camp and hospital is the probability of their carrying infection from the latrines to food or to drinking water.

Army latrines in a semi-permanent camp or hospital usually consist of a seat and a bucket, which is seldom covered in such a way as to prevent flies from settling on the fresh excreta and the soiled paper. Attempts have been made to keep flies out of the buckets by cleansing the buckets with cresol or crude oil or paraffin, or to keep flies away from the fæces by having enough cresol-emulsion in each bucket in which to submerge the fæces. In actual practice none of these plans is really successful, as the attraction of fresh fæces for the flies is so great that nothing will keep them away, and even if a quantity of cresol-solution is placed in buckets, as fæces fall into them they form a pyramid, which soon rises above the solution, and flies come to it. In some camps the latrines have been fitted with lids that drop over the seat, but there is still an opportunity for flies to enter in the space between the bucket and the seat, and they do enter, though in much less numbers than if the bucket is left open, as the partial darkness

somewhat checks them from entering the buckets. In most camps the buckets are of different heights, so that the distance between the top of the bucket and the bottom of the seat varies considerably.

The most convenient plan for really preventing flies from walking on the fæces and soiled paper, both during the time the bucket is in the latrine, and also while it is being transferred to be emptied and cleaned, is to make a flat lid with a handle out of any kind of ration box; this slides over the bucket and under the seat. It is covered with a white-wash containing a little bleaching powder on the lower side, and the words "Replace cover" are stencilled on the upper surface in such a position that when the cover is in position the words are visible through the hole in the seat. There are two points that require attention: first it is necessary to convince the O.C. of the unit that the covers are necessary, and serve a useful object. Then he puts it in local orders that they are to be used, and their use becomes a regular routine. Secondly, one has to see that none of the buckets has projecting lugs which prevent the cover from coming down quite flat on to the rim of the bucket. All those buckets in which the lugs (to which the handle is attached) project above the rim should be returned to store and others drawn which do not project. If new buckets are not available a request should be made to the R.E. to allow the lugs to be lowered; this is easily done by cutting off the studs and replacing the lugs an inch or so lower down. Men soon learn to replace the covers, and as there is always one of the latrine men at work near the latrines all day, he can see to the replacement until all the men in camp are used to them. A notice-board should be placed at the entrance of each latrine stating that they are to be replaced directly after use. All latrine seats should be scrubbed daily, the buckets should be washed out after the fæces and urine have been poured out. They are most easily washed by putting them into a tub containing 1 part cresol to 10 parts water, and washing with a brush provided with a handle a foot long. Then they should be allowed to drain upside down, and finally wiped over, inside and out, with a rag dipped in ordinary paraffin. The outside should be kept bright by rubbing with a rag and paraffin, and a little bath-brick or fine sand. Some people prefer to whitewash the buckets outside, but if they are kept bright and clean the zinc coating lasts longer, and the buckets do not leak so soon. A sharp watch must be kept for leaky buckets, and directly they show signs of a leak they should be condemned.

The disposal of the contents of the buckets is best carried out in the

camp if it can possibly be done. Contractors are usually very unsatisfactory; their charges are considerable, and they are very apt to spill contents of buckets, and to employ leaky carts. At the camp I was in they would discharge the contents of their tank carts in unauthorized places. They can be detected in pouring urine on to grass (which they used to do on dark mornings and evenings) by putting a small quantity of confetti in the urine buckets.

The disposal of the pail contents in camps can be carried out as follows: A urine pit is dug about 6 ft. by 6 ft., and filled with layers of burnt-out tins and stones, and a dry brick floor placed over it. A pipe of cresol drums is built into the centre of the pit, the top drum standing a few inches above the brick floor. By looking down into the pipe of cresol drums one can always see what depth of liquid there is in the pit, so as to judge whether another pit must be dug. In the top of the cresol drum which projects from the urine pit is placed a strainer. This strainer is pierced with ¼-in. holes. To strain the mixture of fæces and urine a shovelful of small coal is placed in the strainer, and a bucket of fæces and urine poured slowly on to it. The urine runs through the strainer into the pit, and the fæces and paper remain on the small coal in the strainer. If the fæces are soft or remain soaking in urine for some hours, a certain quantity of them mixes with the urine and passes through the strainer. This tends to choke the urine-pit, and therefore the fæces should not be left soaking in the urine longer than can be helped.

Several different types of furnace have been used to burn excreta. Seeing that fæces contain 70 per cent. of water they cannot be burned easily, except by feeding them a little at a time into a considerable volume of material already raised to a good heat. An early pattern of furnace was one of brick, built in a square at the bottom, and up to about 6 ft. high, and then tapering inwards, and with a brick chimney built vertically above the fire. The total height was about 14 ft. In this furnace the fæces were burned with the aid of camp rubbish, and very little fuel, if any at all. They acted well, but being built of ordinary bricks they did not last long before they developed serious cracks. We were then supplied with small Horsfall furnaces, which had an outer casing of metal and fire-brick lining. If these furnaces are used carefully they work admirably. The two principal points to which attention must be directed are: first, not to let the fire get too fierce, which easily may occur when the camp rubbish is dry and contains substances that burn rapidly; secondly, care must

be exercised in putting in the fæces, so as never to let fæces touch the heated fire-brick lining. The right way to add the fæces is to scrape out a saucer-shaped depression on the top of the fire and tip the fæces into it. If this is done and the temperature of the furnace is properly regulated there is no reason why the fire-brick lining should not last for many months, but if carelessly treated the lining can be injured in a few weeks and the outer casing cracked as well. These furnaces are rather expensive, and their weight makes it difficult to transport them in some cases. Various extemporized furnaces to do similar work have been built in many camps. An ingenious type introduced by Captain Smale, D.S.O., is built up of square tins filled with a mixture of clay and breeze. This is fitted with charging and raking doors made of hammered-out cresol drums and a cresol-drum chimney. When the fæces of the tins inside the furnace are burned away the clay and breeze has burnt hard and retains its position and shape. A furnace has also been built out of sand-bags filled with clay. A brick furnace somewhat on the lines of the Horsfall furnace has been built with a coil of piping introduced at the back of the chimney, so that some of the waste heat is utilized to supply hot water.

Whatever type of furnace is built, both it and the rubbish to be burnt should be protected by a shed. In this shed should be kept the brushes, &c., for cleaning the buckets, and a shelf for the basin, soap, nail-brush and towels for the furnace-men, who should be provided with overalls, extra boots and old uniforms for use at their work. They should sleep in separate tents and their mess-tables separated from other men, and should not be employed in any other way. They are usually paid eightpence a day extra, and should be given frequent opportunities to bath. The work is not so unpleasant as it might seem at first sight, and after men get accustomed to using the furnaces they are able to burn the fæces effectively, and with the emission of very little smell.

Flies congregate in the incinerator sheds, and can be caught there in large numbers, especially in the evening when they come to the warmth and to shelter from cold wind.

Methods of Keeping Down the Number of Flies.

In addition to keeping down the number of flies by preventing their breeding near the camps and hospitals, all means should be taken to catch and kill them in camps and hospitals. There are five methods which can be employed :—

(1) The regular use of a large number of glass or wire-gauze vessels containing a solution attractive to them.

(2) Poisoning with arsenic solution.

(3) Poisoning with formalin solution.

(4) Catching them with a sticky material spread on paper, string, old tent rope or wires.

(5) Killing with "fly-flappers."

(1) *Solutions of sugar or sugar and beer, or sugar and casein,* may be used in these traps, but unless a great many are employed the effect has not much result.

(2) *Poisoning with arsenic solution* is very effective, but is not suitable for indoor work. Some brushes or latrine screens stretched on stakes near a manure heap and sprayed daily with a solution of $\frac{1}{2}$ oz. of sodium arsenite and some sugar in a gallon of water are very effective.

(3) *Poisoning with formalin solution* is useful if the flies have no access to any other liquid, that is in an office or a store-room, where the windows can be closed, or in open rooms in dry weather. If the flies can get out to drink from puddles or water-butts they do not drink formalin mixture. The formalin mixture must be of the correct strength; if it is too strong they will not drink it, and if it is too weak they are not killed by it. The best strength is 1 volume of formalin to 39 volumes of water. Some sugar, treacle or honey is a useful addition. This mixture is placed in tins or saucers on shelves in convenient places. The method is not a suitable one for larders or kitchens or mess-rooms, because the flies do not die in the saucer, but all over the place. The mixture can be used with great effect in dry weather near a manure dump. It can be placed in the shallow tray formed by inverting a cresol-drum. It is poured out $\frac{1}{4}$ in. deep and a piece of bread put in the middle of the pool of liquid for the flies to stand upon; it is often found that the ground is thickly covered with dead flies. Men who mix and distribute formalin must not be allowed to get it on their hands, as it has a very injurious effect.

(4) All the commercial fly-papers are useful and effective, but the expense of using them in effective numbers is great, and the issue of fly-paper is never sufficient to employ freely. An *effective sticky mixture* can be made on the spot, which is quite as effective as anything that can be bought and very much cheaper. It can be applied to paper, string, old tent-ropes or wires. The sticky mixture (or "fly-glue") is prepared by painting a hot mixture of two parts by weight of powdered resin to one part by weight of castor oil on to any suitable material. It

requires a little practice to make the mixture, which is prepared by heating the ingredients in an old saucepan or dixie, and stirring all the time with a stick. This mixture is very stiff and tenacious when cold, but when hot it is quite fluid like ordinary oil-paint. As it is difficult to carry about papers coated with the fly-glue, it is best to apply the glue to wires, for, as already stated, in most camps there is a quantity of wire to be found which comes on the hay-bales. This can be straightened out and cut into 2-ft. lengths. An inch at one end is bent so as to make it possible to hang the wires on nails. The mixture can be kept in tins for weeks or months, but it should not be applied to the wires or paper until they are required for use. The wires are painted with the glue, and then placed in a bucket made out of an old cresol-drum ; 500 wires can be easily carried by a man. They are hung up on nails in latrines, kitchens, larders and food stores; in a marquee they can be hung on a string stretched between the two poles at a distance of 9ft. above the ground. As soon as they are full of flies, or in any case not later than after two days, they are replaced by fresh wires, and the old ones are placed in bundles in a low furnace and burned off. They are then ready to be glued again, and can be used all the season, and then put in a dry place for the following year. In a bell-tent they may be hung with the bent end hooked in each of the three small ventilators; in this position the sticky portion does not touch the tent, nor the poles, nor the men's heads or clothes. There is nothing in the mixture which is attractive to flies, but they like to settle on anything which is dark and narrow, and hanging down.

I experimented by adding to the mixture honey, sugar, a fresh fish flavour and a stale fish flavour, but on trying wires so prepared alternated in rows with others coated with the usual mixture I found the last mentioned caught as many flies as those coated with a flavoured mixture. One man can coat about 8,000 wires a week, and the cost of the resin and castor oil is not more than two shillings a thousand. One advantage attaching to the fly-wires is that they do not flap about in a draught as do fly-papers.

If it is desired to apply the fly-glue to paper, a convenient plan is to punch a hole in some hundreds of strips of paper, the strips being old newspapers cut 3 in. wide, and then to lay a strip on a board which is 6 in. longer and twice as wide as the strips, and paint the fly-glue on to the strip—the next strip is laid on top and painted, and so on, until as many are done as are wanted. Then they are all carried on the board to the places where they are to be put up, and peeled off one at a time, and hung on nails.

(5) A convenient "*fly-flapper*" which answers as well as the wire flappers sold in shops, can be made out of a piece of old canvas or tarpaulin attached to a stick about 2 ft. long. It is best to make a number of small holes in the flap, so that the fly does not escape a blow by being swept away by the movement of the air caused by striking a blow. If a number of men use these flappers for a few minutes every evening, when flies are settling for the night, a great many can be killed in this way.

Drying of Clothes.

If a camp is going to be occupied for more than a few days, it is worth while putting up an arrangement for drying men's clothes. This may be done out of materials which are available everywhere. A pipe is made of cresol-drums by taking out the tops and bottoms, and slightly tapering one end of each, and driving one into the other so as to get a pipe about 20 ft. long. This pipe is supported 2 ft. off the ground on small piers built of old bricks or stones; a simple furnace is built at one end and a chimney at the other. Ordinary camp rubbish, leaves, twigs, &c., are burned in the furnace, and a light wooden framework is constructed 1 ft. above the pipe. The wet clothes are supported on this framework; 4 ft. above this another framework is built, and a tarpaulin stretched over it to keep the rain off. This is an arrangement similar to that which is used at most mines for drying clothes. It is customary at a mine to get an old boiler tube about 2 ft. in diameter and make a fire at one end, tilting the tube to promote a draught. Such a tube is not generally available in a camp, but the cresol-drum pipe with a furnace at one end and a chimney at the other forms an efficient contrivance.

Uses of Cresol Drums.

Cresol drums, which are always available in large numbers, are very convenient for the making of surface drains. By piercing a number of holes (which is easily done with an entrenching tool) the drums are converted into pipes which are laid in ditches alongside the paths in camps and hospitals. If it is necessary to provide for ambulance or lorry traffic over the drain, it can be made to stand such traffic by making holes in both ends of the drum and taking out one end. The drums are then packed tight with large pebbles and the end put back. They are laid tightly, so as to end in a trench which just fits them, and so that there is 6 in. space above them. This space is rammed

with road-metal. Water will run through such a drain almost as freely as if it were an open pipe. In the course of time sand and mud will accumulate in the drums, then the layer of road metal is dug up and the drums lifted out and stood on end. A bucket of water poured through washes out the sand and they are again replaced in position.

Cresol drums are useful in many other ways. They can be used as braziers for charcoal or coke by punching a few holes round the side near the bottom and standing them on three bricks. With the top taken out and a rope handle fitted to them, they make useful buckets; for warming tents at night a cresol drum full of boiling salt water placed against the pole is convenient and safe.

We have an exhibit of the various sanitary appliances to which I have referred on a piece of ground in Manor Street, King's Road, Chelsea, directly behind the Chelsea Public Baths, and I have the authority of the Officer Commanding the First London Sanitary Company to invite any members of this Society to visit it, with their friends, at any time that is convenient to them.

Major FREMLIN, R.A.M.C., gave a demonstration, illustrated by diagrams, of the work of the First London Sanitary Company overseas. The diagrams served to explain many of the various improvised appliances made by the Sanitary Sections in France, Belgium, Egypt, and Salonika, and were drawn by members of the Company.

DISCUSSION.

Lieutenant-Colonel S. MONCKTON COPEMAN, R.A.M.C., F.R.S.: I desire in the first place to thank Captain Moor for his most interesting paper; and also Major Fremlin for the subsequent demonstration. I can speak with special knowledge as to the good work done by Captain Moor since his return from France, as in connexion with the instructional work now being carried out at the Royal Army Medical College, officers from the various commands are afforded opportunity of attending practical demonstrations on the exhibit of improvised sanitary appliances which Captain Moor has been largely instrumental in providing at Chelsea. In reference to that portion of the paper which deals with the destruction of flies, while appreciating the ingenuity of the methods suggested, it is, I think, of importance to bear in mind that the only plan likely to be of real service in this connexion is to devote special attention to the question of preventing the *breeding* of flies, and in order to attain this object it is not by any means necessary to remove all horse-manure to a considerable distance from the camp. Even with the help of a narrow-gauge tramline, as advocated by Captain Moor, transport of the material for a distance of about a mile involves an expenditure of time and labour that may be more profitably utilized in other directions. For, as I have demonstrated, by employing

a method of "close packing" the manure[1] on a prepared site, adjoining the stables if possible, the high temperature resulting from fermentation of the compacted heaps suffices to kill any eggs or larvæ of the fly that may be present. Moreover, the value of the material as a fertilizing agent is but little impaired. If, concurrently, every precaution be taken, by appropriate methods, to prevent flies obtaining access to human excreta or food, necessity should hardly arise for the use of methods aiming only at the entanglement or poisoning of the adult insect. The burning of manure should be avoided as being wasteful and unnecessary. Captain Moor has not discussed in his paper the question of the treatment and disposal of the sullage water of camps. But as this subject has so often presented great difficulties in the past, the perusal of the results of some recent experimental work after their official publication may be found useful. In conclusion I beg to thank Captain Moor for the help he has always so readily afforded me.

Mr. A. BACOT (Lister Institute): I entirely agree that the method of close packing of horse-manure described by Colonel Copeman is the best general means of preventing the breeding of flies in camps. I should like to draw attention to the danger arising from the presence of old cardboard or wooden boxes and large tins in manure dumps or large heaps of mixed rubbish. I have found that flies (*Musca domestica*) will feed, pair and deposit eggs in the dark. These insects can live and breed freely in closed card jars of pint capacity kept in a warm humid incubator. It follows from this that there is more than a possibility that adult flies reared in fermenting heaps may find their way into spaces such as are afforded by empty receptacles buried in rubbish, and continue breeding when the climatic conditions render it impossible on the exterior. A very general defect in fly-spraying fluids with which I have experimented is that while the spray knocks over the flies it touches, and renders them incapable of flight, the majority of healthy flies are not killed if the temperature is as high as 75° to 80° F. If a cage containing flies that have been stupefied by spraying at 50° or 60° F. be placed in a warm atmosphere a large proportion speedily recover. This drawback I find can be remedied by adding a solution of a kerosene oil and soap emulsion to fly-spraying fluids which fail to kill at "summer heat." The soap-oil emulsion is very fatal to flies, but does not kill them immediately on contact; if, however, it be added in suitable proportions to any spraying fluid which speedily knocks them down, it obviates the danger of their recovery.

Lieutenant-Colonel H. R. KENWOOD, R.A.M.C.: I am glad of the opportunity of complimenting Captain Moor and Major Fremlin upon their interesting and instructive paper and demonstration. I have witnessed some of the improvisations alluded to in operation in France and Belgium, and can testify to their practical worth. I have often felt that in these simple expedients to meet Army needs there is much that should prove useful and suggestive to the practical civil sanitarian in rural districts, and that he would do well to make himself practically acquainted with them. With reference to the fly danger, the measure which always appeals to me as of prime importance is that which

[1] *Lancet*, 1916, i, p. 1182.

aims at preventing them from gaining access to human dejecta, and to the sick from communicable disease, and as precautions to this end can never be complete, the careful guarding of all food from their contact is included in this measure. But with this essential precaution of *protection* should be combined that of *prevention* (from breeding), and even *destruction* may be called for, especially when the camp has within a mile many breeding facilities which are not under military control. Among the various methods which have been suggested and applied for keeping down the fly population in camps, that of Lieutenant-Colonel Copeman seems to me to be the most simple and practical when transport is difficult or impossible to obtain, as is so often the case. The fat which, despite grease-traps, gets down to the filter beds in standing camps is, in my experience, a great handicap to the efficient action of purification plant; and anyone who can devise a simple, easily workable means of more completely separating and removing it, will confer a real service. In my opinion it is the fat in ablution and bath waters which is responsible for most of the trouble.

Captain P. HARTLEY, R.A.M.C. (First London Sanitary Company): I have had no experience of the work of a Sanitary Section at a Base Camp in France, but have been in command of a Sanitary Section attached to a division in the field for the past ten months. The conditions are, of course, very different in the two cases, but members may be interested to know something of the work being done by Sanitary Sections at the Front, and to hear of some of the difficulties which are encountered from time to time. To ensure a safe supply of drinking water for troops, R.A.M.C. orderlies are posted at the authorized water supply stations; the supplies are tested daily by N.C.O.'s of the Section and the orderlies add the requisite amount of bleaching powder to each water-cart before it leaves the station. At present only one Horrocks's water testing cabinet is provided in the equipment of a Sanitary Section; this is a most useful piece of apparatus and it would be a great advantage if we were provided with three or four for our work. One of the most difficult and important sanitary problems at the Front is the disposal of human excreta. They must be either buried or burnt. The latter is by far the safer and more satisfactory method of disposal, but there are occasions when incineration is impracticable, and burial—with the possibility of endangering the water supplies—is the only alternative. Wherever possible, fæces are burnt, and to carry this out successfully with all units in a division is a problem of some considerable magnitude. It is essential that well trained and intelligent orderlies should be employed, that solid and fluid excreta should be separated before the incineration of the fæces, and that a suitable incinerator should be provided for the purpose. It has been found that in medical units, such as Field Ambulances, incineration of fæces is carried out in a satisfactory manner, but difficulty is often experienced in burning the fæces from other units of a Division. Bad stoking is perhaps the most frequent cause of failure. There are many types of incinerators which will burn fæces, but I do not think the type used is so important a factor of success as proper stoking and intelligent supervision by the orderlies. As regards the fly problem, it is most important to deal with breeding places, particularly manure and refuse. In one place

where the division was stationed for some months during the summer, a manure dump and a refuse tip were established, and all manure produced in horse lines was carted away daily to a dump, where it was stacked. Difficulty was frequently experienced in dealing with civilian manure and with manure from Army horses stabled in premises belonging to civilians, since in this latter case the owners of the premises are entitled to the manure. Fly-papers were issued during the summer and hung in messes and kitchens; I also recommended that one fly-paper should always be hung in the meat safe, to catch any flies which gained access while the doors were opened—1,200 fly traps of a very good type were issued to the Division, as well as seventy syringes for spraying purposes. Several thousand "oil-resin" fly wires, similar to those recommended for use by Captain Moor, were made, and were found to be very effective. They are cheap, very easy to make, and readily renewed. They were hung in permanent latrines and in this way large numbers of potentially dangerous flies were caught. Billet refuse was collected in sacks hung in billets, and the contents taken once or twice daily to the incinerators and burnt, and the sacks returned to the billet. Civilian refuse was dealt with in the following way: barrels labelled "Detritus" were placed near civilian houses and the occupants were requested to place all their rubbish in the barrels; the rubbish was taken away daily in carts to the refuse tip and burnt. I have seen a small plant, similar to the one described by Colonel Copeman, for dealing with soapy water from baths and laundry. After clarification with bleaching powder, the water is passed through a filter of coke and sand. The final effluent is perfectly clear, and as it generally contains free chlorine, it is free from pathogenic organisms, and can either be used again or run away into the streams or drains. There is an inevitable wastage of personnel in Sanitary Sections at the Front, and we hope that the reinforcements supplied will be of the same quality as the men they replace. The work now being done by Sanitary Sections at the Front is of extreme importance, and it is most important that men of good education and special qualifications should be enlisted, and all should receive the special training in Field Sanitation at the Company Headquarters before being sent for service abroad.

Captain MOOR, R.A.M.C. (in reply): I must express my thanks for the criticism of those who have kindly taken part in the discussion. I entirely agree with Colonel Copeman's view that it is necessary to deal with flies at their breeding ground, but it is also essential to deal with flies in the Camps, owing to the fact that many of their breeding grounds are impossible to control. I consider that *all* the known means of their extermination should be employed. Simultaneously and persistently, I have specially drawn attention to the necessity of the utilization of manure in growing vegetables, and the utilization of waste food in the production of fresh eggs and fowls, both from the hygenic value of fresh foods for the sick and wounded, and as an economic means of combating the world-shortage of food during war-time. I would also point out the great value of light portable tram-lines for the transport of manure and any other material, on account of their cheapness in prime cost and in working and the ease with which they are laid, moved, and relaid as required.

Section of Epidemiology and State Medicine.

President—Dr. G. S. Buchanan.

(*November* 24, 1916.)

(Chairman—Sir Shirley F. Murphy, Lieutenant-Colonel R.A.M.C.)

The Epidemiology of Cerebrospinal Fever.

By W. H. Hamer, M.D.

As the wanderer through cold mists on the fells may be gladdened by a penetrating gleam of sunshine, opening up warm glimpses of distant woods and fields, so was I rejoiced, some weeks ago, on encountering, in a somewhat chilling environment, the Junior Honorary Secretary of this Section glowing with epidemiological enthusiasm. For a few brief moments there was revealed over the hills and far away a sunlit world of practical endeavour, as to the actual existence of which I had become altogether unmindful. Then came the reaction and mild voicing of certain difficulties and fears, but I was assured that epidemiology was not yet, as I had gloomily supposed, finally destroyed, no, not even when the healthy carrier hypothesis was formulated. Your Honorary Secretary's vigorous brushing aside of all objections and difficulties put me in mind, inevitably, of the prince who battled with the briars which surrounded the enchanted palace, and I was filled with hope, that, as the good sword of the new statistical methods was in his hand, he might hack a way through and awaken the Princess Epidemiology. I remembered, in the fairy story, how all those who lived in the palace had become spellbound, just on the instant, retaining the very attitude appropriate to the action on which they were intent when they were enchanted. In particular when your Hon. Secretary urged me to read a paper before this Section, two figures recurred to my mind, those of the cook whose hand was

raised, and of the scullion whose ears were about to be boxed, and I could not but compare my own case to that of the scullion, and reflect that any stirring of activity on my part, if it had any influence at all upon the situation, would assuredly be likely to result in corresponding activity of the upraised hand of some expert, who, though epidemiologically speaking spellbound, was nevertheless still fully proficient in all the arts of the confectionery of the laboratory. Your Secretary, however, continued to urge that it is the duty of every one in or about the old palace to do his bit, and I have hence thought again and again of his exhortations, of the horror of the enchantment, and of the intensely rapid growth of the briars; though, indeed, if the fairy story is to be given its modern application no mere briars are here in question, but luxuriating causal schizomycetes, reproducing themselves with inconceivable rapidity, strain upon strain, paraforms commingling with pseudoforms, all stimulated by enrichment media to hitherto unheard-of development, crowding out opsonins and phagocytes, entering into close alliance with aggressins, and withal manifesting ability to undergo mutation to such an extent as to make even sober-minded and philosophical bacteriologists begin to wonder whether it is not really time to set about doing a little weeding. The outcome of this nightmare has been that I have tried to put together a record of the impressions, fondly believed by your Hon. Secretary to be the waking impressions, aroused in the mind of an enchanted scullion, and I have entitled this essay "The Epidemiology of Cerebrospinal Fever." I must admit, however, that I have been seriously handicapped all the time by thinking somewhat ruefully of the hand of that enchanted cook.

This subject was, of course, discussed at two meetings of the Section less than two years ago: an epidemic was then in prospect, it is now in retrospect; the point of view has certainly changed, and the design of this paper is not, as it might conceivably have been at an earlier date, to advance any special claims on behalf of any one of the four strains of meningococcus, or of the three (alpha, beta and gamma) varieties of the parameningococcus or of the pseudo-meningococcus, or of any reputedly non-pathogenic cocci indistinguishable from the aforesaid; nor do I even hold a brief for the healthy carrier hypothesis; it is sought rather to ascertain whether any facts throwing light upon cerebrospinal fever can be discerned from study of its recent behaviour. I hope to show, to begin with, that it has occurred in this country again and again, probably for many centuries, and certainly since the period of the remarkable outbreaks which accompanied the English sweating sick-

nesses of 1485-1551. I trust you will accept Sydenham's account of the new fever of 1685, as depicting a cerebrospinal fever outbreak in the London of 230 years ago. You will, at any rate, agree that posterior basic meningitis killed children long before Gee and Barlow first introduced that disease into the nomenclature, and you will doubtless accept the view that these forms of meningitis and cerebrospinal fever themselves appeared and disappeared, much as they do now, long prior to the time of Weichselbaum, and thus before the initiation of recently introduced methods of attempting to control the course of epidemics.

To the healthy carrier doctrine reference must, of course, be made, because of its bewildering bearing upon practice. Some years ago, when the hypothesis was as yet in its infancy, I remember a former President of the Epidemiological Society urged that the only satisfactory solution of the difficulties which he clearly saw must arise, was to be found by recommending "segregation of the sound." Now we know, thanks to improved bacteriological methods, that no one is sound, and the administrator, therefore, is perforce content as a rule to dilute the ardour of his zeal with a certain amount of cool discretion. At the time of writing this, however, reports are forthcoming concerning the poliomyelitis of New York, where more than 2,000 cases were notified during one fortnight of last August. Doubtless many were reported in error, for there was coincidently a considerable decline in summer diarrhœa, and notifications of cerebrospinal fever were at a much lower ebb than in this country, but none the less a London paper has advocated the placing under surveillance of adults newly arriving from affected American cities, and it has even been suggested that restrictions should be imposed upon a London nurse who had been in contact with a child suffering from infantile paralysis. It would appear worth while, therefore, in face of panic proposals of this kind to sum up the results of recent experience gained with regard to segregation of contacts. I recollect some years ago at a discussion on serum therapy, held under the auspices of the Royal Society of Medicine, we were urged to begin by considering not the maladies to which serum therapy was generally believed to be applicable, but some of those to which it was almost universally held not to be applicable. In our modern dealings with what the Americans call "well persons" there is a tendency to proceed on similar lines. If there be anyone here, in whose mind there still remains any scintilla of doubt as to the advisability of segregating healthy carriers of say the staphylococcus or streptococcus or

Bacillus coli, I hope to be able to fan that spark into flame, and to induce the waverer to give further consideration to the question as to whether the segregation of "well persons" is likely to have any useful influence whatever in controlling the spread of cerebrospinal fever or poliomyelitis.

In tracing the history of cerebrospinal fever the most striking characteristic of the epidemic prevalences is, undoubtedly, their tendency to occur just before or after, or coincidently with, other forms of widespread epidemic manifestation, and especially with influenzas, sweats, "trousses galants," and catarrhs. The earliest clear accounts of association of meningitis with epidemic catarrh are those given by Hecker. Further light upon the subject is forthcoming, moreover, in Creighton's "History of Epidemics in Britain." In the Milroy Lectures of 1906 I made an analysis of Hecker's data, and this shows how encephalitis in Germany, a putrid fever accompanied by phrenitis (the so-called Hauptkrankheit) in Westphalia, Hesse and Friesland, and epidemic cerebritis in France, shortly preceded in the countries named, the first sweating sickness of 1485 in England; similarly encephalitis, putrid fever, and a fever (said to resemble the febrile cerebritis above referred to) immediately preceded in Germany the second outbreak of sweat in England in 1506; a great influenza prevailed over Europe in 1511; then later encephalitis (Hauptkrankheit) again appeared, this time coincidently with the third English sweat of 1517, and upon this Hecker remarks, "We believe, therefore, that we are fully justified in classing the epidemic described to have taken place in Holland and Germany in 1517 with the influenzas, and in declaring the morbid commotion in human collective life, which thus manifested itself, to have been a forerunner of the English pestilence"; when the fourth English sweat prevailed in 1528 it "spread to the Continent and to Ireland," a "trousse galant" appeared in France, and a "pestilential spotted fever destroyed the French military youth before Naples"; coincidently with the fifth sweat, of 1551, there were prevalences of malignant fevers, and of "influenza" in Germany, Swabia and other parts of the Continent.

A remarkable account of brain fever, from the point of view of the relationship of certain forms of a disease so designated, occurring epidemically and in association with epidemic catarrh or influenza, is contained in the chapter entitled "Encéphalite," or "Fièvre cérébrale," in the second volume of J. A. F. Ozanam's "Histoire médicale des maladies épidémiques" (Paris et Lyons, 1835). He writes (p. 118)

of this epidemic meningitis, as we may surely term it: "Elle se combina, en 1557 et 1559, avec l'épidémie catarrhale qui parcourut successivement l'Allemagne, la Hollande, la France et l'Espagne; elle fut mortelle pour les enfants." The epidemic affected Italy too and, later, other parts of Europe. He adds, "La céphalée se combina encore, en 1580, avec l'épidémie catarrhale qui parcourut toute l'Europe." In 1588 there prevailed, in canton Bâle, "une céphalée maligne que l'on nomma hauptwehe," and an account is given of a military outbreak in 1616. Then follows a description of the "new disease," described by Willis in 1661, in which post-mortem examination showed "épanchemens séreux dans les cavités du cerveau dont les vaisseaux étaient légèrement injectés." Ozanam does not, indeed, refer to Sydenham's account of the outbreak, in London in 1685, of a "new fever," which I brought to the notice of the Section two years ago,[1] an epidemic disease in which cerebral symptoms commonly supervened upon fever and catarrh, and in which purple spots were frequently noted. Ozanam passes on, however, to describe an outbreak of "encéphalite" at Aumale, in 1757, and to refer to another epidemic reported upon by Saalmann in Westphalia in 1788, and styled by him "phrénésie et paraphrénésie, par rapport à l'inflammation des méninges et du cerveau." Ozanam then proceeds to consider the classical outbreak at Geneva described by MM. Vieusseux and Mathey. This last-named epidemic is, of course, the outbreak which has been universally accepted by all modern writers as one of cerebrospinal meningitis; but it is, indeed, curious that the earlier history has been ignored, particularly having in view the fact that Ozanam treats Vieusseux's epidemic as only the last of a long series.

On reaching the nineteenth century the summary of Hirsch is available, and it is often referred to as if it comprehended the whole of the known history of the epidemic disease. Hirsch states that the "prevalences" group themselves into four periods, and these, with the recent outbreak, make five in all. It is interesting to observe that, if the years which stand out in Hirsch's Chronological Survey (i, p. 7) as those of widespread epidemic prevalence of influenza are taken, and an interval of some two to four years added on to the dates specified, the then succeeding periods of ten to fifteen or twenty years are the years of Hirsch's "prevalences," and of the latest prevalence of cerebrospinal fever. Thus:—

[1] *Proceedings*, 1915, viii, p. 77.

Pandemic influenza (Hirsch's Chronological Survey)	Hirsch's four "Prevalences" and the fifth prevalence of cerebrospinal fever
1802-03	1805-15
1831-33	1837-50
1848-51*	1854-75
1873-75†	1876-86
1889-90	1893 and onwards

* There was wide diffusion of influenza in 1857-58 also.

† This prevalence was particularly well marked in the western, but also affected the eastern hemisphere; it did not make a very deep impression on death returns in Great Britain. The following prevalence of cerebrospinal fever was, in contrast to Hirsch's three preceding prevalences, but little marked in the western hemisphere, but it is recorded to have affected Birmingham, Glasgow, and Dublin in these islands, and to have involved also various European countries. As will be seen later, if a community suffers severely from influenza, it is apt to escape lightly in the succeeding cerebrospinal fever prevalence, and vice versa.

A review of the literature of influenza and cerebrospinal fever makes it clear that the phenomena presented in these diseases need to be examined from two standpoints—first from that of the communities, and second from that of the individuals, affected. As regards the former, the pandemic or major waves of influenza spread over the world at intervals of from fifteen to thirty years; the "trailing epidemics" or minor waves follow one another, generally speaking, roughly at yearly intervals, being favoured by waning of the protection afforded by previous attack and precipitated in large measure by seasonal influences —these "trailers" may be compared to "ripples upon the surface of the ocean swell" of the major waves. At the crests of the major waves, there is a marked tendency for a certain number of cases with nervous complications to manifest themselves, but the development of *prevalences* of cerebrospinal fever is encountered only in the intervals between the crests of the major waves of influenza, and these developments are favoured by the same seasonal influences which promote the development of "trailers" of influenza. Thus, as Chalmers pointed out to this Section two years ago, cases of cerebrospinal fever had been recognized in Glasgow almost from the beginning of the pandemic outbreak of 1890, but the widespread prevalence of cerebrospinal fever was not developed in Glasgow until 1906-07.

Turning now to the other standpoint, that from which study is made of the way in which these two diseases react upon particular individuals; the pandemic waves of influenza, sweeping as they do over communities in some six to eight weeks, exhaust the available susceptible material to varying extents; they do not affect the entire population, even in the large cities, and, in the country, great numbers of people escape

altogether. Those who are attacked acquire a temporary immunity, but they are apt to suffer again in course of time, as their protection fades away, and on the development of the "trailing epidemics"; the form assumed by the malady in the individuals attacked in the "trailers" is never, however, quite that presented by the majority of sufferers in the pandemics—the knock-out character of the invasion is much more rare and, on the other hand, complications (respiratory, gastro-intestinal, and nervous) are far more common. Thus the reaction observed at different phases of the major wave, between the influenza poison and the unprotected member of the affected community, is by no means constant. Just as the community's resistance varies, so it must be realized that the apparent intensity and the very mode of attack of the poison unquestionably exhibit noteworthy variations at differing points of time—for example, on the crest of, or in the trough of, a wave.

There seems good ground, therefore, for suspecting that the epidemic form of cerebrospinal fever may represent the particular type assumed, on the development of a "trailing epidemic" of influenza, in certain individuals, belonging to communities which have already suffered more or less severely in a preceding pandemic outburst. Such a suspicion is confirmed when it transpires that the individuals who are the subjects of cerebrospinal fever, in these outbreaks, are not those who have already suffered from influenza, but are, as a rule, the young children, recruits, immigrants, and recent arrivals from country districts, who have newly joined the affected community and who have not acquired any sort of previous protection. In the light of this consideration the varying manifestations of influenza and cerebrospinal fever in, say, London or New York on the one hand, and in areas comparatively removed from the great centres of communication and traffic, on the other, find quite simple explanation.

Whether such an interpretation of the facts be accepted in its entirety or not, at any rate the available epidemiological literature affords ground for regarding cerebrospinal fever as a disease which simultaneously affects large parts of the inhabitable globe, and the history of which stretches back into the past as far as any records describing the particular kind of symptom-complex in question are traceable. The malady is thus clearly marked out as being within the purview of this Section, the objects of which have been defined as having relation to the "occurrence, persistence, and variations in type or character of epidemic disease."

The point of view I have outlined is not, however, it must be

admitted, universally accepted; indeed, it is still not infrequently assumed that cerebrospinal fever dates from 1805, in which year it has been seriously suggested that the meningococcus sprang forth, fully armed, by mutation from the gonococcus. The meningococcus itself, be it remembered, was not described until 1887. We are told by two latter day bacteriologists that "whether the disease is an entirely new one, or has always existed, is a matter largely for antiquarian speculation." This is the kind of remark that makes one wonder whether the Princess Epidemiology really is dead or whether, peradventure, she only sleeps an enchanted sleep. Apparently the reason for dating the origin of cerebrospinal fever from 1805 only, is just that a particularly clear account of the outbreak of that year was given by contemporary writers at Geneva, but if this line of argument is to be accepted as valid, diphtheria must have originated at the time of Bretonneau, typhoid fever at that of Jenner and Stewart, and so on. If epidemiology is to be allowed any say in the matter at all the literature certainly justifies the conclusion that the disease is no new one.

The question of diagnosis of cerebrospinal fever outbreaks is now, of course, greatly complicated by bacteriological considerations, which have led in the last year or two to the concentration of attention upon very delicate and difficult laboratory tests, with a perhaps not altogether unnatural tendency to promotion of a feeling that the outbreaks of pre-bacteriological days must be regarded as being wrapped in complete mystery. Two modern writers, Dr. Foster and Dr. Gaskell, remark, however, that "the apparently enigmatical march of the disease in the old epidemics (by old, is apparently meant those of the nineteenth century) acquires a fresh interest and meaning when an attempt is made to trace the path of long past carriers." Thus they find cerebrospinal fever at Geneva in 1805, and in Massachusetts in 1806, and they say, "as to whether any emigration from Switzerland took place there is no evidence, but there has always been interchange between Geneva and North America." Again, an "Odyssey" of *carriers* explains the march of the disease from the Pyrenees to the Rhine in 1839-40. Yet, again, "the disease appeared at Bardney, in Lincolnshire, in 1867. In connexion with this apparently isolated outbreak it must be remembered that farmers near the recently reclaimed fenland were in the habit of employing gangs of reapers from Ireland, and that this may have been the method by which the infection was imported." Here then are two points of view; we find ourselves in fact at the parting of

the epidemiological and the neo-bacteriological ways. The difference is as great as that which distracted the two great empires of Lilliput and Blefuscu. On the one hand the "Big-endians"—or epidemiologists—by preference deal with outbreaks of disease on a large scale; the "Little-endians," on the other hand, approve a theory which has been carefully worked out by applying it to material obtained from isolated individuals and submitted to an exceedingly elaborate scheme of laboratory testings. The "Big-endians" claim that there is a noteworthy relation between the world movements of epidemics of influenza and those of cerebrospinal fever. The "Little-endians," taking on the other hand Geneva and Massachusetts, the Pyrenees and the Rhine, or Dublin and Bardney, claim that they have evidence which can be utilized for drawing deductions of quite a different character. Epidemiology is, of course, intimately concerned with the former findings, but is at a loss in dealing with the latter. The admittedly imperfect character of the data is, in the former case, compensated for by taking the broad view; the imperfection is rendered particularly glaring in the latter by taking a narrow one.

When Hirsch refers to outbreaks in Geneva and Massachusetts, he means to say they were *recorded* in these places. Epidemiology reads into this, in the light of later experience, very widely diffused prevalence. Similarly the disease may have gone from the Rhine to the Pyrenees, or from the Pyrenees to the Rhine, or appeared practically simultaneously in both localities, in 1839. And, again, the evidence for association between Dublin and Bardney, in 1867, seems, epidemiologically speaking, unduly pressed, for, judging again in the light of later experience, the disease *recorded* in Dublin, in 1866-67, doubtless also affected other places in Ireland, and it is almost unthinkable that Bardney was the only part of England invaded at that time. Bruce Low in his paper read before the Epidemiological Society in 1899 told us that cases were reported during 1866 in London and in Rochester, and he reminded us that even in England in 1899, this particular malady was "frequently overlooked." Why then assume that Bardney must necessarily have derived its infection directly from Dublin? I have referred to the instances cited because they illustrate differences of point of view which are of fundamental importance if bacteriologists and epidemiologists are ever to arrive at an understanding with regard to cerebrospinal fever.

A very instructive case, which may be mentioned here, is one which has been made the subject of careful investigation by Surgeon-General Rolleston, and it is also commented upon by Dr. Mearns Fraser, in his

annual report for Portsmouth, 1915. A private at Eastney barracks developed cerebrospinal fever on January 15, and it was suggested that the infection was introduced from Canada. On analysing all the available information it was found that if any connexion between the Canadian and the Eastney epidemics was to be maintained, "it must be assumed that there were at least two undetected carriers." On the other hand Surgeon-General Rolleston points out that "the almost simultaneous outbreak of cases in other parts of the country, and the weakness of the assumption of the hypothetical carriers, make it probable that the epidemic was due to some undetected chronic carrier." Dr. Mearns Fraser adds that "cases broke out at almost the same time in other parts of the borough, and at this date London and many other parts of the United Kingdom were also involved." In the light of this knowledge the attempt to trace infection in each instance to a hypothetical healthy carrier seems quite a work of supererogation. The medical officer who is confronted with the new demand that each outbreak shall be traced not merely to importation of infection, but to such introduction of mischief, not by affected persons but by "well persons," may well cry out in desperation, "O that way madness lies, let me shun that."

It may be added that it is sometimes stated that the United Kingdom "enjoyed a marked relative immunity" throughout the nineteenth century. Now, how is this compatible with the admitted risk of infection from Geneva or from Ireland, and from numerous other places, the presence of mischief in which must have threatened the United Kingdom as a whole far more seriously than it menaced either Massachusetts or the individual village of Bardney, in Lincolnshire? Epidemiology frankly admits the imperfection of its records, and does not attempt to track healthy persons from China to Peru; it recognizes, moreover, the fact that almost the whole inhabited globe was involved in each of these epidemic prevalences. Despite the imperfections it does, however, claim that, taking a broad view of the records of cerebrospinal fever, they group themselves into periods, and these show a special relationship to pandemic influenza, a relationship which would be fully explained if it were premised that cerebrospinal fever is the form in which the morbific influence is especially apt to manifest itself epidemically in certain persons—namely, those unprotected individuals who are attacked in the "trailing epidemics" which follow after the pandemic prevalences. It may, at any rate, be argued that the "enigmatical march" of infection in the old epidemics affords

no reason whatever for assuming that "healthy carriers" of meningococci, to the exclusion of actual sufferers from one or other of the manifestations of disease exhibited in these epidemics, have played any special part in promoting the spread of cerebrospinal meningitis.

Foster and Gaskell's last word on this aspect of the subject is, it is worth noting, that "It is not necessary (therefore) to attempt to trace a direct spread from any particular epidemic, since the matter is more a question of the occurrence of the appropriate conditions than of the introduction of an extraneous infective agent."

Finally, on this question of history, it may be remarked that the writers quoted, after detailed reference to recent very fatal prevalences on the Gold Coast, East Africa, &c., agree (p. 11) "that the disease is more widespread in tropical countries than is generally recognized," a view which may be contrasted with their feeling the need for linking up Geneva and Massachusetts, and Ireland and Bardney, in previous epidemic prevalences.

There are certain epidemiological features characterizing the outbreaks described by earlier writers, which are abundantly confirmed by almost all observers of the recent prevalence. The description of these given by Foster and Gaskell in their monograph is, fortunately, available, and certain supplemental observations may be added.

(1) There is "the disconnected way in which the bulk of the cases occur," or, as they say elsewhere, there is the "little apparent connexion between them." They add the further observation that "the number of nurses and medical attendants who have contracted the disease is small."

(2) "Two types of community appear to be especially susceptible" (to epidemic prevalences), "children in crowded town areas, and troops." They add, that the disease occurs "more especially among newly-formed units"; and, again, in communities which are "rapidly changing," for example, in the population of New York with its "constant stream of immigrants from Europe."

(3) "The most important factor in bringing about the spread of the disease is overcrowding."

So far there is complete agreement, but now comes a striking difference between older and later observers, who take quite different views concerning the relation of the disease to influenza. The earlier observers, as has been already noted, for the most part assume that cerebrospinal fever is part and parcel of the epidemic manifestations of influenza. At the other extreme the observers of the recent prevalence are particularly careful to discriminate and distinguish. Thus some say,

the outbreak coincided in point of time with an outbreak of influenza, but they are careful to add, this was doubtless the result of chance; or, alternatively, such an attitude as that expressed in the following quotations, is adopted. "It is not uncommon to find that some comparatively slight illness has preceded the acute onset of cerebrospinal fever, such as influenza or sore throat." . . . "The diagnosis of early cases of cerebrospinal fever from influenza presents some difficulty." . . . "Mild cases of cerebrospinal fever may be unrecognized, and classed as influenza, but no proof exists that the former disease is ever so slightly marked as not to develop at least some of the diagnostic signs."

Here, then, again, are the epidemiological and the neo-bacteriological points of view, and the last quotation clearly sets out the distinction between them. On the one hand the disease may be regarded as a complication or sequela of influenza, it being assumed that owing to special circumstances some individuals, when attacked by influenza, develop cerebrospinal fever, and thus to the infection of influenza is superadded the complication of involvement of the central nervous system. On the other hypothesis cerebrospinal fever and influenza are held to be quite distinct diseases; if cerebrospinal fever appears at all it is developed quite characteristically, and in order to explain sporadic occurrence of the cases, it is assumed that apparently healthy persons may transmit the causal organism. Curiously enough, while it is maintained that at least some of the diagnostic signs must be present in cerebrospinal fever (*see* above), it is quite naïvely assumed as a matter of course that a carrier of the infection of the disease cannot be expected to present any signs of illness at all.

Certain considerations relating to the two rival hypotheses may conveniently be stated as follows. On the left hand side are set out the points in favour of or against each theory from the point of view of its supporters, and on the right hand side are certain comments made from the opposing point of view.

First Theory.

(*A*) *In Favour.*

(1) The fact that not infrequently, in the accounts given of pandemic influenza, mention is made of cases exhibiting involvement of the nervous system with symptoms strongly suggestive of cerebrospinal fever.	To these accounts, however, the upholder of the second theory attaches little, if any, importance; or at most admits that they may be regarded as of "antiquarian" interest.

(2) The common supervention of widespread outbreaks of cerebrospinal fever a few years after development of pandemic prevalences of influenza; these have been ascertained, in certain instances, to have occurred simultaneously with the "trailing epidemics" of influenza which are known to prevail more especially in the great centres of communication and traffic.

The upholder of the second theory tries to explain these outbreaks as being due to the movements of healthy carriers. Some authorities, moreover, question altogether the influenzal nature of "trailing epidemics."

(3) The involvement in these last named outbreaks, for the most part, of persons (young children, troops collected from country districts, immigrants, &c.) who have not previously suffered from influenza.

The fact is, of course, admitted. The involvement of troops is, however, attributed to "special virulence of the causal organism, which enables it to attack older persons than those for whom it is ordinarily apt to manifest predilection."

(*B*) *Need for Further Confirmatory Evidence.*

(1) There should be association in families, regiments, &c., attacked, of cerebrospinal fever with influenza.

(2) There should be cases in which it is impossible to differentiate between influenza and cerebrospinal fever in the early stages of illness.

I have endeavoured to obtain evidence with regard to these two points by instituting an analysis of the returns which have been made by London Borough Medical Officers. A summary of the conclusions reached will be given later.

(*C*) *Objections.*

The theory involves acceptance of belief in an undiscovered *causa causans* of influenza, which, it must be assumed, may be associated, in different individuals and in different epidemics, with various species and strains of secondary invaders.

In epidemiology the fact that something or other has not been proved to exist, does not necessarily imply that it does not exist. Moreover such experiments as those of George B. Foster, of Boston (*Journal of American Medical Association*, April, 1916), and those of Dr. Hort and his collaborators, may be cited as having a bearing on this question.

Second Theory.

(*A*) *In Favour.*

The association of the meningococcus with cerebrospinal fever.	The upholder of the first theory regards the meningococcus as playing merely a secondary rôle. Further reference to this objection will be made later.

(*B*) *Need for Confirmatory Evidence.*

(1) It should be shown why infection so rarely spreads from sick persons.	No satisfactory answer to this question has been forthcoming. The suggestion has been recently made that the meningococcus is essentially a saprophyte, and only possesses "potentialities of parasitism" (*see* p. 43).
(2) It should be shown that healthy carriers infect, and that they are more numerous in stricken communities than in the absence of prevalences.	There is need for further inquiry from the statistical side with regard to this question.

(*C*) *Objections.*

The theory involves acceptance of belief in an undiscovered something which activates the meningococcus and enables it to assume pathogenicity.	The same comment applies here as in the corresponding section (C) belonging to the "First Theory."

Recent inquiry has brought to light further information concerning certain of the six sections included in the above analytical statement. This evidence is either mainly epidemiological (section (*B*) of Theory I), or mainly bacteriological (section (*A*) of Theory II), and may be profitably considered under these two heads.

The Further Epidemiological Evidence.

I have tried, from time to time in the last twelve years, to obtain some light with regard to the difficulties of section (*B*), sub-sections 1 and 2, of Theory I. My attention was first specially directed to the likeli-

hood of confusion arising between epidemics of cerebrospinal fever and influenza, at the time of the occurrence of "An epidemic simulating influenza" in East Herts, in the winter of 1904-05 (*see* the Report by Dr. R. A. Dunn and Dr. M. H. Gordon). There was some disposition at the time to regard this as a "new disease." Gordon, who made the bacteriological investigation, did not consider that the meningococcus was at fault, but cases with marked nervous complications occurred, and the malady was said by many observers not to be influenza. Dr. Lovell Drage, who had seen a number of the cases, agreed with me, however, in claiming that influenza was in question (*see* the Milroy Lectures, 1906, p. 39). Again, in 1906-07, coincidently with development of cerebrospinal fever in Belfast and Glasgow, an "Outbreak simulating influenza" occurred in an institution in Holborn, and it was reported upon by Dr. Bond.[1] In a contribution, prepared at Dr. Bulstrode's request, to the discussion on Dr. Bond's paper, I ventured the suggestion that in some of the outbreaks of cerebrospinal fever which had been described by medical inspectors of the Local Government Board, in those of Dr. Dunn and Dr. Gordon, and of Dr. Bond, and, perhaps, in Belfast and Glasgow also, it was really influenza with special involvement of the nervous system that was at fault. During the winter of 1906-07 there was no very noteworthy increase in the recorded incidence of cerebrospinal fever in London, though influenza was known to be prevalent there at that time. With a view to clearing up the question and studying any suspected cases, the disease was made notifiable by the London County Council, on Sir Shirley Murphy's recommendation, on February 26, 1907.

Question then arose as to whether the ordinarily occurring cases of posterior basal meningitis should be notified, and the Council accordingly sought the help of the Royal College of Physicians. The President appointed a Committee to advise, and they reported, *inter alia*, that there were no demonstrable differences in the bacteriology of the two diseases, and recommended that it should be assumed that "posterior basal meningitis is included under the term cerebrospinal fever." The Council, on Sir Shirley Murphy's advice, instructed Dr. Wanklyn to proceed to Glasgow with a view to learning any lessons which the experience of that city might teach, and, later, his services were placed at the disposal of London practitioners for consultation with regard to doubtful cases. From the first Dr. Wanklyn

[1] *Trans. Epid. Soc. Lond.*, 1906-07, N.S., xxvi, p. 110.

insisted upon the difficulties of diagnosis, and in a report (Appendix I to Sir Shirley Murphy's Annual Report for 1907) he very carefully considered how far reliance could be placed upon bacteriology. Dr. Wanklyn and I frequently discussed at that time the extent to which cerebrospinal fever was found in association with certain forms of illness —influenza, sore throat, pneumonia, diarrhœa, &c.—affecting other members of invaded families.

Shortly after this date Dr. Frölich was in this country, and Dr. Bulstrode and I had the opportunity of learning from him much that was of great interest to us in connexion with influenza, and the views held by Norwegian medical men concerning its protean manifestations. About this time, too, the results of a research by Dr. Brorström were made public. Dr. Brorström investigated an outbreak of epidemic poliomyelitis in Sweden, and came to the conclusion that this prevalence was really a manifestation of influenza. He examined groups of cases in families, and gave instances, for example, of three children exhibiting influenza with broncho-pneumonic symptoms, while two others developed poliomyelitis. The difficulty of distinguishing in all cases in London, moreover, between poliomyelitis and cerebrospinal meningitis became strikingly apparent when, thanks to Dr. Batten, the former disease was also made notifiable; it was interesting to find that a similar difficulty was shortly afterwards experienced by medical inspectors of the Local Government Board, in the course of their inquiries in Devonshire, the Midlands, and elsewhere. I was able to pacify Dr. Reece, temporarily, by assuring him that all returns concerning the London cases of poliomyelitis were made on blue forms, and all returns concerning cerebrospinal meningitis on white forms. Furthermore, at about this time, the accounts of the epidemic in Silesia were forthcoming from Germany,[1] and the disappointing result of the attempts made to control the movements of healthy carriers was freely and fully exemplified by the German investigators.

At length with the outbreak of war, and the arrival in London of recruits from remote parts of the country, many of whom were aggregated together in considerable numbers, a notable prevalence of declared and notified cerebrospinal fever was plainly manifested in London. It was felt that there was now opportunity to study further the nature of the relationship between influenza and cerebrospinal fever. Dr. Brincker, Dr. Forbes, and later, Dr. E. H. Ross,

[1] *Klin. Jahrbuch*, 1908, xix, p. 4.

acting for the London County Council, were frequently called in consultation by practitioners, and the London Borough Medical Officers of Health were carefully collating particulars as to "recent illness, including discharge from ear or nose, enlarged glands, &c.," and also as to "health of other inmates of house, especially noting whether any presented symptoms resembling those of cerebrospinal meningitis, influenza, sore throat, gastro-enteritis or 'nasal catarrh.'"

On analysing the returns made it appeared that among the 462 cases occurring (during the period January 1 to May 22 of the year 1915), concerning which particulars were obtained, there was a history of recent influenza in thirty-seven instances, and in twenty-seven of these the patient was said to be suffering from influenza within seven days of the onset of symptoms of cerebrospinal fever. Colds, sore throat, cough and catarrh were noted as having affected seventy patients prior to attack by cerebrospinal fever, and ten patients had recently suffered from pneumonia. Contact with cases of influenza colds, &c., was noted in sixty-six instances. Two questions were thus seen to arise (Annual Report, 1914, p. 35): "Whether the number of cases of cerebrospinal fever giving a history of (*a*) recent attack by, and (*b*) contact with, cases of influenza was *greater than might have been expected as a matter of chance.* The determination of the probability of the occurrence of (*b*) would have necessitated the use of some hypothetical figure which would represent the average number of persons coming into association with the average Londoner in a week. It is obvious that the use of such a figure, in the absence of any precise data for its determination, would detract from the value of any conclusions drawn; consideration was therefore limited to the determination of (*a*), where the only unknown quantity in the problem was that denoting the prevalence of influenza."

Mr. G. H. Day kindly calculated for me the number of cases of influenza which it might be estimated would occur in the London population, week by week, on the basis of the attack rates furnished by returns relating to 3,000 officers in the employment of the London County Council. The number of cases of cerebrospinal fever occurring in each week was known, as also the number of such cases giving a history of suffering from influenza, within seven days of the onset of cerebrospinal fever. The probability of the occurrence of the association of cerebrospinal fever with a history of previous influenza within seven days could thus be ascertained.

In summary of the results obtained the following statement was

made: "Suppose that 462 persons are chosen at random from the London population, in groups of from twenty to thirty, during each week of the first five months of the year 1915 (these were, in fact, approximately the numbers of the actual sufferers in each week). One would expect, having regard to the prevalence of influenza at the time, to find that some two or three of the 462 persons thus chosen would either be suffering from or have recently recovered from an attack of influenza; turning, however, to the 'selected' group of 462 sufferers from cerebrospinal fever, it is found that not two or three but as many as twenty-seven give a recent history of influenza attack. This latter number (twenty-seven) compared with the 'expected' is so large as to afford strong ground for the belief that some relationship exists between the two diseases. This presumption is materially strengthened when regard is paid to the large number of cases of cerebrospinal fever giving a history of contact with sufferers from influenza."

As the outcome of the outbreak of 1914-15 many reports appeared, some of which contained special reference to association of catarrh and cerebrospinal fever. At the discussion held here last year Dr. Newsholme observed: "In reading through a large number of manuscript reports of outbreaks of this disease, during the last few weeks, I have been much struck by the fact that in a large number of instances there were, for about a fortnight at the beginning of the illness, catarrhal symptoms, which afterwards merged into meningeal symptoms." In the same discussion Dr. Buchanan and I urged the importance of studying the relation of meningitis prevalence to increased mortality from influenza, bronchitis and pneumonia, and Dr. Mearns Fraser stated that "he had been particularly struck by the fact that in a large proportion of the civilian cases there had been an outbreak in the house of influenza with severe headache a week or two previous to the case of cerebrospinal fever." In my recently published Annual Report (p. 10) I have given a summary of some of the observations made with regard to this question in various Army and Navy reports, and to this there should be added the witness borne by Lundie, Thomas, Fleming, and Maclagan,[1] who found that nasopharyngitis was an initial prominent symptom in every case of cerebrospinal fever examined by them; there may, further, now be added the testimony given in the recently published Commonwealth of Australia Report. The writers say: "It is, indeed, hard to resist the conviction that epidemics really consist of epidemic nasopharyngeal catarrh, with occasional complicating meningitis, and

[1] *Brit. Med. Journ.*, May 15, 1915, p. 836.

further investigation will probably prove the correctness of this view" (p. 18).

At the conclusion of the recrudescence of cerebrospinal fever of the early part of the present year, it became possible to make a fuller analysis of the London returns dividing up the period under review into epidemic and non-epidemic periods (Annual Report for 1915, p. 11).

So far as "influenza" within seven days of onset of cerebrospinal fever was concerned, there were in the non-epidemic period (1914) no cases; during the epidemic of January-May, 1915, there were twenty-seven cases, while chance would only have warranted an expectation of some two or three cases; then, in the ensuing period of declining prevalence there was only one such case, presumably a chance occurrence; and, in a recrudescence (January-April, 1916) there were five cases, a number slightly in excess of that which might have been anticipated as a mere chance result. Furthermore, in the four periods in question, the proportions of cases with a history of *contact with influenza* were 1·6, 5, 1 and 2 per cent. respectively.

It has been suggested that the association of influenza with cerebrospinal fever in epidemics is merely fortuitous, and the criticism has been urged that a history of influenza just preceding declared cerebrospinal fever is due to the fact that the early stages of the latter disease may closely simulate those of the former. But the critics cannot satisfactorily explain away the frequent association in epidemic prevalences of the two symptom-complexes, nor again the association of cerebrospinal fever, in particular military huts, houses or families, with cases of influenza, catarrh, &c., and not, as a rule, with other cases of cerebrospinal fever. I examined, moreover (loc. cit.) as a kind of control experiment, the extent to which association with soldiers was recorded in the notified cases of cerebrospinal fever, and found it much lower than the percentage in the original experiment, though it might, *ceteris paribus*, have been expected to exceed that percentage.

So much for the examination of the extent to which study of the recent prevalence confirms views previously held with regard to association of influenza and cerebrospinal fever. It is time now to revert to Section (*A*) of Theory II, and here it is very difficult for anyone, not a working bacteriologist, fully to appraise the evidence available. The case is, however, so ably and impartially set out by Foster and Gaskell that reference to their statements may be made under the following heads, with a view to focusing attention upon some of the difficulties felt when the accepted bacteriology is regarded from the epidemiological standpoint.

The Recent Bacteriological Evidence.

(A) The Place of the Meningococcus in the Group of Gram-negative Cocci.

Here it is stated that "The differentiation of the meningococcus is complicated by the fact that it is doubtful whether it is really a specific organism; it is really more likely that a group of closely allied organisms exists, any one of which may be the cause of epidemic meningitis. This multiplicity of strains is possibly the reason why all work on the biological properties of the organism has been so conflicting."

The variations in size and in staining reaction, the possibility of confusion with other Gram-negative diplococci, and the occurrence of Gram-positive forms in sub-cultures of the meningococcus are then discussed.

"One organism can be eliminated for practical purposes from consideration—namely, the gonococcus. . . . It is doubtful if it has ever been obtained from the posterior pharynx. It is also doubtful whether it is ever the cause of meningitis, though instances of this have been described. On these grounds it is, therefore, unnecessary to consider the methods of differentiation of the meningococcus and gonococcus in this book; the matter is not an easy one, and depends partly on the power of the meningococcus to ferment maltose, a power which the gonococcus does not possess. The differentiation by agglutinating reactions is difficult, as also is the differentiation by complement fixation, for cross fixation may take place to some considerable degree."

Then as to the means of distinguishing the meningococcus and parameningococcus from five other commonly occurring and very nearly allied throat organisms, it is stated that "The sugar fermentation tests are very slow," and "The test of growth at 23° C. is of partial value" . . . "agglutination reactions are extremely indefinite throughout the group, and are unreliable for classification purposes." Very interesting statements are made on pp. 151 and 152, as to failure of growth and inhibition of growth of the meningococcus under certain conditions; the reasons for which are not, it is stated, fully understood. "The pathogenicity of the meningococcus with regard to the ordinary laboratory animals is comparatively slight. . . . Too much stress should not be laid on the reproduction of the disease in animals by sub-dural or intraspinal inoculation, for a large number of organisms will do the same, many of which are never found to be causes of meningitis occurring in the human body."

(B) The Question of Fermentation Reactions.

The question of fermentation reactions is then considered. It is stated that "the fundamental difficulty in making use of these reactions for the purpose of differentiation is the complicated nature of the medium which has to be used, and the difficulty of sufficiently sterilizing it without causing alterations in the sugar present. . . . The discordant results of various observers on the fermentation of these sugars by the Gram-negative diplococci are due to this difficulty. The matter is the more important, since the meningococcus and some other members of the group take some days to show with certainty their fermentative power, and the terminal reaction may be only comparatively slight. It is, therefore, always necessary to work with an uninoculated control tube. A series of tubes prepared without the proper precautions may give all sorts of results, so that organisms may be considered to ferment a particular sugar, owing to a reaction which the tube alone would give if incubated without inoculation. . . . Elser and Huntoon have shown that many of the recorded observations on the fermentation of sugars by the meningococcus are erroneous, the reactions obtained having been due to alterations in a badly prepared medium. . . . It is absolutely necessary to subculture from the sown tube to make sure that growth has taken place, and that no contamination has been sown. The latter point is certainly important, and in some cases very difficult to avoid when dealing with cultures from the throat. . . . It has been stated that the meningococcus ferments glucose and maltose only, and this is in all probability the fact." Certain exceptions are, however, referred to. "The glucose reaction varies considerably with different strains of meningococci. We have studied for some months a strain obtained by lumbar puncture, which completely failed to show acidity in glucose tubes." An explanation is sought by assuming that two opposing factors may be present, the production of acidity by the fermentation of the glucose, and the production of alkalinity by proteolysis. In the strain in question the latter masked the evidence of occurrence of the former. "*Micrococcus catarrhalis* is said not to ferment any sugar; we have, however, found that indications of acidity may be observed on the second or third day in a glucose tube. . . . It is possible that here also a balanced reaction is taking place."

(C) Agglutination and other Serological Tests.

"Earlier workers endeavoured to substantiate distinctive agglutination reactions. . . . (Dunham) found increase in agglutinating power was not sufficiently great to overcome the possible errors in technique . . . not only in preparing a satisfactory emulsion, which is almost impossible with certain organisms of the group, but also in the length of time the reaction takes, and the uncertainty of obtaining a really highly agglutinating serum. The agglutinating power of normal serum from various animals is very variable, not only in different kinds, but also in different individuals of the same kind. Another difficulty arises with the microscopic method; certain organisms, notably *Micrococcus catarrhalis*, undergo acute sedimentation. . . . (Elser and Huntoon) find a very great variation in the power to agglutinate in various meningococci, some of them not agglutinating even in the lowest dilutions. These agglutinable strains, however, produced sera which would agglutinate agglutinable strains at high dilutions, although such sera had no power to agglutinate their own strains . . . (Gordon) has arrived at the conclusion that the difficulties in using agglutination as a differential test are due to the fact that the meningococcus is not a single species, but is composed of a group of four separate organisms. . . . It is at present an open question whether such groupings of strains do in reality exist, and whether the simple serum reactions will ultimately turn out to be satisfactory, if used according to Gordon's methods. . . . The difficulties met with in agglutination and other serum reactions have given rise to further attempts to distinguish separate organisms which are similar to the meningococcus, but yet not meningococci. Among these may be mentioned the pseudo-meningococcus of Elser and Huntoon, and the parameningococcus of Dopter. (The former) fails to absorb specific agglutinations from a serum immunized to the meningococcus . . . it is probable such a group of organisms does exist. . . . (The latter) was not agglutinated by meningococcus serum but fixed complement with this . . . it is doubtful whether it is more than a rather extreme variant of the meningococcus."

"Other serological tests have been advocated as being of use in the differentiation of the Gram-negative cocci, but no satisfactory results have been obtained. Opsonic tests have been used, but the phagocytic power of white corpuscles towards different strains of meningococci varies to a very great extent. The opsonic technique is, therefore, of even less value than usual for the purposes of differentiation. Comple-

ment-fixation tests have also been studied to a considerable extent, but with unsatisfactory results."

The outcome of all this seems to be that the bacteriologist can determine, within very narrow limits of error, whether or no a particular organism which he has isolated belongs to a group, including the *Micrococcus catarrhalis*, any one of the four strains of meningococcus, the pseudo-meningococcus, the parameningococcus, the gonococcus, and certain so-called non-pathogenic cocci. If the information be forthcoming that the material yielding the organism comes from a case of sore throat, the bacteriologist surmises that the first of these organisms may be in question; if from the cerebrospinal fluid he suspects one of the next six organisms, if from the urethra he is on the look out for the gonococcus. In the absence of knowledge of the source of origin it is very difficult, if not, indeed, impossible, to distinguish with certainty between these various types of organisms.

This evidence, it may be submitted, does not suffice to show that in each instance the germ causes the disease, it only justifies the conclusion that the several diseases and the various organisms are apt to be associated with one another; and inasmuch as the existence of disease is apparent as a rule before the presence of the organism is demonstrated, the presumption, on the laboratory evidence, might be held to be that the disease favours excessive development of the germ rather than that the germ sets up the disease.

Lundie, Thomas, Fleming and Maclagan, in the paper already referred to, in which they claim that catarrh is constantly present as an initial symptom in cerebrospinal fever, state that, in their opinion, "the primary invasion of the mucous membrane is by a streptococcus, which prepares the way for the entrance of the more delicate meningococcus." A further step is taken by those who maintain that an ultravisible virus must first appear upon the scene, and that it is the morbid condition set up by the ultravisible virus that is responsible for the activation, first of the streptococcus, and then of the meningococcus.

The ability of a particular type of micro-organism to flourish on a particular "terrain" is, of course, a well-known corner stone of bacteriological theory and practice. The inquirer has recourse to the special material, be it hay, souring milk, or putrefying rabbits' blood, to supply him with the organism he desires to use in his investigations. So it might conceivably be urged, he may look to the mucous membrane of the throat for certain streptococci, and it may be (later) for the more

delicate meningococcus; to the urethra for the gonococcus; and to the cerebrospinal fluid for the meningococcus. M. G. Bertrand in his "Étude biochimique de la Bactérie du Sorbose,"[1] has illustrated the specialized nature of the work done by particular micro-organisms. He writes concerning his bacterium, "Il n'est pas jusqu'à la délicatesse des appétits de ce microbe qui ne soit utilisable dans le but de distinguer plusieurs isomères, ou de prévoir la structure la plus intime de certaines molécules compliquées."

Just as this particular type of germ luxuriates when it can attack sorbose, or certain closely related sugars, may not the meningococcus also luxuriate upon its own special soil? Bertrand points out that his bacterium, as soon as it has facilitated a certain chemical transformation, "cesse precisément d'exercer son activité comburante," and he adds, "Enfin, les pathologistes, eux-mêmes, ceux qui ont entrepris la tâche difficile de débrouiller le problème de l'immunité et de la receptivité à l'égard des agents infectieux, trouveront dans l'étude de la bactérie du sorbose des arguments expérimentaux en faveur du rôle trop oublié, sans doute, parce qu'il est mal défini, de ce qu'on a appelé si justement le *terrain*."

There remains to be mentioned a point of some importance. We are told that "the meningococcus, if present in the posterior pharynx, is almost always present in large numbers" (Foster and Gaskell, p. 122). Strictly speaking this should read—The meningococcus, *if it can be demonstrated at all* in the posterior pharynx, is almost always present in large numbers. It follows that there must be some cases, and there may be many, in which organisms indistinguishable from the meningococcus are present, but not in such numbers as to be discoverable. Now various observers have shown that from 3 to 30 per cent. of contacts yield positive results, *qua* meningococci; and Dr. Eastwood's specially undertaken investigations make it clear that at least 10 per cent. of the non-contacts (Londoners) examined gave positive results. Having regard to the probability above referred to, that quite a number of persons, in whom meningococci cannot readily be demonstrated, harbour these organisms, or organisms indistinguishable from them, there seems good reason for bearing in mind the possibility that an organism indistinguishable from the meningococcus may not infrequently be encountered as a normal inhabitant of the throat, just as the pneumococcus and certain streptococci are known to be found

[1] *Annales de Chim. et de Phys.*, 1904, 8me sér., iii, p. 181.

there ; or, again, as the hog cholera bacillus is a normal inhabitant of the intestine of the pig, or as pyogenic cocci of the kind which assume such large importance in small-pox are known to be abundantly present upon the human skin. It follows, then, that the case for assuming great meningococcus development to be the result, rather than the cause, of cerebrospinal meningitis, is very greatly strengthened. It is submitted, therefore, that while search for confirmatory evidence of Theory I, on the epidemiological side, yields results meriting further attention, a corresponding study of recent evidence on the bacteriological side rather leads to strengthening of the suspicion that the meningococcus merely plays a secondary, and not a primary causal rôle, in determining the course of development of cerebrospinal fever.

If all this be admitted, the precise determination of an organism isolated from a throat is seen to present a problem of great difficulty; and yet, as Ledingham says, "the criteria employed for the identification of meningococci from the nasopharynx have probably not always been of a uniformly strict character." In cerebrospinal fluid, the question (from the bacteriological point of view) is comparatively simple, for, says Ledingham, " organisms other than the meningococcus are only very rarely met with in samples of fresh fluid from cerebrospinal cases;" and, therefore, he adds, some of the tests "may legitimately be dispensed with, especially if work is pressing."

As a matter of fact, in the many hundreds of reports from London bacteriologists in 1915 and 1916, there are only two incidental references to the *Micrococcus catarrhalis;* in one case a first report states that " the bacteriologists were unable to state that the diplococcus was not really the *Micrococcus catarrhalis*"; and, in a second, there was found a diplococcus, " possibly the *Micrococcus catarrhalis*, or the coccus that Still described." In view of the admitted difficulty of distinguishing between *Micrococcus catarrhalis* and the meningococcus, and the fact that it is perforce oftentimes the accepted practice to dispense with the attempt finally to clinch the diagnosis between them, the two reports in which reference is made to this difficulty may interest any latter day Diogenes who, armed with his lantern, is prowling about searching for an absolutely matter-of-fact bacteriologist. A sceptic who entertains the notion that one Gram-negative coccus may quite possibly undergo mutation into another, under appropriate conditions, may go so far as to speculate whether the recent outbreak might not quite as justly be attributed to a modified strain of *Micrococcus catarrhalis* as to the meningococcus itself; particularly when it is recollected that a similar

outbreak in East Herts a few years ago, was ascribed by Gordon to the *Micrococcus catarrhalis*. It must be borne in mind, however, that though in that outbreak Gordon was able, in the light of the knowledge then available, definitely to exclude the meningococcus, the test then regarded as absolutely reliable for purposes of differentiation is now no longer deemed trustworthy.

It remains to add that recent advance in bacteriological knowledge renders it very difficult to frame rules for the guidance of those who propose to investigate the problems presented on an attempt to control the spread of the disease by restricting the movement of healthy carriers. This difficulty will be seen to be the more serious when it is realized that it is claimed "the disease need not be carried from one patient to another by means of one single carrier, but many intermediate carriers may be concerned in such a transmission"; and when it is admitted by experts that "if some of these intermediate carriers were of the transient type, the tracing of the path of infection would become impossible."

I cannot, I think, do better than close these rambling observations by two appeals to high authority, one with regard to poliomyelitis, and one with regard to cerebrospinal fever. First, Dr. Simon Flexner, in a paper on poliomyelitis, writes as follows: "We do not possess a generally acceptable theory to account for the epidemic waves of disease. What is required is an adequate explanation of the initial rise, persistence, and the final fall of the wave as represented by the varying number of the affected. That mere presence of the microbic causes of the disease does not suffice to produce epidemics has long been known. It is just the discrepancy between the occurrence of the microbic cause in sporadic cases of potentially epidemic diseases and the absence of true epidemics that has led to the formulation of the hypothesis of concomitant causes of v. Pettenkofer and of Naegeli. While the one supposes a necessary ripening of the microbic agent in the earth as a pre-requisite, the other invokes the co-operation of a second, though unknown, but subsidiary micro-organism. The subject has not been rendered essentially more comprehensible by the discovery of the healthy and chronic carriers of infectious micro-organisms, or by the more ready detection of so-called abortive cases of infection. Indeed, these discoveries only add to the perplexity, since they prove that potentially infective micro-organisms capable of starting epidemics are more frequently present in our surroundings than has hitherto been supposed."

And then, second, there is the hypothesis that cerebrospinal fever is the outcome of a "saprophytic epidemic" of the meningococcus "in the throats of the population at large." The main outlines of this hypothesis are given in the careful report of the Medical Research Committee. The organism, we are told, is "essentially a saprophyte, though with potentialities of parasitism"—divided up into races, some of which may "attain a greater virulence"; there is "spread of the epidemic strains side by side with the domestic and relatively harmless strains indigenous to the locality, so that there are carriers of either, indistinguishable except by serological means."

These conceptions are regarded as representing a "working hypothesis only" . . . there can be "no pretence of finality . . . it seems doubtful whether immunological races will be found quite sharply separable" . . . they are defined "by methods the relative values of which may still be disputed" . . . may depend "upon slight changes in the atom groupings of the bacterial protein determining its reaction with a particular antibody" . . . and "we know very little as yet about the stability of such atom groupings, and the extent to which they may undergo spontaneous variation."

In the light of these guarded statements of the Committee I ventured (Annual Report for 1915, p. 13) to make a suggestion as follows:—The Committee incidentally declare that "a widely air-borne distribution of the meningococcus" involves "a hypothesis to which all its biological properties are strongly opposed" (p. 52). And yet an ability on the part of something or other to pervade the atmosphere of living-rooms, theatres, and places of assembly (limited, it may be, but certainly exceeding that ordinarily associated with "causal organisms"), seems a *sine qua non* if outbreaks of influenza and catarrh are to receive explanation; and these outbreaks, according to the testimony of both bacteriologists and epidemiologists, occur coincidently with outbreaks of cerebrospinal fever. Why not, then, it may be asked, allow that the "potentialities of parasitism," to which the Advisory Committee allude, may be called into activity by this same unknown agency, which might thus be regarded as primarily concerned with the development of these epidemic diseases? The manifestations of the influence in question are then, in some subjects, accompanied by multiplication of epidemic strains of meningococcus and by cerebrospinal fever, and in other subjects by development only of harmless domestic strains of meningococcus with no cerebrospinal fever; but in these latter subjects there may, unfortunately be development of epidemic

strains of pneumococci or of streptococci, or of Pfeiffer's bacilli, with corresponding pneumonia, tonsillitis or influenza. The Advisory Committee advise (p. 57) that "cerebrospinal fever is an epiphenomenon of the (saprophytic) epidemic." The alternative view is that the unknown agency is the original cause of the "influenza" outbreak, which assumes various types, and it is the appearance of the corresponding "epidemic strain" in each case that constitutes the epiphenomenon.

(*November* 24, 1916.)

The Outbreak of Cerebrospinal Fever at Salisbury in 1914-15.

By M. Greenwood, Jun., Captain R.A.M.C.(T.F.).[1]

The following observations summarize the statistical section of a report on the Salisbury outbreak which I was instructed to prepare in collaboration with Dr. W. J. Penfold, of the Lister Institute. Dr. Penfold's absence from England makes it impossible to complete the report, but, as some of the facts may be of interest in connexion with Dr. Hamer's paper, it seemed desirable to publish these notes. It is to be hoped that Dr. Penfold will ultimately discuss the whole subject.

At an early stage of the epidemic which attacked the civilian population of Salisbury in the winter of 1914-15, Dr. Penfold was instructed by the Governing Body of the Lister Institute to visit the city and collaborate with the local authorities. A laboratory was equipped and preparations made to supply vaccine for the prophylactic inoculation of all who desired to avail themselves of the process. A polyvalent vaccine, embracing in most cases six distinct strains of meningococci, was manufactured, the strains being all derived from Salisbury cases. Adults normally received a first dose of 250 to 300 millions, followed a week later by 1,000 millions. No untoward results of inoculation were observed and the local reactions were, in the great majority of cases, slight. In all, over 4,000 persons were inoculated, and particulars of most of these inoculations are furnished below.

[1] From the Lister Institute of Preventive Medicine.

At the outset, certain general features of the epidemic are worthy of notice. In Tables I and II are recorded the cases of cerebrospinal fever notified among the civilian population of the city, and similar particulars respecting the troops within the Salisbury Plain District. The diagram, in which the monthly returns are reduced to percentages, enables the two records to be more easily compared (*see* p. 47).

TABLE I.—MONTHLY DISTRIBUTION OF CASES OF CEREBROSPINAL FEVER IN SALISBURY.

Month		Number of cases
December,	1914	2
January,	1915	9
February,	,,	18
March,	,,	4
April,	,,	8
May,	,,	—
June,	,,	1
July,	,,	2
		44

TABLE II.—MONTHLY DISTRIBUTION OF CASES AMONG THE TROOPS IN SALISBURY PLAIN DISTRICT.

Month		Number of cases
October,	1914	2
November,	,,	3
December,	,,	4
January,	1915	7
February,	,,	16
March,	,,	12
April,	,,	9
May,	,,	9
June,	,,	6
		68

It will be seen that the course of events was not the same in the two outbreaks. Both show a maximum in February, but the city epidemic declined more rapidly thereafter than did that among the troops. As the absolute numbers are small, this difference might perhaps be regarded as unessential. The point can, however, be tested by a method devised by Pearson [4], and the application of his method shows that if we regard each month's cases as a separate group, the odds against the two records having arisen by sampling from a common population are about sixty-three to one. If the groups are widened, the October, November, December and January cases being classed together, and those for May, June and July also grouped together, the odds fall to some twelve to one, but are still appreciable.

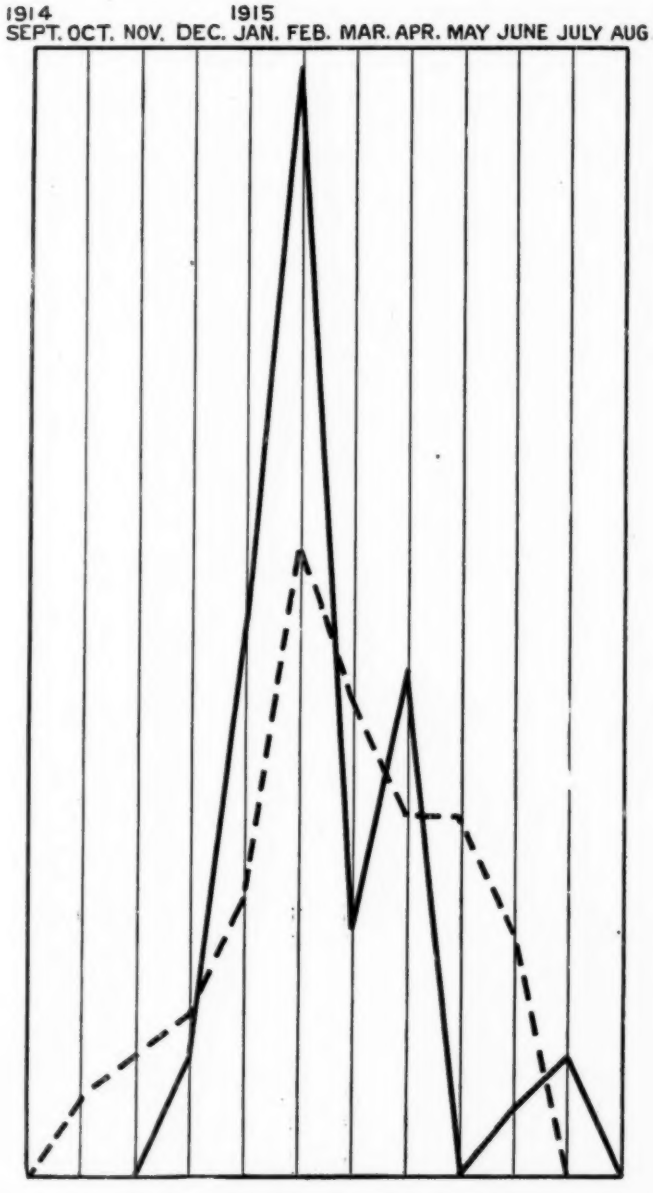
It is, therefore, hardly safe to regard the difference between the two records as a mere error of sampling. The populations at risk were exposed to almost identical meteorological conditions, but, of course, differed in age- and sex-constitution. This difference *may* influence the form of the epidemic curve, but there is no obvious reason why it should do so although we should expect it to modify the *general* severity of the outbreak. Another possible explanation must certainly be borne in mind. Inoculation was introduced in February, and Dr. Penfold, together with the local authorities, displayed so much energy that by the first week of March something like 10 per cent. of the population had been inoculated. In other words, the appearance of a large proportion of inoculated persons coincides with the decline of the epidemic at a faster rate than prevailed in the case of the uninoculated troops of the surrounding districts. No careful reasoner would assign decisive importance to this coincidence, but it deserves to be recorded.

TABLE III.—WEEKLY INOCULATIONS IN SALISBURY.*

Period	Inoculations performed		Totals
February 15-21	22 (22)		22 (22)
,, 22-28	800 (846)		822 (868)
March 1-7	1,691 (1,755)		2,513 (2,623)
,, 8-14	706 (729)		3,219 (3,352)
,, 15-21	140 (142)		3,359 (3,494)
,, 22-28	43 (44)		3,402 (3,538)
March 29—April 4	22 (22)		3,424 (3,560)
April 5-11	16 (16)		3,440 (3,576)
,, 12-18	46 (46)		3,486 (3,622)
,, 19-25	101 (101)		3,587 (3,723)
April 26—May 2	86 (87)		3,673 (3,810)
,, 3-9	26 (26)		3,699 (3,836)
,, 10-16	4 (4)		3,703 (3,840)

* The figures in parentheses include additions received after the analysis had been completed. To the total of 3,840 can be added 334, the number of inoculations known to have been performed, the records of which do not give dates. This grand total of 4,174 is still incomplete.

Let us now turn to the details of the inoculations. Table III contains the data available at the time the analysis was prepared (February, 1916). Since then 393 further names have been received. Of these, 137 could be tabulated by date of first inoculation. As, however, the additions do not finally complete the total, and their incorporation in the tables would not sensibly affect the argument, the analysis has not been repeated. From the figures in the table,



Proportional incidence of cerebrospinal fever in different months. Full line gives civilian cases, broken line military cases.

and an estimate of the numbers inoculated and not there recorded, we can safely conclude that by March 7 between 2,000 and 3,000 persons had received at least one dose of vaccine (the tabulation is by date of *first* inoculation). We have unfortunately no accurate knowledge of the total population of Salisbury at this time, owing to the disturbance produced by the War in a city which is the geographical centre in a ring of camps and the Headquarters of an Army Command. It can, however, be surmised that the number of residents was greater than at the time of the census in 1911. Apart from natural increase, losses due to men joining the colours were probably more than counterbalanced by an influx of civilian labourers and other persons employed by the military authorities. The effective population is likely to have been between 22,000 and 24,000. But this fact, coupled with the circumstance that after March 7 only fifteen cases of cerebrospinal fever occurred, makes it quite hopeless to expect to learn anything of value from a comparison of the incidence of the disease upon inoculated and uninoculated populations as wholes. The possibility of errors of sampling deprives such a comparison of any force.

The truth of this contention will now be demonstrated. Mr. Udny Yule and myself have pointed out that there are objections to the use of Pearson's well-known "Goodness of Fit Test" in cases of the present kind [3], but we recognized its value in rough practice and it may well be used here. If then we take the uninoculated population to have been, in round numbers, 20,000, and the inoculated 2,500, and if no cases of disease occurred among the inoculated, how many cases should we require to find among the uninoculated to make it probable (by Pearson's test) that the two groups are differentiated? We have the following table:—

	Not attacked	Attacked	Totals
Inoculated	2,500 ...	0 ...	2,500
Not inoculated	$20,000 - x$ ...	x ...	20,000
Totals ...	$22,500 - x$ ..	x ...	22,500

What value of x will give us χ^2 equal to, say, 8 (which corresponds to P = 0·046, or odds of 954 to 46 against the difference being a chance event)? Solving for x on this assumption, we find that it is approximately equal to 64. That is to say, we should require to find more cases among the uninoculated than were recorded during the whole epidemic season in order to invest the absence of disease among the

inoculated with statistical significance. Let us pass then to the special age-groups which are, according to previous experience, peculiarly liable to attack. These groups will consist of persons up to 5 years of age, 5 but under 10, 10 but under 15. The numbers inoculated up to and including March 7 are shown in Table IV, so far as they are known, and none of these persons was attacked by cerebrospinal fever. The city population under the age of 15 is less likely to have been seriously disturbed by war conditions than that at adult ages, and may be estimated as 6,023. Seven cases of cerebrospinal fever were notified as having occurred among persons of the ages in question during the period from March 7. Consequently we have this state of affairs: 983 inoculated with no cases, 5,040 uninoculated with seven cases. Might this arrangement be the result of random sampling? The answer is that, whether the four-fold test above mentioned, or another and possibly better test be employed, the odds against this being a mere chance happening are small (the former test actually makes the arrangement more likely than not—i.e., if the samples really came from a common population we should, in the long run, get as great a divergence as observed, or a greater, in more than 50 per cent. of trials). This conclusion is not sensibly affected even if we assume that another 200 of the unrecorded but inoculated fell within the age-groups.

TABLE IV.—NUMBERS INOCULATED WITHIN CERTAIN AGE GROUPS UP TO MARCH 7, 1915.

Age group	Number
—5	74
5—10	364
10—15	545
	983

[This table does not include the later returns, but only a few of the later returns come within the assigned limits.]

It is important to realize exactly what has been proved. We have *not* proved that the inoculations were useless. *Prima facie* their efficacy was complete, for no inoculated person contracted the disease; but there were not sufficient cases of the inoculated for us to be able to assert with confidence that the favourable result was more than a chance happening. It may be added that this method of computing and comparing incidence rates in terms of the whole population of a town is, in the writer's opinion, of very little value. It can only be appropriate when we know, or may reasonably assume, that all the persons

composing the population were equally exposed to risk. This assumption is very rarely justifiable in the case of a disease which occurs in epidemic form, and not usually appropriate even in the instance of an endemic disease. Thus, in the former case, inhabitants of the house or street which furnished the initial cases, whether these be importations or arising *de novo*, are peculiarly exposed to risk, while, in the latter, differences of social class may be of moment, not to speak of other factors.

I desire here to call attention to an epidemiological problem which is not always studied so closely as it deserves—viz., the respective shares of infection and unfavourable environment in the production of multiple cases of disease. We all recognize that the mere occurrence of a series of cases in a house is no proof of contagion, and can cite many examples from the history of, for instance, enteric fever, but we are nevertheless a little prone to attribute such sequences to contagion or at least personal infection, when we have to deal with, epidemiologically speaking, less familiar diseases. In general terms we may say that a distribution of cases in which infection plays no part will be some function of constant probabilities of falling a victim, while if personal infection is involved the probabilities themselves will be functions of the numbers of initial cases. But, apart from the difficulties of exhaustively enumerating the hypotheses based upon an assumed constancy of risks the distinction is still too absolute. The risk might *appear* to be a function of the number already affected (as, for instance, deaths in a bombardment when the number of guns concentrated upon a certain trench was steadily increased), although the cases themselves had nothing to do with spreading the disease. I think it is possible to throw some light on this difficulty by mathematical methods, and hope to discuss the problem in a future paper; it is only mentioned in this note to guard against the risk of seeming to attach too much importance to the inquiry next described.

The evidence adduced by Chalmers, Farrar, and others, reveals a tendency on the part of cerebrospinal fever to show itself as aggregations of cases at foci with little tendency towards general dissemination. Thirty years ago Wolff commented on the fact that the 180 cases of cerebrospinal fever recorded in Hamburg between 1880-85 occurred in 131 streets, about one-eighth of all the streets in Hamburg, and not containing one-eighth of the total population. Chalmers and others have noted the apparent relation between the occurrence of multiple cases and the existence of bad ventilation (not necessarily dirt) in the dwellings. These results point to the establishment of danger zones.

But the only evidence we can usually have of the existence of such danger zones is the occurrence of a case or cases. Perhaps this case, arising it may be fortuitously, creates the danger, perhaps it is merely the objective sign of the existence of other conditions, such as imperfect ventilation, likely to react upon others. Without trying to solve this problem, the essential difficulties of which have been mentioned above, we seem justified in making the point that the associates of a patient are subject to a greater than average risk of themselves taking the disease. The rarity of military contact infections does not invalidate the conclusion. It is true that the degree of overcrowding in sleeping quarters is usually much higher than within even the poorest class of civilian dwellings, but the closeness of association, day in, day out, between the members of a civilian family is considerably greater since so large a proportion of time is spent by soldiers in the open air, and the civilian population includes young children.

For these reasons I thought it well to inquire into the fate of the contacts of the Salisbury cases. It was found that of 240 known contacts 190 had been vaccinated, and fifty had not been vaccinated. No cases of cerebrospinal fever occurred among the former, among the latter there were four cases. In arriving at the number of cases among contacts only those developing after close and direct association with a previous case were reckoned. The four are made up of Mrs. D. and Corporal R., house contacts of E. D., and of W. S. and T. R., workmates of a patient. The evidence connecting at least two other cases with a previous one is strong, but not, in my opinion, conclusive. Tabulating these results, we have :—

	Inoculated	Not Inoculated	Totals
Not affected	190	46	236
Affected	0	4	4
Totals ...	190	50	240

Which leads to $\chi^2 = 15{\cdot}458$ and $P = 0{\cdot}0015$, or odds of 665·6 to 1 against the event. If we apply Bayes's theorem, we find that after 190 trials with no successes the odds are 594 to 1 against getting four or more successes in fifty trials. Alternatively, the probability of getting a sample of 190 without a success after fifty trials with four successes is 0·00036, and the mean of the two chances is 0·001. Either method leads to the conclusion that the observed difference between the fortunes of the uninoculated and the inoculated contacts is hardly likely to be an error of sampling.

Let us now criticize the result. The comparison is not vitiated by any unfavourable age constitution of the uninoculated contacts. On the contrary, the inoculated subjects comprised a larger proportion of children and young adults. Neither is it rendered illegitimate by any evidence of closer association between the uninoculated contacts and the patients than obtained among the inoculated. So far, so good; but two other objections cannot be so easily met.

The first is, that two out of the four cases, and everything turns upon but four cases, occurred at the end of January when the epidemic was at its height. At this stage few had been inoculated, so that most of the contacts then were uninoculated. If the disease is infectious, and if its infectivity is a function of time, that is to say, if cases arising towards the end of an epidemic are less likely to produce secondary cases than those occurring at the beginning or middle of the epidemic, the comparison instituted is unfair. I admit the hypothetical nature of the objection, but in our existing state of ignorance of the epidemiology of the disease, it cannot be left out of account. I have attempted to avoid the difficulty by restricting the comparison to the contacts of cases notified in March and later months only. This limitation reduces the numbers to seventy inoculated persons with no cases, and thirteen uninoculated persons with two cases; a distribution which, if less improbable than the previous one, still has the respectable odds of nearly ninety-nine to one against it (measured by Pearson's test). I think, therefore, that the objection does not deprive the comparison of value as evidence even if it remains in general terms unanswered.

The second criticism is that we have no details of the *length* of exposure to risk in the individual cases forming our data. Upon any hypothesis, length of exposure, whether to the possibly infective patient or to the environmental conditions which favour the disease, must be of the essence of the matter. Had we been dealing with an absolutely large experience we might perhaps have assumed that individual variations of exposure would average out and that one sample would not be more favourably circumstanced than the other. We have indeed no evidence that there *was* any such distinction, but since the total numbers are small, and all that we have arithmetically proved is that random variations are unlikely to have given rise to the observed difference, we can hardly neglect the possibility of material variations—other than the fact of being or not being inoculated—having played a part.

If the results obtained are weighed against the objections cited, I

think the following conclusion may be drawn. The experience of the vaccinated contacts was more favourable than that of the unvaccinated contacts. The difference is too great to be dismissed as a mere error of sampling and cannot be explained by differences of age constitution or closeness of association. Variations of infectivity and length of exposure cannot, however, be eliminated and *may* account for the result, but there is no evidence that they do so.

One further trial remains to be described. In a certain number of families a proportion of the members were inoculated before the disease appeared among them. In all fifteen families returned cases after some 10 per cent. of the inhabitants of Salisbury had been inoculated (Table V). If we regard these families as fifteen bags each containing certain numbers of black (inoculated) and white (uninoculated) balls, and draw at random one ball from each what is the chance that all the balls drawn will be white? I find that the chance is 0·163 or odds of a little more than five to one against. This result is also, so far as it goes, in favour of inoculation.

TABLE V.—FAMILIES IN WHICH CEREBROSPINAL FEVER OCCURRED AFTER 10 PER CENT. OF THE SALISBURY POPULATION HAD BEEN INOCULATED, WITH PARTICULARS AS TO THE NUMBERS IN EACH FAMILY INOCULATED BEFORE THE DISEASE BROKE OUT.

Reference	Number uninoculated	Number inoculated
Pe.	3	—
Ba.	6	1
Pa.	7	2
Cr.	7	—
Sk.	2	2
Sy.	7	—
Lo.	5	—
Ha.	10	—
Sw.	6	1
Ro.	4	3
As.	6	—
Wi.	6	—
Rb.	3	—
Cl.	4	—
Ho.	7	—

Lastly, a circumstance deserves mention which, although not within the purview of a statistical analysis, is of some significance. During the epidemic season of 1915-16, cerebrospinal fever did not become epidemic at Salisbury. It does not follow that because in one year there was an epidemic we should expect a recurrence in the following year; indeed, in the case of some diseases exhibiting a periodicity with

an amplitude greater than twelve months, such an event would be unlikely. But in the case of cerebrospinal fever the experience of other towns suggests that when once introduced it rarely dies out within a year, so that it is open to anyone to attribute the recent immunity of Salisbury to the effects of last year's vigorous inoculation campaign. The only comment I have to make on this argument is that it seems to me quite as convincing as most of the arguments to be found in bacteriological treatises upon cerebrospinal fever.

The results of this inquiry will now be summarized. In the first place, it appears that the form of the epidemic wave on the civil population differed significantly from that characterizing the outbreak among troops occupying the surrounding districts. This difference corresponds to the establishment of a sensible proportion of inoculated persons among the population and may be causally connected therewith. The statistics can, however, only establish a correspondence, not a causal connexion, and the value of the correspondence is weakened by the facts that we are working with very small numbers, that our knowledge of the epidemic form of the disease is incomplete and that we cannot even exhaustively enumerate the factors which may have co-operated in producing the result.

In the second place, the attack-rates upon the inoculated and uninoculated populations as wholes and at certain age groups are favourable to the former, but, even if significance attaches to such comparisons in general, the absolute magnitudes here involved do not lead to results beyond the limits of probable chance fluctuations.

In the third place, the after-histories of contacts yield data suggesting a utility in inoculation. This evidence is open to certain objections but is of real value.

In the fourth place, the distribution of the disease between inoculated and uninoculated members of families reporting cases after the widespread introduction of inoculation is favourable to the inoculated, but the numerical measure of the advantage is not decisively great.

The combined effect of these results is not overwhelming but is sufficient to justify the following conclusion—viz., that a *prima facie* case in favour of prophylactic inoculation has been made out. It appears that general inoculation may be an efficient weapon in our struggle against cerebrospinal fever as an epidemic disease.

From the point of view of the epidemiologist, it is desirable that an adequately controlled experiment on a large scale should be undertaken. Failing this, a complete analysis of the military data should be

carried out with the object of providing controls on future results derived from inoculated populations. In particular the time-curve of an epidemic, the case-rate amongst close contacts, especially under conditions approximating to those of civil life, and the curve of age susceptibility require to be investigated in greater detail and on the basis of far larger numbers than has so far been possible.

The notion that epidemiology is an occupation for the leisure moments of a bacteriologist is especially prevalent in connexion with cerebrospinal fever. Dr. Hamer's paper may help to dispel this illusion, which is my excuse for the publication of these notes. I wish to add the expression of my sincere thanks to Dr. Fison, Medical Officer of Health of Salisbury, Mr. G. F. Fowles, Sanitary Inspector, and the various medical practitioners in Salisbury, who have provided data for the statistical analysis.

BIBLIOGRAPHY.

[1] Chalmers. *Trans. Epid. Soc.*, 1906-7, N.S., xxvi, pp. 3-26.

[2] Farrar. *Report of Medical Officer to Local Government Board*, 1905-6, Appendix 10, p. 171, also *Trans. Epid. Soc.*, 1905-6, N.S., xxv, p. 245.

[3] Greenwood and Yule. *Proc. Roy. Soc. Med.*, 1915 (Epid. Sect.), p. 113.

[4] Pearson. *Biometrika*, 1911, viii, p. 250.

[5] Wolff. *Deutsche med. Wochenschr.*, 1888, xiv, p. 771.

DISCUSSION.

Dr. E. W. GOODALL: I am sure those present will agree with me that the Section is deeply indebted to Dr. Hamer for his most interesting and ingenious paper. The subject dealt with is a very important one; still more important will be the conclusions at which we arrive regarding it; for the acceptance of the author's views would probably go a long way towards an alteration in our methods of handling a very serious epidemic disease. The paper is one of practical as well as academic interest. Before I discuss Dr. Hamer's main thesis, I wish to say a few words on a point which, in my opinion, he has laboured somewhat unnecessarily; I mean the fact that cerebrospinal fever was not a new disease when it was described by Vieusseux at Geneva in 1805. On that point I quite agree with him; the evidence he brings forward from writers of times long past amply proves his point. But I do not think that any epidemiologist, when he talks of an epidemic disease being discovered, means that it had never previously existed. He uses the phrase in the same way as when he talks of an explorer discovering a new country. So far from any of the epidemic diseases prevalent in our day being new, I would maintain that most of them are as old as history and probably older. They may have been more prevalent in one age than another; they may have varied in their severity from time to time; they may have shown changes in certain of their less important features; but these things are accidents; essentially, and in the main, the diseases in question are the same now as they were centuries ago. That is the opinion I have formed from combining my clinical experience with a perusal of the works of certain ancient writers.

I turn now to what I conceive to be the main object of this paper—namely, that cerebrospinal fever is really one of the manifestations of influenza. The evidence most relied upon in support of this thesis is the occurrence of epidemic meningitis in association with influenza epidemics, and especially as "trailing epidemics," the prevalence of catarrhal affections during the recent epidemic of cerebrospinal fever, and the statement that a certain number of persons in London during the last epidemic were attacked by influenza within seven days of the appearance of symptoms characteristic of cerebrospinal fever. In respect of the "trailing epidemics," I confess I should like to be furnished with more detail than is given in the statement that young children, recruits, immigrants, &c., are especially the victims in these. However, let that pass. The difficulty in discussing epidemics of influenza lies partly in the vagueness of the name and the symptoms of the disease. I would raise the question at once, whether the various epidemics of influenza which have occurred since the first one of our generation—the epidemic which began towards the end of 1889—have been all of the same nature. Certainly the symptoms of the patients in some of them have been very different from those observed in the first epidemic. And I think it may fairly be suggested that

under the name influenza more than one epidemic disease has been included. Again, is mere association sufficient to prove that one epidemic is the same as another? In times still not at all remote epidemics of typhus and relapsing fever followed closely upon one another; yet I have never heard it suggested that these diseases were the same. Another explanation also offers itself of some of the anomalous epidemics referred to by Dr. Hamer as examples of its protean manifestations. Take the well-known epidemic in East Herts in 1904-05—I think I may now call it classical, so often has it been used as an example. We are told that in this epidemic there were cases whose symptoms did not permit of their being distinguished from influenza, or scarlet fever, or meningitis, or enteric fever, or diphtheria. These cases all occurred as units of one epidemic, presumably due to a common cause. I venture to suggest that they really were examples of the diseases mentioned, occurring simultaneously in the district, and not of a protean influenza. I recall two "epidemics" occurring in institutions in which I had the opportunity of seeing many of the cases. The one was in a large asylum for lunatics. Both inmates and staff were affected. There were three different diseases simultaneously present: enteric fever, acute lobar pneumonia, and a febrile affection with or without diarrhœa, which was not enteric. The other was even more striking in its variety. It occurred at a large public boarding school. On the day of my visit there were many cases of measles, many of influenza, eight of scarlet fever, several of mumps, two of chicken-pox, and one of diphtheria under treatment. Subsequently, further cases arose. Now, if in two comparatively small, but self-contained communities, so many infectious diseases can be simultaneously present, why not in the much larger and open collection of communities comprised in the district of East Herts?

As to Dr. Hamer's argument derived from the statements made by a certain number of patients, or their medical attendants, that they suffered from influenza within seven days of the onset of cerebrospinal fever, I would ask in the first instance what is the value of the evidence? In what way was it collected? Are the statements first-hand, or are they obtained through two or three different persons? But even if they are thoroughly to be relied upon, yet, in spite of what Dr. Hamer says, they are capable of a much simpler explanation than that the patient suffered first from influenza and then from cerebrospinal fever, that, in vulgar parlance, the one disease "turned into" the other. It is notorious that in not a few instances the initial symptoms of influenza and cerebrospinal fever are very similar, if not identical. Again, it is also notorious that an indefinite febrile attack (in which guise more than one of the acute infections frequently present themselves) is often labelled "influenza" to start with for want of a more accurate diagnosis. I neither intend nor wish to disparage the general practitioner. He is often placed in a difficult position, particularly when he has to deal with patients of the poorer classes, because he is expected and pressed to make a diagnosis straight away, and "influenza" is his refuge. The mathematical calculations based upon the figures derived from the statements alluded to are doubtless accurately carried

out; but they are hardly likely to lead to a trustworthy result if the original figures are vicious.

For the reasons I have given I do not think that Dr. Hamer has proved his thesis; into the bacteriological aspect of the question I shall not enter, as I understand there are those present who are more capable of dealing with it than I am. But Captain Greenwood's conclusions as to the effects of prophylactic inoculation appear to support the views held by the bacteriologists as to the rôle of the meningococcus in the causation of cerebrospinal fever.

Professor W. J. R. SIMPSON, C.M.G.: I should like to associate myself with the previous speakers in their remarks of appreciation of the value of the papers read by Dr. Hamer and Dr. Greenwood. Dr. Hamer has dealt with an abstruse epidemiological subject in a very able manner. He has established that cerebrospinal meningitis is not a new disease, which is a very important point, seeing that the tendency of the day is to label any unfamiliar disease new. With Dr. Goodall, I am somewhat sceptical of new diseases. A disease may be new to a locality, just as cholera was new to England in 1832, and yet it was old in its endemic home. As the world becomes better known owing to more rapid communications, discoveries of unknown diseases will be made by science, but the discovery that a disease exists does not make it new. I am inclined to think that the relationship which Dr. Hamer endeavours to establish between cerebrospinal fever and influenza is not supported by Indian experience. There is one thing certain about the pandemic of influenza in 1891, that it was imported into India by passengers arriving from Europe. The people of Bombay were the first to be attacked, and the disease soon spread from Bombay to other parts of India. It arrived in Calcutta by the railway and first became epidemic in the Indian mercantile quarter in direct communication with Bombay. In its dissemination there was no sign of aërial infection further than that which occurs in close association with those attacked. It was a new disease to India at that time, and was unconnected with the sporadic form of cerebrospinal fever which had long been present in India and which now and again appeared in overcrowded gaols. As far as I am aware, cerebrospinal fever has not become epidemic since. Then, again, the epidemic of cerebrospinal fever in East Africa in 1913, an epidemic of a most formidable kind, in which thousands of the natives of the country died but which was confined to certain areas, has not been followed by influenza, nor was it preceded by an epidemic of influenza. It was the same in West Africa. Arguing on similar lines one might claim the same relationship in regard to plague. Plague occurred in India five years after the epidemic of influenza. Cerebrospinal fever was preceded and followed by plague in East Africa: and cerebrospinal fever followed plague in West Africa, but in a remote part of the country. If Dr. Hamer had stated that an epidemic of influenza is the warning messenger of other epidemics, I think I should be inclined to agree with him. To me a pandemic means the manifestation of the existence of exceptional forces and activities at work in the great laboratory of Nature,

which, by processes yet unknown, result in the exaltation in virulence and infection of microbes latent in the animal or vegetable world, in an area or areas of the globe where these diseases are endemic. Once they have acquired their infectiveness and virulence, which may be the case in several places, it then becomes generally a matter of transport from place to place.

I am old-fashioned enough to think that exceptional heat, moisture, and other physical causes, such as droughts, floods, and earth disturbances, as well as famine and war, play an important part in the revival and causation of pandemics. There may be a gradation in the origin of epidemics, especially those with great powers of diffusion. For instance, physical conditions, such as droughts and floods, produce diseases in the vegetable world caused by the attacks of microbes, then herbivorous animals dependent for their food on the condition of vegetation are affected, and later man receives his diseases from the lower animals. I do not think we quite realize the important part which animals play in the spread of epidemics. Plague is now known to be a disease among rats and other lower animals which is transmitted to man; sleeping sickness is another disease transmitted from lower animals; Malta fever is another, anthrax another, and I think there is evidence that small-pox is maintained, spread, and acquires its virulence by the agency of certain animals.

I have no strong views in favour of the meningococcus as the cause of cerebrospinal fever, but it holds the field at present and has been found in every country associated with the disease.

Dr. Greenwood's contribution is valuable as a record of the results of an experiment in inoculation on a small scale. Should other experiments, perhaps on a larger scale, give similar and more definite results, it may be hoped that they will afford, when sufficiently numerous, some sound basis for advocating inoculation in this disease.

Dr. HAMER (in reply): Two main criticisms have been made. First, it has been urged, it has not been clearly shown that sufferers from cerebrospinal fever are, generally speaking, persons who have not been rendered relatively immune by attacks of influenza. The point is deserving of further examination, but it is definitely known that among sufferers from cerebrospinal fever young children are especially apt to be included, and there is ground for thinking that new arrivals in towns from country districts are also particularly prone to be attacked. The question clearly calls, however, as Dr. Goodall has remarked, for further careful statistical analysis. Then, secondly, it was said, that I have included too much under the somewhat vague designation "influenza." Dr. Goodall seems almost disposed to regard pandemic influenza as a different disease from that seen in "trailing epidemics." Again, there seems to be a notion that in some individuals recurrent attacks of influenza always present the same features. With this view I cannot agree. My experience shows that successive attacks, in one and the same person, may have some things in common, but that each attack possesses an individuality of its own. The thesis

I have endeavoured to defend is based upon the fact that, as Hirsch puts it, "no disease is more constant in the aggregate of its symptoms than influenza"; but, at the same time, no malady is more protean in its manifestations, in particular outbreaks and in particular patients. These principles are, epidemiologically speaking, sound, but they can only be accepted by those who are prepared to admit the possibility that the reaction between the causal agent which produces influenza and the communities affected is not always the same. There is, in fact, a rhythmically exhibited variability and also an underlying persistency of type in influenza prevalences; and this is made clear by examination of the disease at corresponding phases of its cyclic manifestations. The clue to the threading of the maze is to be found by making use of this well recognized epidemiological law, even at the risk of being compelled to make some modification of popular bacteriological prepossessions.

Section of Epidemiology and State Medicine.

President—Dr. G. S. Buchanan.

(*February* 23, 1917.)

(Chairman—Lieutenant-Colonel C. J. Martin, R.A.M.C., F.R.S.)

The Louse Problem.

By A. W. Bacot.[1]

Of the three kinds of Anoplura parasitic upon man only one, *Pediculus humanus* (the clothes louse), is of sufficient importance as a disease transmitter to cause serious anxiety on the part of military authorities for its control. *Phthirius pubis* (the crab louse), a small, readily distinguished insect of very specialized form is restricted to limited hair-clad areas of the body. It is disreputable rather than dangerous.

Pediculus capitis (the head louse) is so closely related to the body louse that it is only grudgingly, if at all, allowed specific rank apart from the latter. Many authorities are disposed to treat the two insects as more or less stable races, separated by structural details of minor importance. The divergence of habits between *Pediculus capitis* and *Pediculus humanus* in relation to their host would seem, however, to create a position which justifies distinctive nomenclature, as well as separate consideration from the practical point of view.

In popular estimation the presence of any of these three insects affords proof of personal uncleanliness, lice being still very generally conceived of as breeding in, if they do not actually feed on, dirt. The association with dirt is really adventitious, arising from the fact that the personal neglect, either enforced or voluntary, which is

[1] Entomologist to the Lister Institute.

essential for any permanence of the infection, is almost invariably accompanied by a lack of soap, water, and clean garments, whereas it would of course be an easy matter to free oneself from lice and yet avoid washing.

As regards food the extreme specialization of mouth parts restricts all three insects to an exclusive diet of blood, obtained under what are probably sterile conditions, so far as any matter extraneous to fluids circulating within the body is concerned.

With *Phthirius pubis* I do not propose to deal and with *Pediculus capitis* only in so far as its distinctive identity from *Pediculus humanus* is concerned.

Immature Period.

Lice belong to the group of insects having an incomplete metamorphosis. Between the larval, nymph and adult stage there is no break in general resemblance, mode of life or method of feeding. The only distinguishing points of any practical importance between immature and adult insects is the smaller size and consequent ease of hiding of the young, together with their ability, during the moulting phases, to resist the action of heat and insecticides for a somewhat longer period. As a set-off to these disadvantages, from their host's point of view, we have only the fact that they extract less blood.

Between the period of emergence from the egg and maturity, both *Pediculus capitis* and *Pediculus humanus* shed their skins thrice; the time occupied differs in relation to food supply, temperature and innate variability under reasonably favourable conditions; twelve days may be accepted as a fair average. The record of forty larvæ which emerged on the same day, were kept in a box in a vest pocket and afforded opportunities of feeding during seven out of each twenty-four hours, will give a fair idea of their constitutional variability.

First Moult.	Second Moult.	Third Moult.
3 per cent. on third day.	15 per cent. on seventh day.	5 per cent. on tenth day.
42 per cent. on fourth day.	72 per cent. on eighth day.	3 per cent. on eleventh day.
55 per cent. on fifth day.	13 per cent. on ninth day.	55 per cent. on twelfth day.
		32 per cent. on thirteenth day.
		5 per cent on fourteenth day.

Pairing.

Pairing takes place at any time. It was frequently observed when the insects were examined after feeding, but the males on these occasions had sometimes little if any food in the alimentary canal. The

period during which the insects remain together is generally considerable, many occasions of over an hour were observed. This is probably by no means the limit, but failing prolonged watching one cannot be certain that the union is a prolongation and not a repetition. As happens with the fleas, the male underlies the female, a position which renders it possible for both sexes to feed during the act of fertilization. The males apparently require some hours' interval between the final ecdysis and the act of coitus, but females, still quite soft and not fully expanded after their recent change from nymph to adult, are often seen paired.

Egg Laying.

Food and temperature are the governing conditions in egg production. Feeding is essential, but laying may commence as early as the second or third day of adult life; by which time the female may have partaken of several meals and, apart from very exceptional circumstances, will have been fertilized. Virgin females, however, lay quite as freely as impregnated ones, only their eggs do not hatch.

Temperature approximating to that between the outer garments and the body is essential (75° F. to 93° F.). Sikora points out that it is probable that when day garments are removed and exposed to cool air (below 60° F.) at night, egg development will be stopped, although feeding may take place by day.

There is a cleavage in habits between the head and clothes louse in regard to the instinct and method of oviposition. Under normal conditions *Pediculus capitis* lays its eggs on hair, *Pediculus humanus* on textile material. In captivity either species can be induced to lay on hair or woven fabric if these conditions are properly adjusted. Tests in boxes lined with flannel and containing a small quantity of human hair resulted in the female of *Pediculus capitis* laying 80 per cent. on the hair and 20 per cent. on the flannel, while those of *Pediculus humanus* laid 20 per cent. on the hair and 80 per cent. on the flannel.

Individuals of both species were then reared from these eggs, which had been carefully segregated so that the following four lots of adults were obtained: *Pediculus humanus* reared from the eggs laid on hair; a second lot reared from the eggs laid on the flannel, and two batches of *Pediculus capitis*, one reared from the eggs laid on hair and the other from those laid on the flannel. Twelve males and twelve females of each of these four lots were placed in similar boxes, lined with flannel and containing a quantity of human hair, great care having been taken

to get the quantity and arrangement of the hair similar in each box. The boxes were kept under the same conditions and the feeding periods and arrangements were as nearly as possible identical. After five days the eggs were counted and their position recorded with the following results:—

EGG-LAYING INSTINCTS OF *Pediculus capitis* AND *Pediculus humanus*.

Pediculus humanus bred from Eggs laid on Hair. ♀♀ and ♂♂.

359 eggs	On hair	38	=	11 per cent.
	On gauze cover of box	1		
	On flannel, side next box	37	=	10 ,,
	On flannel, exposed side	283	=	79 ,,

Eggs laid on hairs only when these came into contact with the flannel.

Pediculus humanus bred from Eggs laid on Flannel. ♀♀ and ♂♂.

344 eggs	On gauze cover of box	4	=	1 per cent.
	On flannel, side next box	55	=	16 ,,
	On flannel, exposed side	285	=	83 ,,

Pediculus capitis bred from Eggs laid on Hair. ♀♀ and ♂♂.

274 eggs	On gauze cover of box	1		
	On flannel, exposed side	14	=	5 per cent.
	On hair	259	=	95 ,,

Pediculus capitis bred from Eggs laid on Flannel. ♀♀ and ♂♂.

340 eggs	On gauze cover of box	4	=	1 per cent.
	On flannel	2	=	½ ,,
	On hairs close to flannel	2	=	½ ,,
	On hair	332	=	98 ,,

This result confirms the previous one as to the distinctiveness of the ovipositing instinct and clears away any doubt as to the possibility of the females being heterzygotes of the same species. The less fixed character of the instinct exhibited in the first trial being probably due to the individuals of *Pediculus capitis* being taken from a stock box lined with flannel, the central space of which consisted of a matted mass of hair, while in the case of *Pediculus humanus* the box was filled with a roll of flannel: under these circumstances it is not improbable that the instincts became dulled, and casual action resulted. Some care is necessary in arranging the boxes; these must not be too small and the amount of hair and its arrangement must not be such as to cause a mat of interlacing hairs at any one point, more especially near the top or bottom of the box. A previous experiment which I performed largely failed owing to a want of discretion on these points; the lice used were taken from the

same crowded stock boxes as the parents of those used in the second trial. The females of *Pediculus humanus* laid 56 per cent. of their eggs on the hair and only 44 per cent. on the flannel, while those of *Pediculus capitis* laid 92 per cent. on the hair and 8 per cent. on the flannel.

In another experiment the attachment of the eggs was carefully studied; as a result it appeared that the method of attachment varied, females of the head louse endeavouring to cement the eggs to a single hair in regular alignment with its direction, while those of the clothes louse attached the greater proportion of their eggs to two or more hairs, for preference choosing those which crossed at an angle.

Fecundity.

With entirely unrestricted feeding I am told that an egg production of ten per day is possible for females of *Pediculus humanus*. During my own trials in which the opportunities for feeding averaged about seven hours per day, the greatest number of eggs laid by any one female was 295—an average of 6·4 eggs per day during her adult life. For limited periods this average has often been exceeded, in some instances eleven, twelve or fourteen eggs have been recorded in a single day. It is possible that these figures would be continued day by day under natural conditions. An experiment in which a number of females of *Pediculus humanus* were given the opportunity of feeding for two hours daily in addition to the seven each night, showed unmistakably that additional feeding raised the daily average, while the fertility percentage was not affected.

The fecundity of *Pediculus capitis*, judging by the number of eggs laid in captivity, would seem to be generally of a lower order than that of *Pediculus humanus*. The highest average was only four per day, with a total of 141, but this was easily bettered by females fed twice daily, in later tests, when the stock from which the insects were drawn had been living in captivity for a year, so that we may very reasonably consider that the average is higher under natural conditions, but even the later trials suggest that the ratio is lower for the head than for the clothes louse.

Fertility.

Renewed pairing at intervals of not more than twenty days is essential to the fertility of the eggs laid by females of *Pediculus humanus;* this is the ascertained limit. The more usual period

would seem to be in the neighbourhood of fifteen to seventeen days. With *Pediculus capitis* the period was generally seven to ten days, at longest, not more than twelve.

A single male of *Pediculus humanus* effectually impregnated eighteen out of twenty-one females, while a male of *Pediculus capitis* fertilized ten—this smaller number was due to the lack of opportunities during his period of greatest vigour, rather than to any want of vitality—the supply of virgin females having temporarily failed.

Length of Life.

Speaking generally, the full adult life of *Pediculus humanus* was about five weeks; that of *Pediculus capitis* under similar circumstances only four.

The longest recorded life of an adult male *Pediculus humanus* was thirty-two days, and that of a female forty-six. The average of a number of females was thirty-four days.

Of the head louse all my records are shorter: one male lived thirty days and a female thirty-eight days, but the average adult life of a number of females was only twenty-seven days.

The length of life of unfed lice depends to a large degree upon temperature and humidity. At a medium temperature such as 60° to to 65° F. the majority of vigorous adult lice live three or four days, a few survived five, and a single specimen seven days. I was informed at one of the infirmaries that their tests had shown a possibility of nine days' survival. As this period agrees with the limit mentioned by both Sikora and Peacock we may conclude that it is correct.

At higher temperatures the survival period without food is shorter; at 76° F. all the lice experimented with died within five days, and at 97° F. within three days. Larvæ just emerged from the egg, unless fed, lived less than twenty-four hours at 93° F., and when kept in a box in the waistcoat pocket the majority lived but little more than a day without food. None survived a second period of twenty-four hours. Cold conditions seemed only less fatal than the hot. When kept at a temperature ranging from 28° F. to 30° F. vigorous adults all survived two days and, though a few showed signs of life after four days, they died without feeding. None of the active lice survived a week in the cold room.

So far as my experience goes nymphs and larvæ, when about to moult, usually show a slightly longer survival period than adults.

Habits.

When feeding, lice are as wasteful as fleas, without the excuse of the latter that fæces consisting of only partially digested blood afford nutriment for their young. In comparison with the bed-bug the digestion of lice appears extremely superficial, and there seems to have been but little development in the direction of the storage of a reserve supply of food, but, as with *Cimex lectularius*, in which this has taken place, provision exists for carrying over the blood in the alimentary canal through the moults, from one stadium to the next. They are sedentary feeders and appear to rely upon the efficiency of the salivary fluid they inject to dilate the capillary system of their host if they fail to penetrate a vein. If a number be placed upon a small area some may obtain satisfaction within thirty seconds of penetrating the skin, others have to wait several minutes—occasionally as long as fifteen—before the blood flows.

Proportion of Sexes.

When a number of lice are kept together in a box and allowed to increase, one does not as a rule notice any disproportion in the relative number of the sexes, but with offspring reared from single pairs the proportion of males and females seems to be almost casual. Any proportion, from equality to broods consisting of entirely males or females, may result; this fact has, I understand, been independently discovered by Dr. Hindle as well as by myself. The following figures will serve to illustrate the phenomenon. The small number of offspring reared in some instances is due to the females dying, probably from old age, within a short time of their segregation. The method pursued was to take paired couples from a stock box.

Proportion of Sexes reared from Pairs of *Pediculus humanus* taken from a Stock Box.

No.	Males		Percentage		Females		Percentage
1	1	=	2	...	43	=	98
2	44	=	49	...	46	=	50
3	31	=	65	...	17	=	35
4	*Nil*	=	—	...	67	=	100
5	119	=	73	...	43	=	27
6	39	=	66	...	20	=	34
7	116	=	68	...	55	=	32
8	*Nil*	=	—	...	46	=	100
9	54	=	92	...	5	=	8
10	*Nil*	=	—	...	49	=	100
11	10	=	71	...	4	=	29
12	*Nil*	=	—	...	39	=	100
13	*Nil*	=	—	...	48	=	100
14	7	=	9	...	74	=	91
15	42	=	68	...	20	=	32
	463	=	45		576	=	55

In a numerous colony such a method of reproduction will of course give the same proportion of sexes as would result if each pair produced an equal proportion of males and females. Presumably this departure from the normal method of sex production serves to minimize the dangers of too close interbreeding.

Gregariousness.

Body lice, and possibly to a slighter extent head lice, are gregarious. This is more especially noticeable with the immature forms when approaching a moult. When the insects are kept in captivity this instinct is very apparent, but I have evidence apart from this. During the course of numerous experiments dealing with the destruction of *Pediculus humanus* under captive but otherwise natural conditions, I had several opportunities of observing the behaviour of escapes on my own clothing. It is most remarkable that these free insects tended to collect in close proximity to their captive fellows, in spite of the fact that the latter were imprisoned on pieces of flannel, treated with various insecticides, which from their smell should have exercised a deterrent effect. That the attraction was not always sexual was shown by the fact that immature lice collected in the same way.

Phototropism.

Both *Pediculus humanus* and *Pediculus capitis* are negatively heliotropic, crawling away from the source of light, or if diffuse, towards any dark object or shadow in their vicinity. Should the source of light be directly above them, and there are no shadows or dark surfaces near, they either crawl aimlessly, or what is more frequent, if they are numerous, cluster together. Their dislike for light affords a convenient basis for methods of dealing with them under experimental conditions. It also affords a means of making rough trials of the value of deterrent substances or fluids, as their dislike for these can be tested in relation to their desire to avoid light.

Clustering of Eggs.

Female lice, in captivity, exhibit a sort of homing instinct in regard to the deposition of eggs, returning again and again to a particular spot for which they have a preference. Certain facts tend to suggest that it is the eggs themselves which form the attraction, for I found frequently that when I reversed or otherwise altered the position of the piece of

cloth on which they were laid in relation to the box in which the insects were confined, with the object of getting the eggs distributed in order to facilitate counting, that my aims were thwarted by the female continuing to add to the original cluster.

Hatching.

The hatching of the eggs is conditioned by the temperature, and possibly also by humidity, but of this I have no exact evidence.

Of a number of eggs taken from a stock box, and therefore in various stages of development, none hatched in a room the temperature of which fluctuated between 60° and 65° F., while at 76° F. there was considerable mortality, and the period of incubation of the survivors was erratic, and spread over a longer period than usual. Three hatched within three days, while the emergence of larvæ from thirteen others was distributed over the following nineteen days. Eggs from the same batch hatched readily in my waistcoat pocket with slight, if any, mortality. At 98° F. the period of incubation was very uniform, within five days after removal from the stock box. It is probable that the eggs, ready and almost ready to hatch, and also those very recently laid, were killed by the sudden transfer from the box in my vest pocket (temperature about 85° F.) to hotter and much drier conditions.

One thousand three hundred eggs, kept in an incubator under humid conditions at 87° F., showed the following hatching periods: 3 per cent. hatched on the seventh day; 56 per cent. hatched on the eighth day; 33 per cent. on the ninth day; 8 per cent. on the tenth day; and 0·2 per cent. on the eleventh day.

The temperature of 87° F. is probably not far from the normal. I find that a thermometer placed between my shirt and skin in the region of the chest records 93° F.; between shirt and waistcoat, 86° to 87° F.; in the waistcoat pocket, 85° F.; while in the breast pocket of a buttoned coat it was only 74° F. to 75° F. If the thermometer is placed against my uncovered head it records 82° F., and in the folds of a woman's hair, near but not touching the scalp, the temperature is 81° to 82° F., while if the scalp is touched the mercury rises to 89° F. These readings were made in a room kept at a temperature of 62° F.

The similarity of the hair temperature to pocket temperature suggests that the eggs of *Pediculus capitis* are adjusted for incubation at the same temperature as those of *Pediculus humanus*, and this conclusion is in agreement with my experience when breeding both insects under the same conditions.

Resistance to Cold.

Exposure for seven days to a temperature varying between 30° and 38° F. does not kill the nits, but none survived a period of eleven days, during the last four of which the thermometer fluctuated between 30° and 33° F. Active lice survived two days at 30° to 38° F., and some partially recovered after four days, but died without feeding. None survived seven days.

Relationship of *Pediculus humanus* and *Pediculus capitis* to each other.

Cummings, in his pamphlet on the louse and its relation to disease (Economic Series No. 2, British Museum, Natural History), remarks that the clothes louse differs so little from the head louse that it is necessary to indicate carefully its distinguishing characters, and gives the following points: Size: clothes louse: mean length, males 3·19 mm., females 4·14 mm.; head louse: mean length, males 2·46 mm., females 3·03 mm. Form: In the clothes louse the body immediately behind the head is broader than it is in the same place in the head louse. The lateral angles between the segments of the hind part of the body are appreciably sharper in the head louse than in the clothes louse and the clefts which run in from the sides between the segments are deeper. In the female of the clothes louse the gonopods are narrower towards the tip than in the head louse. The antennæ of the head louse are usually thicker than those of the clothes louse. I have noted that the eggs of the head louse are slightly smaller on the average, and have already remarked on the divergence in the egg-laying instincts. To these points must be added the generally observed fact that the head louse clings to hair and the body louse to clothing.

To sum up these points there is a difference in average size and general form of the insects; a difference in minor points of structure of antennæ and gonopods of the females; a difference in the size of eggs and a variation in regard to the instincts connected with their deposition. Exceptional individuals of either race may produce the characteristics of the other in one or other of these points, but statistically the separation between the insects appears to hold.

Hybridization.

In an attempt to obtain further evidence bearing on the question of specific identity, hybridization was resorted to; cross pairings between males of *Pediculus capitis* with females of *Pediculus humanus*, and vice

versa, were easily brought about, and carried through without difficulty to the F. 3 generation.

The following points are of interest: There was a high mortality among females of *Pediculus capitis* placed with males of *Pediculus humanus;* unless removed after their first union they were invariably killed. The mortality of the males of *Pediculus capitis* placed with females of *Pediculus humanus* was also higher than when paired with females of the same race. There was a marked disparity in the proportion of sexes in the F. 1 generation of the cross *Pediculus capitis* male and *Pediculus humanus* female. Of four pairings the percentages were :—

No.	Percentage of males	Percentage of females
1	74	26
2	86	14
3	51	49
4	76	32

In the reverse cross, and subsequent generations of this cross, no such obvious disparity occurred. In view of the casual sex production in broods of *Pediculus humanus* already referred to, there would be nothing surprising in the above figures were it not that the disparity is all in one direction—namely, a high percentage of males. The small number of broods, however, does not warrant much stress being placed on the phenomena. In size the hybrid insects of the F. 1 and F. 2 generations are approximately intermediate, but extreme disparity in size was noted in some of the F. 3 broods, very small female and large male specimens being noted, in addition to large females, small males, and others of normal sex proportions. Apparently some of the insects had reverted to the paternal proportions, while others retained the hybrid features in this character.

Summary of Features in the Life-history of Practical Importance for Sanitary Precautions.

Eggs take seven or ten days to hatch under normal conditions—i.e., in clothing that is constantly worn; if discarded and allowed to cool for a period each day, the time of hatching may be extended for five weeks.

Active lice can exist without food, and apart from any host, for periods of up to nine days.

Young lice take from ten to fourteen days to attain sexual maturity.

Females, after attaining maturity, require two to four days before they commence to oviposit.

Egg production cannot take place without food, or under cool conditions (below 65° F.).

Eggs laid by unpaired females do not hatch.

Impregnation is not effective for more than twenty days.

As many as ten or twelve eggs per day may be laid by each female.

A total of 300 eggs may be laid by one female.

The female after maturity may live for forty-six days.

Before the close of her life a single female may have 4,160 living offspring.

Preventive Measures.

Pediculosis is a sign of, and depends for its continuance upon, a low standard of life. With a change of garments and the institution of the weekly washing of shirts and underclothing, the number of the parasites is speedily reduced; when sufficient means and leisure obtain among the people to allow of the regular change and cleaning of bedding as well as of clothing, *Pediculus humanus* must die out.

As regards *Pediculus capitis*, where the conditions of life are not so hard that the mother of the family has to spend the time that should be devoted to the care of her children in the winning of their bread, the head louse follows its relative into obscurity. Drink and ill-health may provide sufficient primary or secondary victims to prevent its extinction, but will do little more.

If we had not quite achieved this desirable goal when the war broke out, it seemed within measurable distance in the more wealthy of the European States. With the advent of war, armies were plunged back into that condition of barbarism which renders the washing and changing of garments, let alone bedding, an erratic and occasional proceeding, even when it does not prevent it entirely for weeks or months together.

The solution of the louse problem depends therefore either upon campaigning under a civilized standard of life, or upon the adoption of efficient remedies for the destruction of these insects. The plan adopted by the British authorities partakes of both methods. In the West the stable Front has rendered it possible for the troops in the field to enjoy, in large measure, the civilized custom of the washing and changing of clothing, but in the actual fighting line and in other areas this has not been found practicable, and dependence upon insecticides is essential.

It is to be presumed that the necessary orders are issued for the

supply of remedies to the men in these situations, but that owing to some accident or flaw in transport, they are not always received. Whatever the cause may be, however, complaints are so numerous as to make it appear to a civilian that the state of our troops in the fire trenches is what may be termed "lousiness," tempered by the receipt of insecticides from relatives or friends.

It follows as a consequence that, in spite of the baths, wash-houses and laundrying establishments, intermittent irritation tends to be general, owing to the re-infection of the already cleansed, or to the infection of fresh troops from home by men straight from the fighting zones—pediculosis being continual in the firing line because the dug-outs and resting bunks are in constant occupation.

The defect in arrangements would seem to be, on the one hand, in the failure of the authorities to issue a sufficient supply of insecticides (if any) to the troops actually in the trenches, and, on the other, to recognize that, while all the known insecticides for the destruction of lice are defective in one direction or another, their imperfections may be counterbalanced by complementary use. To reduce pediculosis to its lowest point it is necessary to use relatively stable, slow-acting remedies, in addition to one of quick action if of short duration. Those most serviceable in emergencies are impracticable for continuous use, owing to the large quantity which their rapid evaporation renders necessary.

The treatment of discarded clothing should, wherever possible, depend upon the action of dry heat, not because this is more effective than hot fluids or steam, but because it is more economical. Wet or moist clothing requires drying before use, and time will not usually permit of this being left to sunlight and open air. A drying chamber, if once inaugurated, might just as well be maintained above the level of temperature necessary for the destruction of lice and nits as below it, especially as it is practically certain that the same temperature would destroy the active females of the Sarcoptes, which cause scabies. The heat necessary for the destruction of both lice and nits is 52° C. for a period of thirty minutes. Allowing a margin for contingencies, 55° C. for this period will be quite high enough if the garments are spread and hung. Higher temperatures will kill more quickly and give greater penetration, but the question of their economy is doubtful, unless the conditions of speed and space render it imperative.

The operation of ironing the seams of tunics and breeches, if performed with due regard to the heat of the iron and the speed of its

passage, may no doubt be as effective as it is convenient, when only the simplest of remedies is possible, but if a hot room is available it seems reasonable to suppose that much economy of labour, as well as greater efficiency, could be obtained by hanging the garments in it. If great speed and economy of fuel were desired, a tubular oven with travelling hangers could be arranged on the principle of a biscuit bakery.

In washing or steaming garments or bedding the same temperature as in dry heat is sufficient; higher temperatures will give quicker action, providing thorough penetration takes place. The nits are actually destroyed if they experience the heat of boiling water for half a minute. The addition of chemicals to the water used for washing or soaking is superfluous if its temperature is high enough, and its quantity in relation to the clothing sufficient, to ensure that the nits in the fabric experience 55° C. for thirty minutes, or a higher temperature for a shorter period. On the other hand, if nits and lice can be killed more conveniently or cheaply by the addition of chemicals, it is wasteful to use heat in addition.

I am rather labouring this point, because it is one of the peculiarities of the destruction of insects that economy of thought and extravagance in practice should be so general. The spirit which dominates the illustrations of Mr. Heath Robinson seems also to exert considerable control over the inventors of insecticides, whose common practice it is to endeavour to raise the general efficiency level of their preparations by complexity in combination. The following, though possibly somewhat flamboyant, is a not unfair illustration of the process: "Kummerfeld's wash is useful, and is prepared as follows: Twenty parts precipitated sulphur are incorporated in a mortar with fifty parts glycerine; two parts of camphor are separately ground with fifty of Eau de Cologne, and twenty of borax and 870 parts of distilled water are added; the whole is mixed together, and three drops of an extract of musk are added; shaking in order to prevent the sulphur from settling down; fifty parts of ether are added to the mixture." This principle pervades a large proportion of the pre-war remedies, and some of the recent ones, and has even crept into the work of scientific importance. For instance, it was remarked by one experimenter that cyllin water when cool was not effective, but became so when heated to 60° C.; yet it had already been pointed out in his own paper that dry heat at 60° C. killed the nits.

Infected Dug-outs.

If practicable the wood-work of which these are built should be flat-oiled; this might considerably reduce the risks of infection, as creosote oil is very deadly to insects, and its application might considerably reduce the risks of lice resting on the wood-work. Treatment with Colorado vermin killer, a preparation that I was asked by the Army Authorities to test in regard to its efficiency against bugs, would be about equally efficacious.

Insecticides.

Insecticides may act either by contact or vapour; in the first case they obtain entrance at the insect's mouth or spiracles and poison it or obstruct the trachea and suffocate it. In the second case the vapour which destroys the insect must poison it by way of the tracheal system. Popular opinion, especially among the compounders of insecticides, has decided that strongly-smelling substances are effective owing to their smell—hence the number of essential oils which are recommended or incorporated. As contacts, any essential oil appears to be deadly, but their action, apart from contact under the practical conditions of use, is likely, in the great majority of cases, to be disappointing, the presence of smell being but a poor guide to the quantity of vapour necessary to bring about the death of the insects.

It will be my endeavour to show that for practical purposes it is necessary to use almost all remedies as if they had a contact value only, because under the conditions of use the diffusion of vapour at a concentration necessary to kill is limited to so small an area that the advantage of the vapour poison over a contact poison is of little if any moment.

My first experiment in the use of insecticides against lice was carried out with 25 grm. of pure flake naphthalene, placed in a thin cotton tube worn as a belt next my skin. I then suspended between my shirt and skin a number of gauze bags containing active lice. These bags, which were kept from collapsing by a wire gauze frame, were placed, both at back and front, about 5 or 6 in. above and below the belt. One bag slipped down to within 1 in. of the belt, and in this bag alone were the lice affected during a nine hours' trial, a few out of a number being killed. The repetition of this experiment under modified conditions showed a similar result. In order to test the comparative value of a number of different remedies under practical conditions and determine

their range of action with some degree of accuracy, I constructed a small piece of apparatus to suspend in front of the body, so that the insects could feed during the progress of the tests. In the central compartment was placed a piece of thick lint 1 in. square impregnated with the fluid or substance it was desired to test; lice were placed in all five chambers. The results showed that even with so continuously volatile and effective a substance as naphthalene the vapour action was very feeble, even within a range of 1 in., and practically ineffective at 2 in. While all the insects in the same compartment as the naphthalene were killed within three hours, only 23 per cent. of those in the adjoining chambers were killed within ten hours, and only one individual out of fifty died in the outer chambers during the same period.

More recent experiments, conducted under another method, while confirming these earlier results, go still further in indicating how narrow is the range of effective diffusion. The experiments in question were planned with a view to obtaining a comparative result between different samples of naphthalene; consequently, it was necessary so to adjust the conditions that a proportion only of the insects were killed during the period of trial. Small pockets of thin cotton, 15 mm. square, were stitched on to slips of flannel about 80 mm. square and in these 0·2 grm. of the naphthalene was placed; over this a larger cover, about 47 mm. square, of fine motor veil gauze was stitched, and beneath this the lice were confined. One of the results could only be explained by the supposition that one of the covers was so arranged that it allowed a slightly further range from the source of vapour than did the other. In one case the distances of the margins of the cover from the edge of the pocket containing the insecticide were 14, 17, 18 and 20 mm. respectively; in the other they were 12, 17, 18 and 18 mm. Although the difference was so slight it seemed the only possible explanation of a difference of 17 per cent. in the average mortality during four separate trials.

In order to test the correctness of this explanation the cover was removed from the slip which was giving the higher mortality and replaced with a slightly larger one. The increase in the distance of the sides of the gauze covers from the margin of the pocket containing the naphthalene was 12, 17, 18 and 18 mm. to 18, 20, 21 and 23 mm., the actual increases being 6, 3, 3 and 5 mm. in range. As a result, in place of a mortality of 42 per cent., none of the lice were killed during the normal four-hour period of the trial, and when the period was increased to eight hours only 7 per cent. died.

It is upon the results obtained in these experiments that I base my case that the effective range of vapours between the skin and undergarments is so limited as to be almost negligible. The deterrent effect of the various insecticides experimented with I have been unable to test on a practical scale under the normal conditions of use. It is possible that the vapours set free may have a wider range in deterrent than lethal effect. I am, however, dubious as to placing very much reliance upon deterrents at all, owing to the fact I have already mentioned when referring to their gregarious habits, that of the lice which escaped and enjoyed free range over my body and clothing, the greater number, in some cases all, were found on the flannel slips which had been impregnated with some insecticide. Attracted thither by the presence of their captive fellows the gregarious instincts proved more powerful than the smell of the insecticide was deterrent.

It was also shown by experiment that quite efficient insecticides when rubbed on the skin are of little, if any, use in preventing the insects feeding. The method of test employed was to rub the skin of the forearm thoroughly with the preparation, re-cover the skin area with the sleeve and, after fifteen minutes' interval, allow a number of hungry lice in a box to feed through the gauze cover of the box. In no case did *all* the lice in the box refuse to feed; generally all, or the greater number, fed greedily. Nor were the lice usually affected. In most cases the few deaths which occurred might have been due to natural causes. A considerable mortality, however, followed when sassafras oil was used to anoint the skin. If in many of these cases the same quantity of the preparation had been smeared on the *shirt* and the insects confined under a gauze cover on the area, though it would probably not have prevented their feeding, it would probably have killed them within a few hours, owing to its spreading over the body of the insect and finding its way into the tracheal system.

The only method of testing the differential deterrent action that I have employed so far consists in utilizing the dislike of lice to light in the following manner: A large sheet of filter paper or other rough, fibrous paper, the surface of which enables them to crawl freely, is placed on a table in front of a window. A band of the insecticide is applied to the paper with its ends turning towards the light; the lice are then put down between the source of light and this line, and one can roughly determine, either from their preference to turn back and face the light or to cross the band, which preparation is the better deterrent. As the method has but an uncertain practical value I have

only carried out a few trials, but these show that the best deterrents are not necessarily those with the most powerful odours.

It follows, if what I have stated is correct, that however valuable and interesting from the standpoint of pure science many of the published papers concerning the relative efficiency of various fluids, substances or vapours for the destruction of lice may be, they have but small practical value, owing to a want of appreciation of the essential facts connected with their use. Remedies have to be applied between the clothing and surface of a more or less rotund body, the temperature condition there being in the neighbourhood of 87° to 92° F. What effect their vapour may have in closed tins, under bell jars or when the lice are suspended within a short distance above the insecticides is beside the question. All depends upon the concentration and duration of the vapour resulting from the quantity you can afford to apply to any given area and the effective diffusion range around this area.

Having now put before you the basis of my contention that all insecticides used under the conditions named must be considered from the point of view of practical use as contact remedies only, I shall proceed to consider them from this point of view.

The main points for consideration are: first, cost, because it is useless to trouble about remedies which, if used in sufficient quantities to be efficient will cost more in a week or two than a new shirt—a point that has been largely lost sight of by the purveyors of remedies under a trade name. Secondly, speed of action in relation to its duration. Generally speaking, speed of action and period of duration are in inverse ratio to each other, but the relation is not always a simple one, and may be modified. Very volatile substances and fluids are of little use unless they can be applied to the whole area of the underclothing at one time, which is seldom practicable while in wear, because they evaporate so rapidly that the treated area is free for re-colonization within a few minutes. On the other hand some substances are so slow of action that, although they might very considerably reduce the length of life and breeding capacity of the insects, the user would be continually subjected to irritation by the sick, and by new recruits which had strayed on to him from outside sources.

There is a further point to be considered that is frequently overlooked by the makers of preparations, both professional and amateur, and that is, how the preparation is to be used. Persons devoid of knowledge of the bionomics of the louse, as well as those who have inadequately considered the subject, appear to think that any prepara-

tion, whether fluid, paste or powder, can be rapidly applied by dusting or anointing the skin surface with it, probably the most unpleasant and wasteful method that can be adopted. For, in the first place, one gets all the discomfort and danger of skin irritation, and in the second, if the preparations are at all volatile, they are even more quickly dissipated than would be the case if used on the under-garments.

The question of the best method of use is not always easy to answer, and the difficulty with some very efficient remedies, especially quickly acting, but rapidly dissipated oils, is to devise some means of distributing them evenly over the garments in quantities sufficiently small not to be disagreeable or wasteful, if possible using some vehicle for this purpose which, while retarding their over speedy action, renders it effective over a longer period. For this purpose soap, if it can be used, is very useful as it supplies an easy means of making a paste, while if an emulsion of the remedy with soap is possible, it makes impregnation by watery solution of any strength possible. With other contact remedies again soap enhances their action as it makes contact with and spreading over the wax-like surface of the insects' integument more easy and certain. There is also the final advantage that it will aid in removing the dirt, which accumulates on a greasy surface, when the garment is washed.

For the efficient protection against the breeding of lice in garments, the underclothing should be impregnated with some fluid or substance which will retain its effectiveness for upwards of a week, and as a precaution against enforced delay in re-treatment, an effective and easily applied remedy of quick action should be carried in addition. I am assuming that the thorough impregnation of undergarments can only be carried out in a large camp or base behind the fighting line. Of all the remedies with which I have experimented, the only ones which fulfil the first of these conditions experimentally and are sufficiently low in cost are crude carbolic acid, consisting chiefly of cresols with a trace of phenol and tarry oils, and cresol—the latter being slightly less efficient. The most feasible and effective method of use is to emulsify them with soft soap, using from 45 to 50 per cent. of soap to 50 to 55 per cent. of the crude carbolic. A perfect emulsion is produced by using sufficient heat thoroughly to melt the soap. The correct strength of the solution to be used for impregnating the garments ranges from 5 to 10 per cent. according to the climate or period of the year. If less than $2\frac{1}{2}$ per cent. of the crude carbolic or cresol is used the destruction of the lice is slow and faulty, if more than 5 per cent. irritation may be caused to the skin

in warm weather. The action is by contact and the soap renders this the more speedy and certain. Under cool, dry conditions effective action is slower or ceases entirely because the perspiration is insufficient to moisten the garment and set free the emulsion to act on the insect. As the lice tend to congregate to the less ventilated and more humid situations the impregnation tends to be most efficient where it is most needed. During the summer I found that a shirt treated with a 5 per cent. solution—2½ per cent. crude carbolic—was efficient in the destruction of lice for a period of five or six days—the action being quicker or slower from a few hours to a day, according to the amount of perspiration. During the winter a 10 per cent. solution—5 per cent. of crude carbolic—was necessary to produce the same results. It would follow that the less the sweating the longer the period of action. Treated flannel may be kept at least fifteen days and probably much longer before use without losing its efficiency. An average sized shirt has an area of about 1,600 square inches, and will take up about 1,000 c.c. of fluid, while after wringing it will retain about 500 to 600 c.c. Thorough drying of the garment is essential before wearing. The individual treatment of garments by men is not very practical, and if many garments have to be wrung, either rubber gloves or a mechanical wringer is necessary. The cost per shirt for solution at pre-war prices would amount to about ½d. to 1d. according to the strength of the solution used. The fault of this remedy is that it is rather slow, and does not prevent feeding until in full action.

The most efficient quick-acting remedy of low cost I have used is naphthalene, which for the purpose of the destruction of lice is quite as effective, and for some methods more effective, in its crude cheap form than in its more expensive purified state. It acts by vapour and possibly also when mixed with oil, soap or grease, as a contact. But it is no exception to the general rule of very restricted range of effective action, it must be *thought of* for practical use as if it killed by contact. Under the conditions of use between clothing and body its action is very rapid, killing in about two or three hours if used in sufficient quantities, and feeding seldom occurred during tests. Unless continued action is desired the quantity used need not be large, as its efficiency depends upon the surface exposed to evaporation; it is consequently more rapid in action when used as a powder, or if the fabric is impregnated with it, than if put up in packets.

Naphthalene is, however, so efficient that the retarding of its evaporation may be a decided gain under circumstances where continued

local action is desired. The impregnation of garments or blankets with it is not difficult, as it is readily soluble. Benzine will take up about 40 per cent.; methylated spirit between 5 and 10 per cent.; paraffin up to 15 per cent. But, more important still, it can be dissolved by heat in oil or oily fluids, which can be subsequently emulsified and used to impregnate clothing, &c., in watery solutions while warm.

If used in impregnation it is less wasteful than if used as a powder, because in the latter case it dusts out, but under either system the duration of its effect is very short owing to its rapid evaporation at body temperature. At a concentration of 10 mgrm. per square inch, equal to about 16 grm. per shirt, it will within two to three hours kill all the lice present, but will have entirely lost its lethal action within five hours and its smell within eight. On the other hand, if used in packet form, although its evaporation period is prolonged, its range of action is limited to a radius of about an inch from the packet. It is therefore unsuited for the purposes for which the crude carbolic soft soap emulsion method is so well adapted, but as an adjunct to these for individual use in case of emergency it is invaluable, and might be supplied both as a powder for use by dusting a pinch or two in at the neck, or put up as a paste by mixing with soft soap for the purpose of smearing along the seams of garments. For the latter purpose crude naphthalene is even more serviceable in its "unwhizzed" than in its "whizzed" condition, because the quantity of oily fluid which it contains renders the use of less soap necessary to produce a paste. The soap, whilst restraining the speed of evaporation of both the oily fluid and the naphthalene, does not prevent the preparation spreading in the fabric of the garment, under which circumstances it acts as though it had a contact effect. It is possible that this is really the case, but its increased efficiency in range of action may be due to the enlarged area from which vapour is produced.

Having now put before you the facts, so far as I conceive them, concerning the most efficient methods of checking pediculosis in the Army, at a reasonable cost, I propose to deal shortly with some of the other remedies that have been recommended for use against these pests.

Clayton Gas.

There is no doubt as to the efficiency of this means of killing active lice or mites in discarded clothing or bedding, but I am still doubtful, in the absence of detailed reports, of the exact procedure and control of

experiments, as to the destruction of nits by its means. Nits are very susceptible, and easily killed at certain stages of their development, and, relatively, very insusceptible at others, therefore the possibility of the nits being sometimes killed by the treatment and at others not, is considerable. The chief objection to its use, however, is, I conceive, that of cost and special apparatus to ensure sufficient concentration, while, I suspect, though I confess to having no actual experience, that the time required is much longer than would be needed for dry heat to produce the same result.

Most of the other suggested methods for the treatment of discarded clothing would seem to require even greater elaboration of method and expenditure of money.

Insecticides for Use while the Infected Clothing is in Wear.

Iodoform and Cytisine.—The former, if it could be used in sufficient quantity, might be ideal, but the cost of impregnating garments with it puts it out of the question, whilst in small quantities it only creates smell and causes expense without increasing the efficacy of other remedies. Cytisine is not only too expensive, but also might possibly prove dangerous.

Vermijelli.—The quantity and method of application required for efficient protection would render this remedy not only expensive, but disagreeable. It is questionable whether it acts otherwise than would any heavy grease, such as the rancid butter of the Cossacks. It is certainly ineffective if used up to 10 per cent. strength to impregnate clothing.

Sulphur.—I am by no means so sure that this is so devoid of action as some experimenters have suggested. Under the condition of use on the body it seems to have a very slow action; variable results are possibly due to differences in the humidity of the body. It is, however, hardly worth troubling about while reasonably cheap and definitely effective remedies are to hand.

Anthracene.—This was tried on account of its similarity of molecular structure to naphthalene, but was all but useless.

Tar Oil.—This is very effective if used neat to impregnate flannel, but is not very suitable for this purpose. If it could be evenly sprayed on in small quantities it might afford a practical remedy of some value. Its period of effectiveness when flannel has been thoroughly impregnated with it is about fifty hours while in contact with the body, but when

used in smaller quantities the period would be proportionally less. It can be emulsified with soft soap in the proportion of twenty-five parts oil of tar and ten parts of soft soap, but is not effective when used as a watery solution to impregnate garments, even at a strength of 50 per cent. Presumably, therefore, it acts by its mechanical effect only.

Sassafras Oil.—This is very deadly as a contact and, judging by smell, is one of the ingredients of many of the effective proprietary pastes and fluids. It is one of the few remedies that will kill lice when they are fed on a patch of skin treated fifteen minutes previously. The proprietary pastes and fluids with which I have experimented, however, do not seem to contain it in sufficient proportions to be effective when subjected to the skin test. Economy in its use may be obtained by diluting it with methylated spirit or paraffin (lamp-oil). It can also be emulsified with soft soap as follows: One part soft soap is melted in five parts of water by heat; twenty parts of sassafras oil are then added very gradually, with thorough shaking or stirring. This emulsion can be diluted with water, but does not remain in emulsified condition for more than a few hours after dilution. Flannel impregnated with a 10 per cent. solution, and then dried, killed all the lice within a few hours during the first thirty hours of wear, but after forty-eight hours it was only partially effective during a twenty-four hours' trial.

Legroux's Method.—A trial was made with the impregnated sachets as supplied for use in France. The ingredients are oils of lemongrass, pennyroyal and eucalyptus, to which powdered naphthalene is added. The result was partial and uncertain, the average mortality being only 20 per cent. in an eight hours' trial, while the fact that there were no feeble, but only dead and active lice, raised a suspicion that the small number of deaths which occurred were due to misadventure—probably pressure.

Only a few of the proprietary insecticides have been tested; of these the best known is probably "*Parasitox*," 1s. per stick weighing 43·50 grm. The proprietors' direction is to "rub daily on the inside seams of the clothing and on the body, especially the hairy parts," I have only tested in so far as regards the first half of the direction, the latter portion being, in my opinion, impracticable for reasons already set forth. Slips of flannel were smeared with the preparation and lice imprisoned under gauze on the treated area. The slips were then pinned to the inside of the shirt, so that the insects could feed through the gauze during the period of the test. Used to the extent of 23 mgrm. per square inch, all were stupefied in one and a half hours;

at the close of the twenty hour trial all were dead. A second test on the same piece of flannel showed all but one out of twenty-five lice active after three hours, mostly active after eleven hours, and after twenty-two hours twenty-two living, mostly active, two paralysed, one dead. Used at 10 mgrm. per square inch, a few were stupefied after one and a half hours. After five and a half hours all were stupefied. After twenty-two hours fifteen were living and active, ten were dead. The quantity used would therefore need to be between 10 and 20 mgrm. per square inch, and as a shirt has an area of some 1,600 square inches it would, I judge, be an expensive remedy to use daily.

An insect powder named "Muhak," cost not stated, was dusted on to flannel to the extent of 20 mgrm. per square inch, and the lice imprisoned as before. It stupefied a number in one and a half hours. After twenty hours, eight were still living, seventeen dead. A second trial with the same piece of flannel showed all active after two and a half hours, and also after eleven hours. After twenty-four hours, twenty-two were living, sixteen of which were active; two were dead. Feeding took place during both trials.

A powder supplied by Messrs. Shotter and Jones was tested by the same method. After one and a half hours all were living, mostly active; after seven hours all living, mostly active. After twenty-two hours, twenty-three were living and active, two were dead.

"Meville's" Fluid.—Used as recommended for anointing the skin. A number of lice were fed on the skin area that had been treated with the neat fluid fifteen minutes prior to the test. The insects fed and showed no ill effects. Used neat to impregnate flannel, the trials being subsequently carried out with lice imprisoned under gauze on the flannel, which was pinned to the inner side of the shirt so that the insects could feed, the period of efficiency, as a slow-acting insecticide, continued for seven days. First day after impregnation, 100 per cent. killed within a few hours; second day after impregnation, 100 per cent. killed after eighteen hours; third day after impregnation, a number were killed within forty-eight hours; fifth day after impregnation, ten out of eleven were killed within forty-eight hours; seventh day after impregnation, seven out of thirteen were killed within forty-eight hours. It is probable, however, that its cost would prohibit its use for the impregnation of garments.

Two pastes and a fluid put up by W. A. Proctor, and intended for use by application to the skin, or smearing on garments, were tested. A patch of skin was smeared or wetted with the preparation, and after

fifteen minutes hungry lice in a gauze covered box were allowed to feed on the prepared skin area. The pastes had little, if any, deterrent effect; the insects fed heartily and were not harmed. The fluid had a partially deterrent effect, but some of the lice fed heartily—one out of fifteen died within twenty hours.

Used for the treatment of flannel, all the preparations proved to be active insecticides if applied in sufficient quantities, but their effectiveness soon deteriorated when used between clothing and body, so that after twenty-four hours they were only partially effective in a twenty hours' test.

"*No Germo.*"—0·260 grm. was placed in a small pocket on a slip of flannel and the lice confined under a gauze cover above it; the pocket holding the preparation was 15 mm. square, the gauze cover above had an area of 2,246 mm. Under these circumstances crude naphthalene would have killed from 70 to 80 per cent. of the insects within four hours. The record of "No Germo" is as follows: After a four and a half hours' trial no effect; after a twelve and a half hours' trial, four out of thirty were dead.

Two trials were made with the "*Kergold anti-vermin body belt*," for which the suppliers make the following claims: "Perfect immunity from all insect pests. The Kergold is the only belt that affords instant and permanent relief from the bane of the soldier on active service. Vermin simply cannot exist when the Kergold anti-vermin body belt is worn; protects the wearer from head to foot. Total immunity from all further attacks. The medical properties of the belt last for approximately six months." The first trial gave results so greatly at variance with these claims that a second belt was purchased in order to avoid the risk of the first one being a "bad egg." In the first series of trials lice were confined in gauze pockets fastened to the shirt so that the insects could feed during the course of the test. In all, eight pockets were placed at varying distances above and below the belt, from the neck to the thighs; during the course of a continuous twenty-four hours' trial the only lice which died were a few in one of the pockets on the upper part of the chest, apparently from pressure of the braces.

In the second trial much trouble was taken to wear the belt before trial and to induce a perspiration by work in a hot room at 97° F. Four gauze pockets containing lice were suspended within an inch of the belt and most of the insects fed heartily at intervals during a twenty-four hours' test. After this period it was found that out of sixty lice nine were dead, four were feeble, while forty-seven were active. While

conceding the possibility that the dead and feeble were due to the action of the belt, I incline to the view that they were overlaid during sleep.

After the belt had been continuously worn for sixty-two hours another test was carried out. After twenty-four hours there were three dead, two feeble and twenty-nine active lice in the pockets. The belt was then worn for a further period, and after five days' continuous wear it was given a last chance. After a twenty-four hours' trial, during which many of the insects fed heartily, one was dead and twenty-nine were active, including one pair in copula.

Preventive Measures Against Lice.

Heat.—Dry air or water at a temperature of 52° C. will destroy both active lice and nits within a period of thirty minutes. Higher temperatures kill more quickly; water at 100° C. kills the nits in half a minute. In the destruction of lice by heat the all important factor is penetration. Thick or folded garments require longer time than hung or spread ones. Bundled clothing is only slowly penetrated. Other factors being equal, dry heat is more economical than wet, because wet garments require drying after treatment.

Cold.—Exposure to cold, 30° to 38° F. or −1·1° to 3·3° C. for two days is fatal to active lice, although they may be living at the end of the period. If continued for four days they are killed within the period. Nits survive exposure to the same temperature for seven days, but not eleven.

Insecticides: Contact.—To keep continuously worn garments free from lice the best method is to impregnate with cresol or crude carbolic acid, employing in soap emulsified form, dipping the garments in a solution containing from 2½ to 5 per cent. according to the climate or season; hot weather needs less, cold more.

Vapour.—Insecticides which act by vapour have a very restricted range—not above 1 in., or, at the outside, 2 in. Naphthalene is probably the most economical and effective of the quick remedies, but at body temperature it rapidly evaporates. If underclothing is impregnated to the extent of 2 mgrm. per square centimetre—10 mgrm. per square inch, or about 16 grm. per shirt—its lethal effect will have entirely passed away within five hours. If used in packets its range of action will be less than 2 in. Used as powder it is wasteful, owing to its dusting out of clothing; mixed with soft soap or grease this waste is prevented and its efficiency is increased. Crude naphthalene is cheaper and more

effective than pure. Sassafras and other essential oils are very effective, but kill by contact, not vapour, under the conditions of use. Smell is no guide to vapour effect.

The Destruction of Nits.

Paraffin Oil.—Immersion for five minutes kills the greater proportion, but not all.

Sassafras Oil.—Immersion for five minutes kills all. A 10 per cent. watery solution of this oil in emulsified form with soft soap is also effective as against nits. (One part soft soap melted in five parts of water by heat, twenty parts of the oil added very gradually with thorough shaking or stirring.)

Crude Carbolic Soft Soap Emulsion.—Immersion in a 5 per cent. solution for five minutes kills all.

Tar Oil Soft Soap Emulsion.—Immersion in a 5 per cent. solution for five minutes kills all.

Potato-paring Infusion.—Recommended by a writer in the *American Journal of Clinical Medicine.* Immersion for thirty minutes was quite ineffective, and did not even kill active lice.

Quassia Chips.—An infusion of 20 grm. in 250 c.c. of water boiled for over an hour was tried; immersion for ten minutes was quite ineffective, and the active lice lived for two days afterwards, but failed to feed.

Chlorine Gas.—Nits exposed to the action of this gas during the course of a trial at the Royal Army Medical College hatched in the normal course and adults were reared from them.

Colorado Vermin Killer.—Carbon disulphide, 10 c.c.; crude carbolic, 91 c.c.; oil of tar, 2·5 c.c.; kerosene, 435 c.c.; killed all nits immersed for five minutes.

Of these remedies tar oil emulsion and a 5 per cent. solution of crude carbolic soft soap emulsion might be used for the treatment of heads, as they are more effective than paraffin oil and cheaper than sassafras oil.

For the treatment of nits in the seams of clothing either of these or the Colorado vermin killer would be effective; the latter would, of course, be too irritating for use where it comes into direct contact with the skin.

DISCUSSION.

Professor G. H. F. NUTTALL, F.R.S.: Having been engaged for a considerable time in studying the louse problem I can perhaps appreciate the value of Mr. Bacot's work better than most of his hearers. Apart from my own investigations I have taken the pains to collect the literature of the subject and ere long hope to publish a full account of what is known of the louse and its misdeeds. So far I have notes on upwards of 520 papers and publications dealing with lice. The literature is remarkable, in that most authors completely ignore the work of others that have preceded them. I may say at once, however, that Mr. Bacot's work is in some ways the best that has been done hitherto on the biology of *Pediculus*. I would mention here that three papers dealing with the biology of the louse will appear next week in *Parasitology*[1] from the pens of Mr. Bacot, my assistant Dr. Hindle, and myself respectively. I would refer those interested in the subject to these papers, since they contain much that is new:—

(1) Are *Pediculus humanus* and *Pediculus capitis* separate species? Mr. Bacot has given us biological differences of his own finding besides those usually quoted. He cites Cummings as an authority for certain morphological differences. All of the differences noted by Cummings, excepting that affecting the form of the gonopods, have been given by other observers in the past. I would note that Meinert and Neumann, two very careful observers, failed to find any constant differences. My opportunities of examining material have been limited, but so far I have found no constant differences between the two purported species collected in four continents. I shall be grateful to persons in different parts of the world who will supply me with a large number of specimens, well preserved in 60 to 70 per cent. spirit, and accompanied by full particulars as to their source. From what I have seen I incline to the opinions of Meinert and Neumann that head lice and body lice are but races of one species.

(2) The influence of temperature on the rate of development is a subject of practical interest. Rubner, who made careful studies on the temperature beneath human clothing, determined that about 32° C. represents the temperature at or near the skin surface of a clothed individual when he is feeling comfortable—i.e., neither chilly nor warm. Mr. Bacot, no doubt with cruder methods, has determined 31° C. to be the temperature in question. From my observations, and those of others, I take it that the optimum temperature for the development of lice is about 32° C. I have found that the eggs kept at 32° C. continuously near the skin, hatched mostly after seven days; at 30° C. in the thermostat, hatched mostly after ten to twelve days; at 37° C. by day and 14° C. by night, hatched after fifteen days; at 30° C. by day and 10° C. by

[1] *Parasitology*, February, 1917, ix, pp. 228, 259, 293.

night, hatched after twenty-seven days; at 30° C. for twenty-four hours, and at 5° to 10° C. for twenty-four hours, alternately, hatched after 35 days.

(3) I have failed to observe any difference in the manner of oviposition of "*capitis*" and "*vestimenti*" upon hairs.

(4) Mr. Bacot's statement that additional feeding of adults leads to a greater number of eggs being laid is confirmed by my observations. When fed twice daily I found, as most observers have, that a female lays four to five eggs per day. Mr. Bacot, when he fed them more frequently obtained an average of 6·4 eggs per day; at times this number was much exceeded. I have, however, obtained better results under the optimum conditions offered by keeping lice confined continuously in a felt cell strapped to the arm. Under these conditions of unlimited opportunities of feeding, and an optimum of warmth and moisture, two females laid an average of ten eggs apiece a day for twenty successive days; one female laid 266 eggs in all before she died, probably from being crushed; nevertheless her ovaries were not exhausted.

(5) *Longevity.*—I have also found that adults may live four to five weeks when fed twice a day. The longest period recorded for lice surviving unfed is ten days, this observation having been made by Hase. Observations as to alteration in the fat-body of lice that are starved are lacking.

(6) The remarkable difference in the proportion of the sexes in various broods, to which Mr. Bacot has referred, was made the subject of special study by Dr. Hindle, and will be found described with full protocols in his paper.

I would note with regard to the duration of the life cycle of lice under normal conditions upon man, that my experiments have shown the development from egg to egg to last seventeen days. In the experiment made by Warburton (1911), who carried lice in receptacles near his person, feeding the insects twice daily, the corresponding period was twenty-four days.

Turning to the matter of dealing with the destruction of lice, I would note that my experiments and a survey of the literature published in all the countries engaged in this war, have both led to the conclusion that *dry heat*, where applicable, is the quickest, cheapest, and safest way of dealing with infested articles of clothing, furs, bedding and the like. I find that moderate dry heat kills all lice and their nits in a short time; they are certainly killed in one minute at 65° C., or in five minutes at 62° C. Immersed in water heated to 76° C., they are killed in half a minute, at 60° C. they are killed in five minutes. In practice, naturally, time must be given for heat to penetrate the objects which are treated. The time required will depend upon the degree to which the objects are packed together, as old experience at the hands of bacteriologists has shown. The *hot air should circulate* freely among the objects to be treated if rapid destruction is to be attained. Various simple ways of dealing with this mode of louse destruction are in use to-day. I shall deal with the subject on a future occasion. The *storage* of infested clothing and blankets in a dry warm room would rapidly lead to the death of vermin. I heartily endorse Mr. Bacot's remarks upon the useless character of most insecticides and repellants. I would add to his list an article advertised as the

" Asiatic body cord," which is said to work wonders; it consists of a string to be tied around the waist, the string being surrounded by a greasy substance having the colour of blue ointment; smeared on a slide and examined microscopically, I detected globules of mercury in it. Naphthalene, as also sulphur, have not hitherto given complete satisfaction, and no little trouble abroad. All the evidence points against the use of sulphur dioxide because of its injurious effects on fabrics and metal, &c., and the smell that clings to articles treated with it. Carbon bisulphide vapour has been found better than SO_2; its odour vanishes rapidly, but is objected to because of its toxicity and inflammability. My experiments carried out last year with twenty-four hour old nits and a number of insecticides will be published in due course, but I may mention a few of the results: Nits were not killed by immersion for twenty minutes in paraffin; ten minutes in benzine, petrol, ether; five minutes in carbon bisulphide. Nits were killed by immersion for ten minutes in 2½ per cent. carbolic acid or in carbon bisulphide; by five minutes in 2 per cent. lysol; by one to two minutes in sublimate vinegar or sublimate glycerine. The use of 5 per cent. carbolic as a head wash, applied for ten minutes, was advocated by Whitfield (1912) for the destruction of *Pediculus capitis*. Cresol soap, to which Mr. Bagot refers, has been used with apparently satisfactory results in the German army, and its use was advised in this country by Copeman. It will be interesting to learn how the method suggested by Mr. Bacot of applying carbolic acid to underwear will work out in practice. Experience in the field has shown that different methods of dealing with the problem are required, and that they must adapt themselves to the most varied conditions. At times the conditions are such that the merest palliatives are all that can be used. Even hand-picking, if persisted in, has given good results. Finally, a word about dead or empty nits adhering to cloth; I have been asked how these are best removed. They can only be removed mechanically by means of a knife blade or finger nail, or by singeing off the fibres to which the nits adhere; no solvent affects the chitin-like cement which glues the egg to the fibre without first destroying the fibre.

Professor LEFROY: I am astonished that Mr. Bacot makes reference to only one published work, and that now, at this stage of the War, he can discuss this problem, and can bring forward no newer remedy than naphthalene, and does not even give the credit due to Major Lelean, who originated the N.C.I. formula in the winter of 1914-15 or earlier. In January, 1915, it became known to us at the Royal College that vermin were really bad at the Front; that the material used in India as crude oil emulsion was fatal to lice, and had been successfully used there for some years, both against head lice and body lice. It was also essential that a special grade of mineral oil, containing a definite hydrocarbon, should be used, as neither petrol nor paraffin were deadly to lice as vapours. I had some emulsion made up, and on my recommendation the Army Medical Service agreed to make a trial of it. This oil emulsion came into use under the name of " Vermijelli," and

was tested in France by Lieutenant Gair, with some fifteen or twenty other preparations. He reported in favour of N.C.I. and oil emulsion jointly, and I am informed that these were officially issued at the rate of $\frac{2}{3}$ oz. per man per week. While in India I had this preparation made at the Medical Stores Depot, Bombay, and issued for use in Mesopotamia, and I still adhere to the opinion that this emulsion destroys lice if used on the body; kills eggs if applied to them; keeps lice from infecting you if the underclothing is impregnated with it. I have never claimed that clothing impregnated with it will kill lice. I know of nothing that can make clothing deadly to lice except certain oils, the application of which presents difficulties. Mr. Bacot, when referring to vermijelli, says: "The quantity and method of application required for efficient protection with this remedy would make it not only expensive but disagreeable. It is questionable if it acts otherwise than any heavy grease, such as the rancid butter of the Cossacks would do. It is certainly ineffective if used up to 10 per cent. strength to impregnate clothing."

Mr. Bacot's sole test of efficiency apparently is that the preparation must be applicable to clothing, and that this clothing will then, if worn, clear you of lice. This is not, in my opinion, the louse problem.

Mr. Bacot bases his remarks on tests made with lice enclosed in muslin bags placed under his clothing; but he does not seem to have made the proper test, which is to become verminous and then treat oneself. His laboratory conclusions, based on a defective method, are contrary to the experience of men at the Front in hundreds and thousands—men, officers, and ladies, from France, Serbia, Mesopotamia have testified in scores to the efficacy of the oil emulsion, and I still maintain that an emulsion of fuel oil of the specific gravity and boiling point that I refer to is an efficient destroyer of lice, if applied on the body in sufficient quantity and vaporized under the clothing with the heat of the body. If otherwise, why has vermijelli proved effectual? Why did the Army Medical Authorities adopt it? For it has filled a gap at a critical time in Serbia, France, and elsewhere. There are doubtless better specifics now, but it is not fair to dispose of the oil emulsion as Mr. Bacot does. The solution of the problem is not made clear by the lecturer; the last paragraphs of his paper refer to many methods, and apparently leave the choice open. What treatment does Mr. Bacot recommend to men who are verminous and to those exposed to vermin infection? If he were sent to a Front what would he do? In such circumstances I provided myself with the oil emulsion discarded by Mr. Bacot, and I found that it worked. The method of keeping lice in gauze pockets fixed under the clothing does not appear to be any real test; the louse is very sensitive, and there is only one real test: *be verminous and clear yourself*. But Mr. Bacot relies on an experimental method which is of doubtful value. The advice given by Mr. Bacot appears to be that the soldier at the Front should rely on the doubtful remedies sent to him by his relatives. But as this paper is entitled "The Louse Problem," it is reasonable to ask what is to replace the methods which have proved so far reliable.

I would further suggest that there is scope for further work, that the limits of the investigation are perfectly definite, and that the body temperature is the one important factor. The Germans are reported to clear their men with cyclo-hexanone ($C_6H_{10}O$), a compound related to what I believe to be the active principle in the fuel oil used. If I were to attempt further work with the louse, I should commence to investigate the cyclo- and allied compounds contained in these oils. Mineral oils are composed of a great range of hydrocarbons, and their insecticidal action varies enormously; the fractions that come off at temperatures between 250° and 400° from the different oils differ in chemical composition, and it is amongst these that I believe the best odourless liquid will be found that will kill vermin at body temperature. I thought we had it, but Mr. Bacot does not agree. It is perhaps going far to dive into the difficult chemistry of the oils in order to obtain a vermin remedy. Perhaps I am mistaken, but Mr. Bacot appears to lump all these together; if so, I understand his lumping together fuel oil and the Cossacks' rancid butter.

Lastly, I hope the Section will not confine its discussion to the academic and scientific aspect of the problem. The thought that there are hundreds and thousands of men scratching themselves at the Front fills my mind to the exclusion of the purely scientific aspect of this work, and I look to the attainment of some practical conclusion that will help them. I trust that this paper and the discussion may not end in talk only but in some real advance in methods of treatment.

Mr. A. W. Bacot (in reply): Of special interest and value are Professor Nuttall's observations relative to the minimum within which the life cycle may be completed; his figure of seventeen days is some three days less than the minimum of my own series, and corresponds to a decided acceleration of the rate of increase in offspring resulting from a single pair.

Professor Lefroy's observations raise points which the practical importance of the problem compel me to examine in some detail. I can, however, pass over briefly the accusations of ignoring the work done by previous investigators. So far as these relate to the purely biological aspects of the louse problem it is sufficient to remark that this paper only treats of them incidentally, and that in the special memoir on the scientific questions involved, which I have contributed to the current number of *Parasitology*,[1] the necessary references will be found. As to the priority of Major Lelean in connexion with the practical use of naphthalene, a professor of entomology must surely know that naphthalene had been employed as an insecticide by hundreds of persons years before Major Lelean dealt with the subject, and that to complain of the omission of any individual's name in connexion with it is as reasonable as to charge a writer recommending opium, without cited authorities, with doing an injustice to Sydenham. The gravamen of Professor Lefroy's charge is,

[1] *Parasitology*, February, 1917, ix, p. 228.

however, threefold—viz., (*a*) That my experimental methods are faulty; (*b*) that I have brought forward no newer remedies than naphthalene; and (*c*) that I have been unjust to the claims of the proprietary article "vermijelli."

(*a*) I need not recapitulate the experimental technique carefully explained in the text of my paper. It is certainly true that a remedy found effective as against lice confined in the manner described might not be effective against the whole of a louse population allowed to wander fancy free over one's body. But it is not less true that a remedy which fails under this method of examination will be futile when the lice are given a still better chance of escaping its influence. My technique clearly provides the minimum standard to be reached. If Professor Lefroy has evolved a better technique, it is his duty to publish it so that other investigators may be able to give it a trial. Vague references to the experience of even "thousands of men, officers and ladies from France," or elsewhere, carry no more weight than the similar utterances to be found in the advertisement columns of the daily press.

(*b*) The charge that I have brought forward no newer combination than naphthalene as a subject for trial should be weighed against the fact that the paper deals with a long series of tests performed with various substances, and a detailed method of impregnating underclothing with a solution of an emulsion of crude carbolic acid and soft soap. I have, of course, no personal interest in the newness or otherwise of any remedy. Professor Lefroy indeed says, in reference to "vermijelli," that "there are, doubtless, better specifics now," but he has failed to mention any, and there is no published evidence that he has performed a single test with any substance other than crude mineral oil emulsion; indeed, I cannot find that even with respect to this heavy oil emulsion, Professor Lefroy has made public any controlled experiments similar to those regarded as essential in ordinary biological or pharmacological investigations. One would perhaps have thought that such elementary desiderata as range and period of action, effect of physical conditions, effective concentration or quantity in relation to area, would have been made public by an investigator so solicitous of bringing comfort to the "thousands of men, &c.," and I must regard the other "thousands of men, officers and ladies, &c." as but phantom substitutes for this exact information. It is perhaps possible to be scientific without being in Professor Lefroy's sense of the word academic.

(*c*) Professor Lefroy's eloquence in no way moves me to weaken what he evidently regards as a censure of "vermijelli." I pass over his suggestion, rather than explicit statement, that "vermijelli" derives specific virtues from containing certain hydrocarbons of specific constitution; no one, not even the "thousands of men, officers and ladies," having produced any definite evidence on the point. He opposes to my remark that "vermijelli" is certainly ineffective, &c., the statement that "I have never claimed that clothing impregnated with it will kill lice." By way of commentary on this positive

assertion I will quote a passage from a pamphlet written by Professor Lefroy and widely circulated some months ago.[1]

> "The emulsion should be used in the hospital laundry as soap, or in combination with other soaps. The articles, after washing, should be rinsed in water containing from 1 per cent. to 2 per cent. (about 2 oz. to the gallon) of the emulsion, well mixed. They should not be wrung out too tightly, but allowed to dry so that the clothing is impregnated with it. The clothes will then confer protection against lice. Vermin hatching out from the eggs upon cloth that has previously been treated in this manner have been found *incapable of survival*." [My italics.]

The difference between claiming that a substance will kill lice and claiming that it will render them incapable of survival is so subtle that I think it a little hard to be condemned as unpractical and academic by a writer who lays stress upon it.

To sum up, I think the claims made on behalf of "vermijelli" by its vendors are in excess of its real merits. My reasons for this opinion are set out in the paper just read, and subsequent experiments simply confirm these results. When Professor Lefroy sees fit to make a quantitative series of trials and publish the results I shall willingly reconsider the matter, but not until this is done.

[1] "Measures for Avoidance of Flies, Mosquitoes, Lice and other Vermin." H. Maxwell Lefroy, M.A., F.Z.S.

Section of Epidemiology and State Medicine.

President—Dr. G. S. Buchanan.

(*May* 25, 1917.)

Chairman—Colonel J. Lane Notter, D.P.H., R.A.M.C.

Recent Researches into the Ætiology of Typhus.

By Muriel Robertson, M.A.

(From the Lister Institute of Preventive Medicine.)

"Typhus fever is an infectious disease which runs an acute course of from twelve to fifteen days and culminates spontaneously in a more or less abrupt lysis. It is characterized by an incubation period of from five to twenty days, a high continued pyrexia and a petechial rash."

This definition of the disease is quoted from Wilder, 1911; he adds that the method of dissemination "is of a peculiar nature not dependent upon the direct transmission of the contagium from the sick to the well, but rather by the transfer of body parasites (lice) carrying the contagium."

In opening this discussion I propose only to touch quite superficially and in passing upon the historical aspect of the question, then to consider the more probable views as to the nature of the actual virus, and finally to give a short critical account of the experimental evidence relating to the transmission of the disease by insects, notably by pediculi.

Typhus is a disease of dirt, misery, crowding and famine, it is associated with periods of unrest and political upheaval and above all with war. In the individual fear and mental anxiety are frequently noted as adjuvant circumstances. The disease has a wide distribution but is restricted to temperate climates. When it appears in warmer regions it is only to be found at high altitudes or during the cold season. Thus in Mexico typhus occurs on the high cool plateau and

not on the tropical seaboard; similarly in North Africa and in India it is a winter and a mountain disease.

From the historical point of view typhus is not particularly interesting, largely because it has not got any very dramatic symptoms by which it can be recognized in early and imperfect accounts. Ancient and mediaeval writers were in a happy state of confusion about all fevers, and typhus seems to have appeared along with plague, relapsing fever, malaria and typhoid, with all of which it has at various times been confused. It was first recognized by Girolamo Frascatori in 1505 as a fever distinct from plague, and the name "petechie" was used for it. Nevertheless it was long considered to be only another and less severe manifestation of the very familiar and much dreaded pest. The confusion of *typhus exanthematicus* with typhoid fever occurs all through the literature up to within quite recent times, and in Germany there is to this day a certain inclination to consider them as one.[1]

From the beginning of the sixteenth century onwards there is a succession of typhus epidemics in every country in Europe; wars and bad harvests, either alone or operating together, seem to bring this disease in their train as an almost inevitable concomitant. There is no particular interest in following these dismal records, it is more to the point to note the very few instances in which wars did not produce serious outbreaks of typhus. As far as I have been able to discover France and America are the only countries where a war has not inevitably brought this disease.

France has rather a curious history in regard to typhus in the nineteenth century: from the end of the Napoleonic wars up to 1852 there is a complete absence of typhus. The winter of 1852-53 was very cold and food was scarce in Paris; dysentery, bronchitis and pneumonia became frequent and in the early spring of 1853 typhus broke out. Interestingly enough this epidemic was not restricted to the poor and badly housed alone, but affected all classes; the mortality in the private practices among the wealthier and therefore better nourished people was, however, very low, only 1 per cent., whereas among the hospital patients drawn from the poorer classes the mortality was 21 per cent. This seems to point to the important connexion between the severity of the disease and the state of nutrition of the population. The army returning from the Crimea brought typhus again into France and there were several epidemics, which, however, died down in the course

[1] *See* for example O. Mueller, *Med. Klin.*, 1915, as quoted in the *Trop. Dis. Bull.*, viii, No. 1, p. 57.

of the next few years. Since then, except for small outbreaks occurring in Normandy and Brittany, involving insignificant numbers, France has been singularly free from typhus. Even the Franco-Prussian war did not, according to Chauffard, produce an epidemic. This may perhaps have been due to the short duration of the campaign.

The only other considerable war which remained free from typhus was the American Civil War.

In the present war typhus has not so far appeared among the Allied armies on the Western Front, but Russia, Serbia, Macedonia and Germany have, as is well known, all had epidemics.

The mortality in different epidemics of typhus varies enormously: it may be as low as 9·9 per cent., as in an outbreak in Dublin in 1836-38. An epidemic with an even lower mortality—namely, 2·5 to 4 per cent.—is noted by Haeser as having taken place in Scotland in 1843. Caldwell, 1916, notes that in the recent epidemic in the Balkans the hospital mortality ranged between 19 and 65 per cent. In 1846-48 there were outbreaks of typhus in Flanders, and Haeser states that in the Canton of Mons the mortality was only 1 per cent., whereas in another district of Flanders there were 734 cases, of which 693 died, equalling a mortality of 94·4 per cent.

One feature stands out very distinctly, especially in the later and more clearly differentiated epidemics, and that is the close connexion of famine with typhus; in every case a scarcity of food seems to create the particular conditions that foster this disease. No such connexion is found to anything like the same extent for instance in epidemics of plague or cholera or small-pox. Curiously enough European relapsing fever seems also connected more particularly with periods of hunger.

Before leaving this aspect of the question, which will I hope be treated in detail by some of the epidemiologists present, I would like to touch upon an account of typhus in Serbia given by Dr. Jeanneret-Minkine. This is a vivid and interesting little book, though in some respects it is superficial and not very accurate. It is, nevertheless, a convincing description recorded at first hand by an experienced and observant medical man. The author is much impressed by the effect of the psychological factors of fear and depression in predisposing to the disease, and as affording a bad prognosis when infected. He also notes the fatigue and exhaustion of the soldiery at the time of the outbreak of the epidemic. In many instances the hospitals themselves were active centres for the actual spread of the contagion, and he gives

a number of striking instances to show that pediculi were accountable for the spread of the disease. He notes for instance that the only hospital where typhus did not break out was an English one at Uskub, which was able to keep itself free from lice, and where the well instructed personnel refused to take patients except in the numbers that their bathing and receiving arrangements could cope with. Jeanneret-Minkine describes his own hospital as being simply flooded with patients who could not be washed, for whom they did not have clean shirts, and for whom there were no blankets; they had therefore to lie in their uniforms. All the medical men took typhus except one, who assured his colleagues that he had been able to keep free from lice.

The author thinks that cimex may also transmit the disease, but the evidence he adduces is not sufficiently sound, and the louse channel is not excluded in the instance cited. He notes among his clinical observations the great frequency of secondary infections, such as abscesses of the parotid gland, a certain number of septicæmias, and not infrequently pneumonia. I wish to draw attention to these complications in typhus as they are interesting in view of some of the many organisms obtained by various observers from the blood of typhus patients. And in this connexion I would like to note how certain epidemics as a whole seem to be marked by a frequent appearance of a particular complication—for instance, the great tendency to gangrene of the feet in the Serbian epidemic. In a bad epidemic in Silesia in 1847 bronchial catarrh and pneumonia were very prevalent complications. These bronchial features are also noted in an Irish epidemic cited by Haeser. Jeanneret-Minkine lays stress in the Serbian epidemic on the well-known feature of the bad condition of the mouth, and considers that many of the secondary infections have obtained their entry by this means. I think that in some of these features we have the explanation of the many cocci, &c., described from the blood of typhus patients, to some of which I must refer in a later part of this account. Jeanneret-Minkine's observations upon the experimental side of our knowledge need not delay our attention.

In passing to the consideration of the actual nature of the virus it is interesting to notice the rapid advance in knowledge that has occurred since 1909, when Nicolle first succeeded in producing experimental typhus in monkeys. Once the investigator can remove a disease from the region of mere description and what one may call police court evidence, into the sterner atmosphere of experiment and scientific deduction, the mystery begins to yield, and although the typhus

problem is not yet by any means solved, we have nevertheless a good deal of knowledge about it. The literature is vast and often contradictory, and I can only claim to have done my best to condense the results of the more important work into the brief space of this discussion. It will be easier to give an epitome of typhus ten years hence than it is to-day, as authors are still at that stirring stage in research when they contradict each other cheerfully upon matters of fact. The earnest disciple, therefore, can have a number of the basic facts relating to typhus according to two or three conflicting doctrines. It is sometimes possible for the critic to arrive at a conclusion from the evidence put forward, but there are many instances in which that is not feasible.

It was clear when Nicolle was able to produce typhus reaction in monkeys by the injection of blood from a patient suffering from the disease that the virus was present in the peripheral blood. Since then monkeys have been used, and of late years also guinea-pigs, as experimental animals in typhus research, and a great deal of our knowledge has been derived from the study and the control of the reaction of these animals under various conditions of experiment. It is rather important, therefore, to consider at once the value of these animal tests. In the case of the monkey I am myself convinced that there is no doubt whatever that the injection of virulent typhus blood produces a marked and clearly defined febrile reaction. It is, however, of the utmost importance that a careful and prolonged observation of the individual animal in regard to its normal thermal chart be made before the test injection. Small variations in temperature are without significance, and many monkeys in captivity run high and irregular temperatures due to causes outside the experiment. These factors must of course be taken into account. One other point can also be disposed of—namely, that monkeys do not in my experience, nor in that of a number of workers, show any febrile reaction to the mere injection of human blood into the peritoneum. I have no personal experience of the typhus reaction in guinea-pigs, but it appears that they also are useful animals though not so susceptible as monkeys; here also the charts require to be considered strictly with respect to the normal chart of the individual animal.

The experimental animal having been found and the whereabouts of the virus determined, the next problem was, in what part of the blood did the infective agent reside, and what was its nature? With this end in view Nicolle, Conor and Conseil carried out experiments in Tunis, and further work was also done by Ricketts and Wilder in America, and also by Anderson and Goldberger.

Nicolle, Conor and Conseil, in 1911, came to the conclusion that the virus was contained in the leucocytes. They considered the plasma, which they found less infective, owed its virulence only to the leucocytes or leucocytic débris from which it is difficult to free the plasma. They further state that the centrifuged blood-serum obtained after clotting is not infective, a result which Anderson and Goldberger do not confirm. Nicolle and his colleagues also give an experiment in which the washed red corpuscles failed to infect, although the blood as a whole was quite virulent. They tested the cerebrospinal fluid, which was clear, and found it devoid of virulence. Anderson and Goldberger repeated Nicolle's experiment, separating the blood elements with great care, and come to conclusions in which they criticize some of the French workers' interpretations. I quote the summary from Anderson and Goldberger, which seems upon the evidence brought forward to be a sound series of deductions (1912, p. 61).

"(1) That the evidence adduced by Nicolle, Conor and Conseil does not especially favour their hypothesis of an intraleucocytic localization of the virus of typhus—on the contrary the infectivity of the centrifugated blood serum obtained after clotting with its low leucocyte content would be in favour of a parasite free in the circulating plasma of the blood; (2) that the blood serum of virulent typhus blood is constantly infective, whether obtained from defibrinated blood or after clotting, instances of its apparent avirulence being explicable by a natural resistance of the monkey; (3) that it may perhaps be possible to deprive typhus blood serum (obtained after clotting) of its virulence by prolonged centrifugation, but that this does not necessarily indicate an intraleucocytic localization of the virus; and (4) that repeated washings of the blood corpuscles do not deprive them of their infectivity, a fact explicable by the physical phenomena involved in centrifugation."

In connexion with this question of the localization of the virus in the blood, Anderson and Goldberger carried out a pretty experiment with citrated blood from the heart of a guinea-pig infected with anthrax. They centrifuged the citrated blood and made plates from measured quantities of the separated elements. The distribution works out as follows: "Whole blood," 100 organisms; "centrifuged plasma," one organism; leucocyte cream, 10,000 organisms; "reds," ten organisms. In this case where there is an organism free in the plasma, its relative distribution in the different blood elements agrees exactly with the relative infectivity put forward by Nicolle and his colleagues for the same blood elements in virulent typhus blood.

Valuable work done by Ricketts and Wilder, and independently confirmed by Anderson and Goldberger, shows that the virus is

contained in the serum obtained by centrifuging the defibrinated blood and also that filtration through a Berkefeld candle robs the serum of its infectivity. This experiment, carried out by Ricketts and Wilder in February, 1910, is carefully documented. The serum was divided; one part inoculated without filtration into a monkey produced a typical attack of typhus; the filtered portion produced no effect whatever. This animal is subsequently tested for susceptibility and shows a perfectly definite typhus reaction. Wilder, in addition, repeated his experiment with the following result: the monkey which received the unfiltered serum showed a typhus reaction, while the animal which received the filtered material remained quite unaffected. When, however, both monkeys were subjected to an immunity test at a later period both were found to resist the attack; the control monkeys to this experiment showed the usual typhus infection. The resistance of the monkey which had received the filtered serum can be explained by one or more hypotheses which the author states, without however selecting the one he prefers.

(1) The monkey may have been naturally immune.

(2) The animal may have been immunized by the filtered serum.

In this connexion Wilder points out that there are variable factors in filtration experiments: for instance he used a different candle, though of the same make and number; also the suction pressure was not controlled. Therefore in the second experiment products may have come through which were excluded in the first instance. In addition the two experiments were made from the blood of different patients who were not at the same stage of the disease. Wilder also suggests that the organism may be pleomorphic and may differ in size at different stages. And he finally adds that fragments of the organism or toxins may have come through which were capable of immunizing the animal.

Nicolle, Conor and Conseil, in their paper of October, 1910, have also one experiment in which a monkey which had received filtered serum from clotted typhus blood showed itself immune to a subsequent infection of virulent typhus blood. This animal (No. 47) exhibited a small rise of temperature, of however less than 1° C., between the fifteenth and twenty-first day after receiving the filtered serum. The French workers look upon this as a positive experiment. In several other attempts the filtered serum gave uniformly negative results. It must be noted that Wilder in all his work is not sufficiently on his guard against the important factor of a natural immunity against typhus either temporary or permanent in a monkey, and Nicolle and

his colleagues do not consider this possibility at all. It is to the credit of Anderson and Goldberger that they did become alive to this very cogent feature, and their experiments reveal a tolerably frequent occurrence of animals who showed a natural immunity against the disease. They state that "22·5 per cent. of rhesus monkeys possess at least a transient natural immunity, and it seems reasonable to consider that in about 3·5 per cent. of monkeys the resistance noted amounts to a permanent natural immunity."

The temporarily immune monkey may receive a full dose of infected blood without showing any reaction. They are however not immunized by this, and will react at a second or third inoculation given subsequently. Anderson and Goldberger, who carried out several filtration experiments, point out that Nicolle's so-called filter-passing experiment hinges entirely on this question of the possible natural immunity of the monkey. The American authors have themselves an experiment in which a monkey received a dose of filtered serum to which he did not react; after thirty-four days he received an inoculation of 6 c.c. of virulent blood and showed no febrile reaction. The authors point out that at this stage they might have concluded that the filtrate had immunized this monkey. However they went on and gave another injection of virulent blood in seventy days from the injection of the filtrate and the animal developed a very typical typhus fever. They have another experiment also exactly parallel to this one.

To summarize these points: There is no experiment recorded, as far as I am aware, in which a monkey showed a typical typhus reaction upon the injection of filtered serum from a typhus patient. There are about twelve experiments in which filtered serum produced no febrile reaction and in which the monkey was not immunized by the injection of the filtered serum. The experiments recorded in which filtered serum from typhus patients appeared to immunize a monkey against a subsequent infection, do not exclude the possibility of the monkey being naturally immune. I conclude, therefore, that the evidence indicates that the virus of typhus will not pass a sound Berkefeld filter.

All the authorities are agreed that one attack of typhus in an experimental animal confers a solid immunity against subsequent attacks.

So much then for the work on the virus treated as a whole, carried out by workers who considered that they could not see it nor cultivate it. I have now to consider some of the work of the people who thought they could either see the infective agent, or grow it, or both. I cannot consider anything like all the views put forward along these lines; I have selected representative examples.

Plotz, in 1915, isolated a pleomorphic bacillus from cases of typhus in New York. He describes his method—the organism is an anaërobe. He isolated the same organism from eight guinea-pigs and one monkey, which were experimentally infected with the virus of typhus obtained from Anderson and Goldberger. Plotz and his colleagues Olitzky and Baehr used their particular method of cultivation of the blood in a number of other febrile conditions such as typhoid fever, tuberculosis, acute osteomyelitis, acute rheumatic endocarditis, &c. They state that "in none of these 198 cases was an organism recovered which in any way resembled the bacillus isolated from cases of typhus fever."

One of these workers, Olitzky, carried out a detailed series of serological tests which I cannot deal with here, and comes to the conclusion that he is dealing with specific agglutinins and precipitins. In the next section of this paper the three authors record experimental work with the virus derived from cases of typhus, or from animal passage, and with the cultures isolated from cases. The animals used were guinea-pigs. They cultivated the pleomorphic anaërobe from a certain proportion of animals inoculated with the typhus virus. To quote their results in their own words: "Of nine guinea-pigs with mild febrile reactions, in only one was the blood-culture positive. On the other hand, in fifteen animals with severe reaction, the blood culture was positive in seven, or almost 50 per cent. This relation between the blood cultures and the severity of the disease was also observed in the human typhus cases."

The weakest part of this work lies in the fact that the cultures isolated proved to be incapable of producing the disease in a quite convincing manner in animals susceptible to typhus. Two cases only are cited where a reaction was produced upon injecting the culture into guinea-pigs, and neither can be held to be perfectly conclusive. Subsequent experiments were absolutely negative, which the authors explain along the usual lines, that the subculture on artificial media robs the organism of its virulence.

Plotz's work has now been confirmed by Popoff, who investigated an epidemic of typhus in Macedonia; the results were published in the *Deutsche Medizinische Wochenschrift*, in April, 1916. He repeated Plotz's work, using precisely the same method, and obtained an organism which tallied with that described by the American authors. Blood cultures were most likely to be successful if made before the crisis and at the height of the rash. He then proceeded to test the sera of typhus patients for the presence of agglutinins, and found that he

could obtain a very clear reaction with sera taken from the patient at or after the crisis. Agglutinins were not present as a rule in the earliest days of the disease, but developed from the crisis onwards and showed the highest titre about seven to fifteen days after the crisis. Popoff could find agglutinins in the sera of two men who had had typhus ten months previously. Moreover he found that the sera of two men, vaccinated three and four months previously with Plotz's bacillus, agglutinated his strains isolated in Macedonia, thus suggesting that the organism isolated by Plotz in America and by the author in Macedonia were identical in this respect.

Popoff put up satisfactory controls to his experiments and his paper reads convincingly. It is true that it may be objected that the demonstration of an organism in the blood during the height of a disease, and the subsequent development of agglutinins specific to that organism, are not in themselves proof that the said organism is the exciting cause of the disease. Nevertheless in many instances—for example, that of typhoid fever—such evidence would be taken as authorizing a diagnosis which nobody would dispute. It can very reasonably be held that *Bacillus typhi-exanthematici* of Plotz is a secondary invader, but it must be admitted that the evidence is beginning to accumulate in favour of this organism. I have given Plotz's work in full, as in conjunction with that of Popoff it seems to constitute the best documented case of any of the organisms grown from typhus blood. Typhus has a bad reputation and pathologists and bacteriologists have a certain impatience with any new claimant in the way of an organism said to be the exciting cause of this disease. If typhus had a clean slate and started as a new condition about which we had no views, Plotz's work would be treated much more seriously and Popoff's confirmation would add the weight of an independent repetition of the result. It may also be noted that a minute organism free in the plasma would fit in with the experiments of Anderson and Goldberger as to the seat of the virus in the infected blood. Plotz obtained further confirmation from Olitzky, Denzer and Husk, who repeated his work in Mexico with similar results. There are some interesting points in this paper, but I cannot go further into them at present. Plotz's bacillus was grown from the presumably infected lice.

I must now consider some of the other organisms put forward as playing a part in typhus.

Rabinowitsch has consistently grown a cocco-bacillus by aërobic methods from the blood of typhus patients—he considers it to be the

causal agent of the disease. I think, as in so much of the work along these lines, that he puts up a very good case for the presence of the organism, but the causal connexion between it and the disease does not appear to me to be established.

Wilson grew cocci from the blood of typhus patients in Ireland, but considered them to be secondary invaders.

Penfold obtained a hæmophilic diplococcus also from cases in Ireland, and I have myself confirmed this result, using his method. One point however came out, that out of the two cases the first gave very few colonies, but the blood was quite virulent when injected into six monkeys, the incubation period being six days; the second patient was past the crisis, and the monkeys failed to react to the injection during nineteen days, when the observations were given up owing to circumstances beyond my control—i.e., the Sinn Fein rebellion—but the blood cultures gave many more colonies than in the case of the first patient. This is, I think, evidence on the side of the coccus being a secondary invader.

Hort and Ingram, in a brief note in 1914, claimed that the exciting cause of typhus fever was a pleomorphic aërobic bacillus which they described somewhat comprehensively to be the same as Plotz's organism and as that described by Rabinowitsch. This seems scarcely probable as the cultural characters seem somewhat divergent.

Hegler and Prowazek (1913) carried out some investigations into typhus. Prowazek noted, in fifty-one cases examined, alterations in the polymorphonuclear neutrophil leucocytes. These changes were not seen in cases of measles and amœbic dysentery, &c., investigated at the same time. He observed fragmentation of the nucleus and in addition round or oval bodies which are sometimes double; these bodies are well-defined, with sharp contours, and stain an intense carmine colour with Giemsa's method. The bodies are never internuclear. Prowazek distinguishes these appearances from Doehle's bodies. The bodies appeared in the leucocytes about the third day of the disease; they persisted after defervescence and were present in one case as late as the nineteenth day of the illness. Monkeys infected with typhus showed these bodies in their leucocytes; they were not found in infected guinea-pigs.

We now come to deal with the transmission, and here again the pioneer work was done by Nicolle and his colleagues in Tunis. In 1909 these workers obtained the first transmission of typhus from monkey to monkey by means of body lice. The details of the experiments are as follows: The virus was obtained from a patient on May 19, on the

third day of his illness and at the time of the appearance of the rash, and inoculated into a chimpanzee. After an incubation of twenty-four days this animal showed a febrile reaction corresponding in type with that of typhus. Blood from this chimpanzee was inoculated into a *Macacus sinicus*, which developed a typical attack of typhus. Body lice, twenty-nine in number, were fed on this monkey on the sixteenth day after inoculation. The next day, and for the following days until the lice had all died, they were allowed to feed on two bonnet monkeys, A and B. Monkey A was bitten by lice varying in number from fifteen to three. Monkey B was bitten for fourteen days by lice varying from fourteen to three in number. The lice were kept between the feeds at a temperature of 16° to 20° C. Monkey A showed no change for twenty-two days when there was a rise in temperature. The fever remitted slowly from the thirtieth day to the thirty-fourth; there was a second attack of fever on the thirty-ninth day, and the monkey died on the forty-fourth day. While this is not a perfectly regular typhus chart it must be noted that fevers of this type may be produced by the direct inoculation of the virus into monkeys. Monkey B had a long incubation period—namely, forty days—during which the chart of its temperature ran in a perfectly normal manner; there was a febrile reaction from the forty-first to the forty-sixth day, and the temperature declined by crisis. The passages of blood made from monkeys A and B on to other monkeys did not give rise to any definite typhus reaction, and the authors call them "réactions febriles avortées." They attribute their lack of success to the lowering of the virulence by passage.

Ricketts and Wilder in 1910 made experiments similar to those of Nicolle and found that they could immunize monkeys against infection with virulent blood by the bites of infected lice. In a later paper Wilder (1911) gives the results of further work upon this question. Eight monkeys were subjected to the bites of lice previously infected by feedings on human typhus patients. Five of these were positive and the monkeys were immunized by this exposure to the bites of the insects, although to quote the author's own words: "In none of our insect experiments were we able to provoke a very characteristic febrile reaction, although slight fever was observed in nearly every case, but when a monkey is exposed to the bites of infected lice he is thereby immunized to typhus fever, so that he proves refractive when later injected with virulent blood."

Wilder carried out rather an important experiment as follows: Six

lice were allowed to feed three times at intervals of twenty-four hours on a typhus patient during the eighth, ninth and tenth day of his illness; the lice were then kept for forty-eight hours at a temperature of 11° C. to 12° C. without food. The intestinal contents of these six lice were introduced into scarifications of the skin of monkey No. 32. The animal showed no reaction, but he had developed a solid immunity and failed to react to the injection of 4 c.c. of virulent blood; the control monkeys reacted quite typically. The interesting features in this case are that it shows that the lice became infected some time between the second and fifth day after feeding; it is also pretty clear that the causal agent must have multiplied in the lice in that time as the quantity of blood contained in the insects during the experiment cannot have exceeded 0·06 c.c., which is much below the lowest infecting dose of the most infectious blood. The minimum infecting dose lies in direct passage of typhus blood between 0·2 c.c. and 1 c.c. of defibrinated blood. It must, however, be remembered in considering the results of this experiment that Wilder was not sufficiently alive to the possibility of natural immunity in the monkey, and one would like to see this piece of work repeated.

Wilder made an interesting investigation by rearing the eggs deposited by infected lice. The young lice were never themselves directly exposed to infection. These young lice were allowed to feed upon a normal monkey (No. 42), which was subsequently tested for immunity. Wilder considers that this monkey was rendered immune, but the chart he gives on p. 89, which shows the temperature of the monkey after receiving the virulent blood which constituted the immunity test, is not quite satisfactory, as there is a slight febrile reaction on the ninth day. Wilder considers that this reaction is so much slighter than that of the controls that he concludes that the monkey was immune, and "owed his immunity to his previous infection by the young lice, and that hereditary transmission of the infectivity of the louse is established to the extent of reasonable probability." Looking at this result of Wilder's with the eye of criticism, one cannot help considering that the evidence, though suggestive, is rather slender to establish so important a point. It should be noted that Anderson and Goldberger failed to obtain any evidence of hereditary transmission of the virus in the louse; their experiments with lice hatched from presumably infected mothers being definitely negative.

Anderson and Goldberger made experiments in the transmission of typhus by lice which corroborate and extend those of Wilder; they

conclude that both *Pediculus vestimenti* and *Pediculus capitis* may become infected with typhus, and that the infection may be transmitted by subcutaneous injection of the crushed insects or by their bites. Anderson and Goldberger failed to transmit the virus of typhus by means of the bites or by the subcutaneous injection of crushed bed-bugs.

In considering the value of all these experiments on transmission detailed above, it is clear that the actual experimental demonstration hinges upon the question of the immunity of the monkey. Natural immunity occurs and is a factor which obscures the result, nevertheless the number of experiments is sufficient to render the question virtually settled. One curious feature calls for notice—namely, that as Anderson and Goldberger show very clearly, a full dose of virulent blood if it fails to produce a febrile reaction also fails to immunize. That is to say, that in direct passage of the virulent blood there is no immunization unless there is a febrile reaction. Now in the transmission by lice the monkey is apparently immunized without the production of a febrile reaction, and the blood of such a monkey if injected into another monkey fails to produce infection. This is rather a curious discrepancy which still needs clearing up.

Before concluding I should like to refer to the quite recent work of Sergent, Foley and Vialatte, and also to that of Rocha-Lima. Sergent and his colleagues describe cocco-bacilli from lice from cases of typhus; they note their affinity for Giemsa's stain and a tendency to bipolar staining. They conclude that if these cocco-bacilli do not constitute the virus of typhus themselves one may consider that they are, as is the case with several Pasteurella, microbes which are witnesses that accompany the true and invisible agent of the infection. Rocha-Lima, in 1916, found in 95 per cent. of lice from patients in a prison epidemic in 1914, large numbers of bodies which took on a red colour with Giemsa's stain. He thought they rather resembled bacteria without being absolutely the same in appearance. Prowazek examined these bodies and considered them to be the same as those found by him in the blood of the patients to which reference has been made above. In handling the lice both Prowazek and Rocha-Lima contracted the disease, of which Prowazek died. The shape of these structures varies from short elliptical coccus-like bodies to definite rods. The usual bacterial stains, such as methylene blue, carbol fuchsin, and carbol thionin, fail to colour the bodies satisfactorily; Loeffler's flagellar stain, carbol gentian violet, and in particular Giemsa's solution, stain the bodies very clearly. The author is convinced that these bodies are micro-organisms. Cultiva-

tion of the organisms on agar and ascitic agar failed. Rocha-Lima has eight instances in which he produced typhus in guinea-pigs by injection of the contents of lice. In two cases he injected part of the gut contents of a single louse after having assured himself by microscopical examination that the material contained the suspicious bodies. This louse-virus had been kept going for nearly a year by passage from guinea-pig to guinea-pig. At the eleventh passage it was injected into a monkey, which exhibited a typical typhus reaction. This louse-virus immunized against the infection with the virus derived from patients. The author admits that bodies similar to those described may very exceptionally be met with in lice which are not derived from patients infected with typhus.

Rocha-Lima extends his work in a later paper (May, 1916), and cites some interesting experiments. He took lice which were presumably normal and let a certain number of them feed on typhus patients, the rest he fed on normal people under similar conditions. Those fed on the typhus patients developed the bodies described above, the other group did not show them. The author now names these bodies *Rickettsia prowazekei;* he is of the opinion that they belong to the Strongyloplasmata and should be looked upon as Chlamydozoa. The parasites are located in the cells of the gastro-intestinal tract of the lice. Cultural attempts made aërobically and anaërobically upon a large variety of media have so far failed.

When Rocha-Lima kept the lice at 23° C. between the feeds upon the infected patients, the Rickettsia did not develop, and the injection of the material into guinea-pigs did not produce infection. If, however, they were kept at 32° C., the Rickettsia did develop and the injected lice produced infection in guinea-pigs. Rocha-Lima found that the lice were not infective till the fifth day after feeding on infected blood. It will be noted that his results are in direct contradiction in regard to the optimum temperature at which to keep the lice to those of Nicolle, Comte and Conseil, of Ricketts and Wilder, and of Anderson and Goldberger. Rocha-Lima states without protocols that he has had positive results in experiments dealing with hereditary infection in lice. The larvæ hatched from eggs laid by a mother on the sixth day after feeding on typhus-blood have shown themselves to be infected. Noeller tried feeding pig-lice on typhus patients, and at the same time let human lice suck the same patient's blood. The Rickettsia developed in the human lice and not in the pig-lice.

I cannot touch for lack of time upon the application of serum-therapy to typhus; work along these lines has been carried out by Nicolle and his colleagues in North Africa.

BIBLIOGRAPHY.

ANDERSON and GOLDBERGER. "Collected Studies on Typhus," *Hyg. Lab. Bull.*, No. 86, U.S.A. Health Service, 1912.

CALDWELL. "The Epidemic of Typhus exanthematicus in the Balkans and in the Prison Camps of Europe," *Journ. Amer. Med. Assoc.*, 1916, vi, lxvi, p. 326.

CHAUFFARD. "De l'étiologie du typhus exanthématique," *Bull. de l'Acad. de Méd.*, 1872, 2me sér., i, p. 1008.

HAESER. "Geschichte der Medicin," Jena, 1882, 3rd ed., iii.

HORT and INGRAM. "The Etiology of Typhus," *Brit. Med. Journ.*, 1914, ii, p. 15.

HEGLER and PROWAZEK. "Untersuchungen über Flecktyphus," *Berl. klin. Wochenschr.*, 1913, l, p. 2035.

JEANNERET-MINKINE. "Le Typhus exanthématique," Par., 1915.

MURCHISON. "A Treatise on the Continued Fevers of Great Britain," 1884, 3rd ed.

NICOLLE, CH. "Réproduction expérimentale du typhus exanthématique chez le singe," *Compt. rend. Acad. des Sci.*, 1909, cxlix, p. 157.

NICOLLE, COMTE, et CONSEIL. "Transmission expérimentale du typhus exanthématique par le pou de corps," *Compt. rend. Acad. des Sci.*, 1909, cxlix, p. 486.

NICOLLE et CONSEIL. "Données expérimentales nouvelles sur le typhus exanthématique," *Compt. rend. Acad. des Sci.*, 1910, cli, p. 454.

Idem. "Propriétés du serum des malades convalescents et des animaux guéris de typhus exanthématique," *Compt. rend. Acad. des Sci.*, 1910, cli, p. 598.

NICOLLE, CONOR, et CONSEIL. "Propriétés du virus exanthématique," *Ann. de l'Inst. Pasteur*, 1911, xxv, p. 13.

Idem. "Le typhus expérimental du cobaye," *Compt. rend. Acad. des Sci.*, 1911, clii, p. 1638.

NICOLLE, BLANC, et CONSEIL. "Quelques points de l'étude expérimentale du typhus exanthématique," *Compt. rend. Acad. des Sci.*, 1914, clix, p. 661.

NICOLLE et BLAIZOT. "Sur la préparation d'un serum antiexanthématique expérimental et ses premières applications au traitement du typhus de l'homme," *Compt. rend. Acad. des Sci.*, 1916, clxii, p. 525.

NOELLER. "Beitrag zur Flecktyphusubertragung durch Lause," *Berl. klin. Wochenschr.*, 1916, liii, p. 778.

OLITZKY, DENZER, and HUSK. "The Etiology of Typhus Fever in Mexico," *Journ. Amer. Med. Assoc.*, 1916, lxvi, p. 1692.

PENFOLD. "Etiology of Typhus," *Trans. Soc. Trop. Med. and Hyg.*, 1916, ix, p. 105.

PLOTZ, OLITZKY, and BAEHR. "Etiology of Typhus exanthematicus," *Journ. of Infect. Dis.*, 1915, xvii, p. 1.

POPOFF. "Über den Bacillus typhi-exanthematicus, Plotz," *Deutsch. med. Wochenschr.*, 1916, xlii, p. 471.

RABINOWITSCH. "Zur Aetiologie des Flecktyphus," *Arch. f. Hyg.*, 1909, lxxi, p. 331.

Idem. "Experimental Typhus in the Guinea-pig and Immunization against it," Abstract in *Trop. Dis. Bull.*, 1916, viii, No. 1, p. 63.

RICKETTS and WILDER. "The Typhus Fever of Mexico," *Journ. Amer. Med. Assoc.*, 1910, liv, p. 463.

Idem. "Transmission of the Typhus Fever of Mexico by means of the Louse (*Pediculus vestimenti*)," *Journ. Amer. Med. Assoc.*, 1910, liv, p. 1304.

Idem. "The Etiology of Typhus Fever," *Journ. Amer. Med. Assoc.*, 1910, liv, p. 1373.

ROCHA-LIMA. "Beobachtungen bei Flecktyphuslausen," *Arch. f. Schiffs- u. Tropenhygiene* 1916, xx, p. 17.

Idem. "Zur Aetiologie des Fleckfiebers," *Berl. klin. Wochenschr.*, 1913, liii, p. 567.

SERGENT, FOLEY, and VIALATTE. "Sur les formes microbiennes abondantes dans les poux infectés par le typhus exanthématique et toujours absentes dans les poux témoins non typhiques," *Compt. rend. Soc. de Biol.*, 1914, lxxvii, p. 101.

WILDER. "The Problem of the Transmission of Typhus Fever," *Journ. of Infect. Dis.*, 1911, ix, p. 1.

DISCUSSION.

Dr. BROWNLEE: Before proceeding to discuss the various points in this paper I have to thank Miss Robertson personally for the trouble and care with which she has synopsized and discussed the work on typhus fever which has been carried on during the last few years. One of the great needs in science at the present day, with so many workers and so great a number of journals, is the periodic issue of such summaries. The history of typhus fever is especially difficult in view of its confusion with other diseases. For instance, the epidemic of fever in Scotland in 1843, which had a very low mortality, was in all probability relapsing fever, while that in Flanders in 1846, with a very high mortality, was equally likely cerebrospinal meningitis. Both these fevers were confused with typhus fever at that date, and still later cerebrospinal meningitis was not differentiated even by such an accomplished clinician as was Dr. Murchison. Among the interesting points noted by Miss Robertson regarding the epidemics in Serbia, one of the most important is the frequency with which complications such as parotitis, gangrene, &c., occurred. I am of the same opinion as Dr. Jeanneret-Minkine, the observer, that secondary infections are largely due to want of nursing care. I have seen a good deal of severe typhus fever and have had throughout the experience of finding it a disease with almost no complications; parotitis though occasionally present on admission in neglected cases, never developed in the hospital, and though local patches of gangrene occurred on the feet and legs during some epidemics, these rarely involved more than the skin, and never developed into extensive sloughs. With regard to susceptibility to typhus there is no doubt that racially there is considerable variation of immunity. The Irish both in Glasgow and London were attacked by the disease in greater proportion to their numbers than the native inhabitants, though possibly this may be explained to a certain extent by the more insanitary surroundings in which they lived. The chief value of the recent work on typhus lies, I think, in the proof that the disease is spread by lice, at least in the majority of cases. The theory that vermin spread typhus is not new. It was an idea always present to the Health Department of Glasgow. Dr. Russell told me that during the whole of his experience as Medical Officer of Health he had known no case of infection in the West End of Glasgow. Many persons residing in the West End, such as house-factors, physicians, clergymen, &c., whose work led them into the typhus fever areas, contracted the disease, but in no case was there any spread to their families or to the nurses in attendance. When, in 1895, I joined the staff of the Fever Hospital in Glasgow, a very long stethoscope used to be given to physicians who wished to examine the chests of the typhus fever patients. This stethoscope was constructed to the order of a member of the staff, now dead, who said "that the stethoscope used for examining a typhus patient should be longer than the longest leap of the strongest flea.". Miss Munro, the matron of the small-pox hospital at that time, who nursed

through all the typhus epidemics of the sixties, a woman of very great natural powers of observation, and one whose opinion on the diagnosis of small-pox we were not accustomed to set aside without grave searchings of heart, was very clear that typhus was spread by lice, and from about 1901 this view I consistently taught to my fever students as the most probable. That the spread was due to vermin seemed almost conclusively proved by the epidemiological evidence. An important point is whether lice are able to pass on the parasite to their offspring, as is known to be the case with some other protozoal diseases. The fact that in the old days it was recognized that typhus fever convalescents were capable of spreading the disease under certain circumstances is in favour of this idea. An instance which occurred under my own observation also suggests this. One of my nurses was away on holiday when typhus fever broke out; she had been nursing that disease on and off for twenty years without contracting the disease, and was considered immune. On returning from her holiday she was sent to the typhus fever ward as a matter of course, and there developed the fever about fourteen days after her return. The attack began severely, but her immunity was real, for the crisis occurred on the ninth day. In this case all the patients were convalescent on her return to duty, and in view of the very thorough cleaning typhus patients receive on their admission to the hospital, any lice present must have been of the second generation, hatched out from nits on the body hairs. If there is no other means of communicating the disease than through lice, this must, I think, be considered a case of mediate infection. With regard to the species of louse which can spread the disease, I am strongly of opinion that it is only the body louse, and that the head louse can have little or no part in the process. With regard to the parasites of typhus fever I think that probably the work of Prowazek and Rocha-Lima is most likely to be found valid. The diplococcus described by some as the parasite is easily enough found in the blood but can have no ætiological relation. Similar organisms are also easily found in other conditions in which it is known that they are at most secondary infections. The bodies which Prowazek describes as contained in the leucocytes, and which stain crimson with Giemsa's solution, seem a more likely explanation. These bodies are possibly identical with bodies I observed in the leucocytes in the spleen of those dead with typhus many years ago. The bodies observed, however, were much larger than those described by Rocha-Lima, though the staining reactions were apparently identical. If of the same nature, therefore, the bodies I saw represent groups of the organism, and not single organisms. This is possible, as the examination of the spleen of patients dead from fever like typhus can rarely be carried out sufficiently soon after death to permit of good cytological observation.

Lieutenant-Colonel E. W. GOODALL: During the last twenty-five years I have seen several cases of typhus, but, as these cases have occurred at intervals and do not constitute epidemics, and as also at the present time I am away from home and unable to refer to my notes, it is difficult to

enter fully into a discussion of the epidemiological bearings of the experimental work. While, in general terms, I can say that the clinical and epidemiological facts within my knowledge agree with the view that pediculi are important transmitting agents of the disease, I am disinclined to hold that we have here the full explanation of the facts. In particular, I doubt whether *Pediculus capitis* is an important cause of infection; certainly cases have been observed in which this insect is unlikely to have been present. A small group of hospital cases came under my notice which illustrate this point. Speaking from memory the facts were, I think, as follows: Some twelve or fourteen years ago there were under my care at the Eastern Hospital three children under 14 years of age suffering from typhus. They were two boys and a girl. The girl and one of the boys were in the febrile stage of the disease, the other boy was convalescent. They were placed by themselves in a roomy, well-ventilated ward of three beds. Both the boys had been admitted from a general hospital, in which they had been treated as in-patients for some days. The girl had been in another ward at the Eastern Hospital for two or three days; that is to say, they had all three of them been subjected to the routine cleansing process which is usual in hospitals, and I think it is fairly certain they were free from pediculi of any description by the time they were moved into the special ward at the Eastern Hospital. Yet one of the nurses who was charged with their care—the night nurse—caught typhus, and had a severe attack. These three cases were part of a group of at least six, of which one, the medical practitioner who first attended the patients in their home, was fatal. Instances such as these incline me to believe that typhus may be transmitted by other means than lice, at any rate occasionally, just as plague of the pneumonic form may be transmitted directly from the lungs of a patient and not by means of the rat and the flea.

Major A. G. R. FOULERTON: I am doubtful as to the value of the earlier statistics in the matter of the case-mortality of typhus fever. In the past there has been much confusion of typhus, typhoid, and relapsing fevers, and epidemic cerebrospinal meningitis. During the war now in progress the case-mortality of typhus fever amongst the Serbian Army in the Field has been 50 per cent.—about the same as the case-mortality was amongst French troops in the Crimean War. It is stated that the case-mortality amongst Serbian prisoners of war, in Austrian and German hands, has been 25 per cent. in uncomplicated cases, and about 40 per cent. in an apparently large group of cases in which typhus fever and diphtheria were intercurrent. The Austro-German armies have had a considerable experience of typhus fever during the war, and a noticeable feature has been the frequent association, in the same patient, of typhus fever with other infective disease—influenza, diphtheria, dysentery, malaria, and typhoid and relapsing fevers. This varied association of infections is doubtless the result of cross-infection in hospital by the agency of typhus-infected lice. Similar cross-infection has come under my notice in connexion with an outbreak of another louse-carried disease,

relapsing fever. The relative frequency of double-infection is an explanation, in part, of the variety of the parasites obtained on examination of the blood from cases of typhus fever. It is difficult to understand, in view of the results of exact experimental work, and with the knowledge that has been gained during the war, how the importance of insect parasites in the transmission of the unknown virus of typhus fever can be questioned. The spirochæte of relapsing fever is certainly carried by ticks, by lice, and possibly by other insect parasites; it is equally certain that the louse is a carrier of the virus of typhus fever, and, so far as knowledge extends at the present time, probably the only common carrier. It is impossible to exclude any possibility of other means for the transmission of typhus infection; but the modern teaching is that a patient with typhus fever who has been freed from lice is no longer a source of danger to others. Nicolle has recently related some details of a criminal experiment, as to the transmission of typhus infection by lice, that came within his knowledge. A warder in a colonial prison maliciously transferred lice from a prisoner with typhus fever to two healthy individuals; both became infected. Professor Jürgens referred, at a joint congress of Austro-German army medical officers which was held at Warsaw in May, 1916, to an experiment carried out by the Germans. Twenty healthy men were confined closely with twenty men who were suffering from typhus fever, but who had been freed from lice, without any spread of the infection. Similar evidence is afforded in the experience of the Russian prisoners-of-war camps in Germany; when the prisoners were freed from lice the previous heavy prevalence of typhus fever disappeared. Again, preventive measures which were directed exclusively to the freeing of certain infected units from lice proved completely effective for the prevention of a danger, threatening at one time, of the introduction of typhus infection amongst allied troops on the Front in France. The seasonal prevalence of typhus fever correlates with the seasonal prevalence of lousiness as observed amongst troops in the field. In the colder months the men put on more clothing, are less inclined to wash themselves thoroughly, and sleep closely together in billets. In the summer months, less clothing is worn, personal washing is more thorough, the men bivouac out whenever they can; and lousiness amongst the troops is distinctly diminished. And doubtless seasonal conditions influence lousiness amongst the civil population in a similar way, so that in temperate climates typhus fever is a disease of the colder months. I believe that the same considerations apply in relapsing fever. In times of famine and privation, also, personal cleanliness is liable to be neglected; and so opportunity for the spread of typhus infection occurs. I do not think that the fact that a skin eruption rarely occurs amongst typhus-infected monkeys and guinea-pigs suggests in any way that the infection transmitted to them is not that of typhus fever; a skin eruption is not by any means always present in human cases. For the rest, Nicolle and Blaizot kept two strains of the virus alive for two years by passage through series of guinea-pigs and monkeys. At the end of two years the blood of an infected guinea-pig injected into a healthy guinea-pig produced precisely the same signs

of disease as those produced by inoculation either with the blood of a case of typhus fever, or with an emulsion of the bodies of lice from a case. The same pathologists have shown that the virus is retained in most of the tissues of infected guinea-pigs after all blood has been removed by irrigation of the vessels with normal saline solution. It does not appear to be probable that the Plotz-Baehr-Olitsky bacillus has any causative relationship to typhus infection. The bacillus does not produce the symptoms of typhus infection in the guinea-pig, nor does inoculation with the bacillus protect the guinea-pig against infection by typhus blood. Also, it is highly improbable that a strictly anaërobic organism can cause a disease of the nature of typhus fever. On the other hand, there appears to be a high probability that the organism, termed provisionally *Rickettsia prowazeki*, which has been identified by Toepfer and Rocha-Lima in the intestinal epithelial cells of lice from cases of typhus fever, represents a phase in the evolution of the virus. Stempell has seen in the intestinal epithelium of infected lice small oval brownish bodies which may be identical with Rickettsia, as described by Ricketts and Wilder, Prowazek, Toepfer, and Rocha-Lima. The "coccobacillus" which Sergent, Foley, and Vialatte have identified in stained films of blood from cases of typhus fever, and also in films of the body juice of lice from cases of the disease; but which apparently they have not succeeded in growing on artificial media, possibly comes into the same category. The evidence as to the causative relationship of the louse-bodies to typhus infection is fairly substantial. Somewhat similar bodies have been described as occurring occasionally in the intestinal contents of normal lice; but they have never been found within the epithelial cells except in lice from cases of typhus fever. According to Rocha-Lima, the parasite appeared in the epithelium of only a few of a number of artificially hatched lice by the fourth day after they had fed on a patient with typhus fever; on the fifth or sixth day after feeding about 50 per cent. of the lice showed the alleged parasites; by the eighth and ninth days the proportion of the number of lice showing the parasite to the number of those free from it was about the same as obtained amongst lice taken freshly from a case of typhus fever. The inoculation of an emulsion made with normal lice was without effect on the guinea-pig: inoculation with an emulsion made with lice containing these bodies was followed by a sequence of symptoms exactly similar to those following inoculation with blood from a case of typhus fever; and the animal was protected afterwards against the effects of inoculation with virulent typhus blood. If Rickettsia represents a phase in the evolution of the parasite of typhus fever, it appears to be probable that the parasite is a protozoon. What is known as to the transmission of the typhus virus is in accordance with what is known as to the transmission of certain other protozoal infections from man to man through a necessary intermediate insect host. A comparison of the method of the transmission of the spirochæte of relapsing fever and of the unknown virus of typhus fever is of interest. In relapsing fever the transmission of the spirochæte is direct; the louse is only one of probably several accidental insect carriers. The spirochæte is contained in the fæces of the louse, and probably can be

transmitted by the bite. And so, a louse after feeding on a case of relapsing fever, can infect at its next feed on a healthy man. This is not so in the transmission of typhus virus. According to Nicolle, to whom must be credited most of our knowledge in the matter, the louse cannot transmit typhus infection until seven days after an infected meal, and does not transmit infection after the tenth day. In other words, the virus of typhus fever must undergo a phase of evolution, covering seven days, in the louse before it can be transmitted to man; and it loses its activity by the tenth day. Also the virus is contained in the body-juices of the louse, and is not present in the excreta. Nicolle's observations are confirmed, to some extent, by Rocha-Lima's observations just mentioned. Rocha-Lima found that Rickettsia first appeared in the epithelial cells of the louse on the fourth or fifth day after an infected meal, and apparently reached the maximum development on the eighth or ninth day. If Nicolle's observations are correct, then the louse must be regarded as a necessary intermediate host in the transmission of typhus infection, just as some species of *Anopheles* is necessary as an intermediate host in the transmission of malarial infection. The question is one of importance when preventive measures against the spread of infection are under consideration. It is obvious that a quarantine of at least twenty-one days is necessary after the last man in an infected unit has been freed from lice—allowing from seven to ten days for the evolution of the virus in the louse, and from six to ten days for the incubation of the virus in man. And even twenty-one days' quarantine from the date of the last case of typhus fever does not cover every possibility. There is the possibility, suggested by a single positive experimental result, of the transmission of the virus from an infected female louse to the ova. However, the experience of the German army during the war has suggested that the risk of transmission through the ova to man is negligible.

Mr. A. Bacot: I have been rather surprised to hear doubt cast upon the theory of contagion and the spread of typhus by clothes lice. So far as my knowledge and information go, the evidence shows a true bill against *Pediculus humanus* as the normal transmitter of the disease. On the other hand, the possibility of occasional transmission by the bed-bug should not be overlooked. The secretive habits and long life apart from the body of its host of *Cimex lectularius* would, supposing it to be implicated, afford a ready explanation of occasional gaps between primary cases and subsequent infection, such as that occurring in the case mentioned by Dr. Brownlee, apart from any question of hereditary transmission through the nits of the louse. A question which I quite expected to hear discussed was the exact relation between famine and destitution and the course of the disease. Is this direct—i.e., by way of lowering the resistance of the population? Or indirect, due to misery and destitution resulting in more favourable conditions for the increase and spreading of the infective agent, the louse? Miss Robertson's reference to the fact that there is a rise in the incidence of the disease in Ireland between the exhaustion of the stock of potatoes from the previous year and the availability

of the new crop, occurring as it does in the warm period of the year, favours the theory of the direct as against the indirect action of destitution. On the other hand, her reference to the fact that destitution increases the incidence of relapsing fever as well as that of typhus favours the indirect explanation, because the factor common to the two disease organisms is that they both gain foothold in their host by way of the louse.

Captain M. GREENWOOD: With reference to the question asked by Mr. Bacot as to the relation between famine and typhus, it is to be remarked that epidemiological history furnishes more than one instance of famine without co-existing typhus. An instructive example is afforded by the case of Schomberg's army before Dundalk in the autumn of 1689. According to Macaulay, the commissariat arrangements of the whole army were inadequate, but the Dutch troops, who were well-disciplined and well-hutted, lost only eleven men, while the English, who were raw, undisciplined recruits, and so dirty that, in the words of a contemporary, "many of them, when they were dead, were incredibly lousy," suffered severely both from typhus and dysentery. Again, in the epidemic of 1817-18, the island of Rathlin, which was as famished as the mainland, had no cases while typhus was ravaging the nearest shores. All the accounts of Irish typhus in the nineteenth century point to the important part played by vagrants in spreading the infection.

Dr. HOWARD BARRIE: During the first year of the war I had the opportunity of observing clinically a large number of typhus patients in Serbia. This widespread epidemic began in the Fourth Reserve Hospital at Uskub, and it was from here that its extent and virulence became known to the world, notwithstanding the opposition of the local Serbian authorities. At the time of its outbreak our wards contained approximately 1,200 patients. From November, 1914, to February, 1915, we had been free from typhus. About February 9 the first case was noted among some recent arrivals who had been transferred from several of the Northern Hospitals. Before the outbreak our patients were incredibly verminous, and the disease, once introduced, continued to spread with alarming rapidity until the vermin were disposed of. My experience does not lead me to suspect either the flea or the bed bug as carriers. There can be no doubt about the body louse being the principal offender in this respect. Possibly the head louse should be placed in the same category, although I am not convinced that the head louse is a carrier. The most decisive proof that one or both these species were entirely responsible for the conveyance was the immediate failure to observe fresh cases when they had been disposed of. The continued presence of the flea and bed bug were noted without additional cases of typhus appearing. Body lice unquestionably show a disposition to flee from infected patients, and in their outward "trek" feed upon the first host on which they find refuge. In this manner fresh cases are infected, and previously infected cases are submitted to an intensified infection. Vermin appear to die during the migration, presumably as a result of the toxins associated with the disease. I have had no reason to suspect any avenue of infection

apart from that afforded by some lack of continuity in the skin. This may be due to a scratch or the bite of a flea or a louse. Possibly the infecting virus is deposited in the excreta of the louse. That the adult louse transmits the disease to its offspring seems impossible. Great as the mortality from typhus appears to be, relapsing fever actually kills more victims. Complications after typhus were remarkably few in our wards. Patients appear to die from failure of the circulatory apparatus. In the absence of vermin the disease is as harmless to attendants as puerperal or other septicæmia. My experience is that the incidence of the disease upon the civilian population of Serbia was much overstated. Curiously enough the Turks were little affected. Frequent bathing may account for this immunity. Among our staff considerable immunity was apparently developed when bathing was frequent. Is it possible that water in itself acts as an efficient disinfectant? A curiously interesting feature observed in the prodromal stages was the sudden and overwhelming intoxication which overtook the victim. A medical colleague started out after breakfast feeling perfectly fit. At noon, while still inspecting billets, he was conscious of a growing incapacity to co-ordinate his muscular efforts, and he was discovered sitting on a doorstep apparently suffering from the effects of alcohol. While incoherent and maudlin he complained of no discomfort. Again, the early onset may be noted in the patient's face hours before he himself is conscious that anything is wrong. Added to the bright eyes and increased vivacity of manner is the peculiar tint of the suffused face, which suggests the tint produced upon a solution of hæmoglobin by carbon monoxide—i.e., a bright cherry-red. Ambulant cases are not uncommon. An Austrian orderly employed in the kitchen reported unfit, and after several hours in bed, during which time he slept heavily, he persisted in walking about a long while, until he finally died of heart failure while crossing a common. In my command we were driven to the use of kerosene as a disinfectant, owing to the absence of any other material. From the time this was employed not a single case of infection occurred among our attendants. Previous to its use, five or six attendants weekly showed signs of the disease. I believe the oil killed the vermin or prevented contact with the body. Both patients and attendants were anointed once daily, or once in two days with neat kerosene. Plenty of fresh air and suitable feeding, combined with good nursing, are effective in destroying the vermin, and will most effectively reduce the mortality of this disease.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

EDITED BY

J. Y. W. MacALISTER

UNDER THE DIRECTION OF

THE EDITORIAL COMMITTEE

VOLUME THE TENTH

SESSION 1916-17

SECTION OF THE HISTORY OF MEDICINE



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1917

Section of the History of Medicine.

OFFICERS FOR THE SESSION 1916-17.

President—

RAYMOND CRAWFURD, M.D.

Vice-Presidents—

HENRY BARNES, M.D.
RICHARD CATON, M.D.
NORMAN MOORE, M.D.
Sir HENRY MORRIS, Bt., F.R.C.S.
Sir WILLIAM OSLER, Bt., M.D., F.R.S.
Sir RONALD ROSS, K.C.B., F.R.S.

Hon. Secretaries—

J. D. ROLLESTON, M.D.
CHARLES SINGER, M.D.

Other Members of Council—

JAMES BERRY, F.R.C.S.
WILLIAM BULLOCH, M.D., F.R.S.
DUDLEY W. BUXTON, M.D.
A. J. CHALMERS, M.D.
ARNOLD CHAPLIN, M.D.
F. W. COCK, M.D.
ALBAN DORAN, F.R.C.S.
DAVID FORSYTH, M.D.
JAMES GALLOWAY, C.B., M.D.
LEONARD G. GUTHRIE, M.D.
E. MUIRHEAD LITTLE, F.R.C.S.
R. O. MOON, M.D.
J. A. NIXON, M.B.
GEORGE C. PEACHEY.
D'ARCY POWER, F.R.C.S.
HERBERT R. SPENCER, M.D.
C. J. S. THOMPSON.
C. E. WALLIS.
F. PARKES WEBER, M.D.
P. MACLEOD YEARSLEY, F.R.C.S.

Representative on Library Committee—

LEONARD G. GUTHRIE, M.D.

Representative on Editorial Committee—

J. D. ROLLESTON, M.D.

SECTION OF THE HISTORY OF MEDICINE.

CONTENTS.

October 25, 1916.

RAYMOND CRAWFURD, M.D. PAGE

Presidential Address: Legends and Lore of the Genesis of the Healing Art 1

November 15, 1916.

D'ARCY POWER, F.R.C.S.Eng., Lieutenant-Colonel R.A.M.C.(T.)

A Revised Chapter in the Life of Dr. William Harvey, 1636 33

JAMES RAE, M.D.

The Medical History of the Exiled Stuarts 59

May 24, 1916.

CHARLES SINGER, M.D., Captain R.A.M.C.

The Figures of the Bristol Guy de Chauliac MS. (*circa* 1430) 71

January 17, 1917.

VINCENT DICKINSON, M.D.

A Case of Cupping Instruments 90

ROBERT STEELE.

A Mediaeval Panacea 93

CHARLES SINGER, M.D., Captain R.A.M.C.

A Review of the Medical Literature of the Dark Ages, with a New Text of about 1110 107

The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

LONDON:
JOHN BALE, SONS AND DANIELSSON, LTD.,
OXFORD HOUSE,
83-91, GREAT TITCHFIELD STREET, OXFORD STREET, W. 1.

Section of the History of Medicine.

President—Dr. Raymond Crawfurd.

(*October* 25, 1916.)

PRESIDENTIAL ADDRESS.

Legends and Lore of the Genesis of the Healing Art.

By Raymond Crawfurd, M.D.

We are apt to regard the history of medicine as a development of only modern growth, yet, if we look carefully into the oldest extant literature, we may see that even then the mind of man was occupied in speculation as to the origin of the art of medicine and in attempts to explain its progress to the stage at which he found it. Thus in the Rig-Veda we find the prevalent belief in the divine revelation of the art tempered by the assertion that, in the selection of food-stuffs and of medicinal simples, animals were the preceptors of mankind. If we then pass on a thousand years to what we may call the threshold of scientific medicine, we shall find a Hippocratic treatise devoted to establishing the position of medicine as a natural corollary of dietetics. So early may we discern a strain of rational empiricism leavening the lump of theurgy and superstition, that bid fair to occupy the whole arena of early medicine.

We see then that ancient literature recognized three springs of medical knowledge—a divine source, an animal source, and a human source—and I propose this evening to advert briefly to the first two of these, leaving the last for consideration on some future occasion.

Hindoo medical legend affords a typical account of the divine exposition of medical knowledge, and of its transference to mankind. Like the Nahuans of ancient Mexico, the Hindoos recognized four successive ages in their chronology. In the first age man remained

virtuous, happy, and free from disease, for had not Brahma, the supreme creative being, given him the four immortal Vedas—the Rig, Yajur, Sama, and Atharva—containing all the knowledge required by mankind during the first age. It should be remarked, in passing, that these Vedas, replete as they are with samples of curative, as well as of preventive medicine, belie an origin in an age exempt from all disease. In the second age man fell away from virtue, and disease appeared, to the curtailment of life and the impairment of memory. In the third age half of the human race was given over to depravity. In the fourth and present age the moral and physical corruption of man grew apace, so that disease flourished exceedingly. These four ages approximate closely to the four Biblical epochs of the primordial bliss of Eden, of the fall of man, of the state of the earth before the flood, and of the age that followed thereafter.

Such compassion had Brahma for man's suffering in this fourth age that he produced a second class of books called Upavedas, or supplementary Vedas, one of which, the Ayur-Veda, was intended to teach the right manner of living, and the prevention and cure of disease, so that man might live happily in this world, in preparation for further happiness in a future state.

The Ayur-Veda, or Science of Life (ayur = period of living; ved = to know) is a sacred medical record of high antiquity, maintaining the general outline of the yet older Atharva-Veda. It shows that medicine in ancient India was part and parcel of theology, and that the same priests as controlled religion controlled also the treatment of disease; it was not till the end of the Vedic period that the physician began to shake himself free of priestcraft and to assume an independent individuality. Fragments only of the original Ayur-Veda survive, preserved in the Shastras or Commentaries on the Vedas. It is said to have consisted of 1,000 sections, each of 100 stanzas. The divine Brahma, with truly human insight, realizing the impossibility of learning so large a work, provided an abridgement in eight parts. (Now-a-days, such is the quality of mercy that, when a System of Medicine is published in no more than seven volumes, it is accompanied by an abridgement in one.)

Divine exponents are the first to handle these divine books of healing in a celestial entourage. Brahma, so say Charaka and Susruta, first instructed the god Dakhsa, the Prajabati, in the Ayur-Veda. This god compiled a book, the Chikitra-Darshana, which he communicated to the two Ashwins, or offspring, of Surja, the Sun. These Heavenly Twins

became the medical attendants of the gods, on whom they performed miraculous cures, and they were authors also of important medical works. Indra, King of Heaven, was so impressed by their wonderful cures that he induced them to instruct him in the Ayur-Veda.

Various legends exist telling of the transference of medical knowledge from heaven to earth. One relates how the Munis, or sacred sages, grieved at the melancholy spectacle of disease, met in council in the Himalayas and decided to ask the help of Indra, to whom in heaven they sent one of their number. Indra duly instructed their emissary in the Ayur-Veda, and on his return he related to the Rishis, or sages, the principles committed to him. In the strength of this knowledge the Rishis succeeded in living in happiness and health to extreme old age, wrought many cures on men, and wrote valuable works on medicine. Foremost in skill was Agnibesa, who instructed practitioners on earth, as the Prajabati did in heaven. Agnibesa's treatise was amended later by Charaka.

The number of different gods associated with healing in Hindoo mythology, is due to the readiness with which, in India, one god supplanted another in popular favour. One other Hindoo legend calls for mention, if only because it recognizes the association of the serpent with medicine: in view of the prevalence of serpent worship in ancient India, it is remarkable that more is not heard of it in Hindoo medical myth. The legend runs that "the Vedas were lost in the Deluge, but were recovered by the great serpent Ananta. At the churning of the ocean by the gods and demons the water of the ocean was converted into milk, and then into butter, from which precious gifts were derived. Among these was Dhanwantari, the physician, or holy sage, the possessor of the water of life drunk by the immortals." Indra instructed Dhanwantari, in heaven, in the Ayur-Veda, so that he practised medicine with great success among the gods. But seeing the many maladies and the misery of mankind he came on earth to cure them. As King of Benares his miraculous cures gained such fame that the divine sages resolved to solicit his aid. Susruta was chosen to be the one instructed by him. To the conservative sentiment of Dhanwantari the Ayur-Veda seemed all-sufficing, but so voluminous that he charged Susruta to abridge and arrange it for easier comprehension. Dhanwantari began by expounding surgery, as formerly there were no diseases among the gods, and only wounds required treatment. His exposition is contained in the six books of Susruta, which seem to have been committed to writing about 500 A.D. The commentaries of Charaka and Susruta

supply the groundwork of the more recent systems of Hindoo medicine; the disciples of Charaka became physicians, those of Susruta surgeons.

The Vedas present the usual blend of prayer and invocation, of magic and spells, familiar in primitive medicine, but through all this flows a thin stream of rational empiricism, touching, on the surgical side, such procedures as the extraction of arrows, the dressing of wounds, amputation of legs, and their replacement by iron substitutes, castration and removal of eyes; and on the medical side the action of numerous herbs and the healing power of water. The oldest, the Rig-Veda, mentions no fewer than 1,001 drugs, of which 760 were herbs. Vedic physicians lived as herbalists in houses, surrounded by gardens of medicinal herbs, but they were instructed to seek knowledge also from shepherds and huntsmen, no doubt in the belief that they in turn will have learnt much from animals. It is noteworthy that many of the instruments described by Susruta are constructed after the form of beasts and birds. The anatomical knowledge of the Vedas is of the most elementary kind, and consists in little more than an enumeration of the chief component parts of the body, while their physiology stagnates in the expansive conception of breath as the source of vital power.

No good purpose would be served by multiplying illustrations of the doctrine of divine revelation of medicine from the records of Egypt, Assyria, Persia, Judæa, and other nations; in essence, they are everywhere alike. Greek legend, however, will repay fuller consideration, for the study of origins serves to throw light on its traditions and symbolism. Among the gods of Greece Apollo, son of Zeus and Leto, is the doyen of healers, while under his guidance Pæon ministered to the medical needs of the gods in Olympus. Other gods and goddesses are endowed with some share of the faculty of healing, thus both at Athens and Epidaurus Athena bore the occasional surname "Health," and Asclepius was said to have received from Athena the blood of the Gorgon, that from the veins on the right side he used for the healing of men and the restoration of life, that from the veins on the left for their destruction. Asclepius is the medium by which Greek medicine came down from heaven to earth. In the earlier legend he stands out as a medical superman, and only later does he assume complete divinity. He was the son of Apollo and Coronis, who was slain by Artemis for infidelity to Apollo. As her body was about to be committed to the funeral pyre Apollo snatched away the child Asclepius (whether by artificial abortion or by Cæsarean section we are not told), and gave

him to Chiron, the Centaur, to instruct in the cure of diseases. One legend has it that Chiron acquired his knowledge from Apollo and Artemis, another from his own observation and experience, gained while hunting on Mount Pelion. Hunters, as we have seen in Hindoo myth, were believed to enjoy special facilities for acquiring such knowledge; certainly acquaintance with the favourite foods of animals must have facilitated approach, and the baiting of traps and pitfalls. Homer tells us that it was Chiron who instructed Achilles in the cleansing and healing of wounds, and Achilles transmitted his teaching to his friend Patroclus. Pindar attributes the use of charms, as well, to Chiron. So great was the skill of Asclepius that we find Castor and Pollux, the Greek Heavenly Twins, insisting on his accompanying the expedition of the Argonauts. Ultimately he acquired the power of restoring the dead to life, but this achievement proved his undoing, for Pluto, in alarm for his kingdom, complained to Zeus, who slew him with a thunderbolt and cast him down to Hades. He left behind him two sons, Podalirius and Machaon, and at least four daughters, Hygieia, Panacea, Egrea, who married a serpent and was changed into a willow, and Jaso: from which of these Hippocrates, who was reputed to be a descendant of Asclepius, was derived, we are not told. The exploits of the warrior surgeons, Podalirius and Machaon, are recounted by Homer, and to his record we need only add, that to Podalirius was attributed the discovery of the art of bleeding. On his return from Troy he was driven by a storm to the shores of Caria, where a shepherd, learning that he was a physician, took him to the king, whose daughter was sick. He is said to have cured her by bleeding her from both arms, while for his fee he received his patient in marriage, along with a rich grant of land.

Asclepius was represented in Greek art with a serpent twined round a staff; a dog and a cock not infrequently figure among his emblems. On a coin of Epidaurus a dog lies beneath the throne on which he is seated. The cock is his habitual offering of sacrifice, and there are the inevitable legends to explain both serpent and dog. Asclepius, it is said, was shut up in the house of one Glaucus, who had sought his aid; while he was deep in thought there came a serpent, carrying a herb in its mouth, and twined itself round his staff. Asclepius killed the serpent, and used the herb to restore the dead to life. The presence of dogs in the sanctuary of Asclepius was explained by a legend that, when an infant, he was suckled by a bitch. According to an inscription found in the temple of Asclepius at the Piræus the dogs were fed on the sacrificial

cakes. If we may credit Sextus Empiricus, the flesh of these sacred dogs was given to the patients to eat as medicine. As we shall see presently, the dog was credited with imparting to man knowledge both of herbs and of the healing of wounds; maybe then its flesh was at one time eaten sacramentally, as embodying the god of healing. There was another legend that Asclepius had been suckled in infancy by a goat, and Pausanias blends the two legends into a story of his being suckled by a goat and guarded by a dog.

According to Ælian, dogs, goats, serpents, oxen and pigs have the power of foreseeing an impending epidemic; it is perhaps worthy of note that three of these animals figure in the legends and symbolism of Asclepius. Inscriptions found in the temples of Asclepius record the healing of blindness by dogs licking the eyes, and the cure, by licking, of a tumour on the neck of a boy. Other gods of healing, for instance the Sumerian Gula and the Babylonian Marduk, are represented accompanied by a dog.

The association of Asclepius with the serpent is of very great interest and importance. The association is not peculiar to him, for his Phœnician counterpart Eshmun likewise has a serpent as his symbol, so has his own daughter, Hygieia, who is represented feeding a serpent from a saucer. In Madagascar, too, Ramahavaly, the god of healing, was held to be the patron of serpents and to employ them in his service, for good or for harm. When his image was carried in public, each of his attendants held a writhing serpent in his hand to inspire awe in the beholders. The Naga (serpent) tribes of Cashmir acquired their medical skill from the serpent, and the ancient Celts by drinking serpent broth. In India it is common to make clay or brazen images of the serpent, and offer sacrifice to them on behalf of the sick, just as Moses in time of pestilence set up a brazen serpent, that all might behold and be cured. The inhabitants of Southern Arabia regarded medicinal waters as haunted by serpent jinn, and in a certain African lagoon dwelt a serpent that relieved madness, and in an Algerian well one that cured sore eyes. There was a prevalent belief in the Middle Ages that the household snake, if not propitiated, would prevent conception.

In the legends of Asclepius we have already encountered the belief that the serpent has knowledge of herbs that can restore the dead to life, and we meet it again in the story of Polyidus. The same belief is current in German, Italian, and Lithuanian folk-lore. A Syrian story tells how the king of the serpents restored three slain men to life by washing them with the waters of life; and Russian folk-lore

represents the serpent as possessing a magic water that heals all wounds, restores sight to the blind, and vigour to the cripple. One Russian tale describes a wonderful garden, in which were two streams of healing and life-giving water, and round it was coiled a serpent. In Eden the serpent knew well the properties of the Tree of Life.

Sacred snakes were kept in the temple of Asclepius and fed with honey-cakes; at Epidaurus certain native snakes were sacred to Asclepius. The temple snakes were credited with effecting cures by licking patients with their tongues. Similarly the blind emperor, Theodosius, received his sight by a serpent laying a precious stone on his eyes, and Siegfried became invulnerable by bathing in the blood of the dragon.

Beside the tales of miraculous cures by serpents there is a crop of legends that seek to explain how man acquired the healing knowledge of the serpent. The serpent, by touching his ears or his lips, makes him to understand the voices of birds, which have the gift of foretelling the future, and the language of plants, so that they disclose to the hearer their secret medicinal properties. That serpents should know the language of birds was not unnatural, for they were believed to be generated from the blood of birds; this idea may be referable to the fact that serpents eat the eggs of birds. Yet another widespread belief held that the medical skill of the serpent could be acquired by eating its flesh; this is merely an illustration of the general belief that the properties of animal, man or god may be acquired by eating the possessor.

Cicero says that the ancients explained the connexion of the serpent with Asclepius, by the fact that, like the healing art, the serpent yearly sloughed its old skin and put on a new, but, in all probability, Asclepius was originally himself a serpent that subsequently became transformed into an anthropomorphic god, for whom conservative religious sentiment preserved the symbol of a serpent. This same conservatism is seen in the custom of sending a sacred snake from an existing sanctuary whenever a fresh one was established. It is not clear why the serpent is represented twined round a staff, just as it was coiled round an apple-tree in the garden of the Hesperides. Can this too be a symbol? —a symbol of some tree-spirit standing yet further back than the serpent in the pedigree of Asclepius. May we bring into line with this the serpent and tree that figure so prominently in the legend and imagery of Buddha?

Zoologists tell us that the serpent is by no means an intelligent animal, despite the fact that it became the legendary embodiment of

knowledge, and of medical knowledge in particular. The serpent of Genesis was more subtle than any beast of the field. The strange sinuous form, the mysterious gliding movement, the annual sloughing of the skin with its annual renewal, the weird fascination of its victims —all these must have served to endow the serpent with the homage of wonder that attaches, often so ineptly, to the unknown; and perhaps the deadly poison of the almost imperceptible bite may have seemed to link it appropriately to the science of herbs, that bring life or death to man.

From this brief consideration of the serpent, to the ancients so much more of a god than an animal, we may pass to the kindred belief that man owed his knowledge of medicine to observation and imitation of the practices of animals. From the evidence of geology we know that the dog was the first animal to be domesticated, and man must have soon observed its habit of licking its wounds, as do also cats and horses. He must have noted again and again how much more readily his dog's sores healed than his own, and not unreasonably attributed this to the licking. It is no matter for surprise then, that we should find this method of healing wounds employed in the temples of Æsculapius, by the tongues of dogs and of serpents. In quite recent years licking of sores by the tongue of a dog was held in repute both in Scotland and France. In the absence of any knowledge of the essential conditions that govern the healing of wounds, it was a natural conclusion that the licking was of therapeutic intent, and akin to the cleansing that was the central feature of the primitive treatment of wounds. There is no certain evidence that licked wounds of dogs heal more readily than those that cannot be reached by licking, and the contrast with the slower healing in man would seem to be due to the wide difference in immunity to sepsis between man and dog, for the dog is highly refractory, even to subcutaneous injection of pyogenic organisms. There seems no reason to suppose that by licking their wounds dogs imbibe toxins that produce specific antibodies which assist them in the struggle against invading organisms, for they appear to have little or no need of such assistance. When one reflects how a child sucks an injured finger, how he puts his hand to a hurt leg or a bruised head, how hard each one of us finds it to keep his fingers from a scab, it seems not improbable that underlying all these tendencies is a common instinct to touch the injured part with tongue or hand, to determine the injury and, if possible, to remove the cause. If in the case of the tongue this should promote healing by the removal of dust and particles

of dirt, this contingent benefit is merely incidental, and does not indicate a definite therapeutic purpose.

There is a very ancient tradition that dogs, when they are ill, eat grass or standing corn to relieve the stomach by vomiting and purgation, and Ælian says that it was from this that the Egyptians gained their knowledge of purgatives and emetics. Aristotle says that wolves do the same, and he might also have included cats. Ælian adds that dogs relieve themselves in this way of worms, and of the excess of bile which causes rabies. The grasses that dogs eat have been identified as *Triticum caninum* (dog-wheat), *Agrostis canina* (brown bent-grass), and *Cynosurus cristatus* (dog-grass). From observation of my own and other dogs I feel sure that dogs do not eat grass exclusively, or even chiefly, when they are sick, and certainly not with the purpose of exciting vomiting or purgation. They are most prone to eat grass immediately on reaching green fields after a spell of town life; as a rule, they cease to eat it, or do so very rarely, after a few days in the country. I have never seen the grass produce actual vomiting; it is usually hawked back, churned up with saliva, and has probably seldom passed far down the gullet; when not regurgitated, I have never known it to cause diarrhœa; any looseness of the bowels is referable to the increased exercise that the dog takes in the country as compared with a town. The number of human beings who chew grass-bents is quite as large as that of dogs that chew grass, and I have an impression that the habit is far more common among town-dwellers visiting the country than among country-folk. The human being certainly chews it because he likes the taste, and avoids swallowing it for fear of exciting vomiting. I strongly suspect that there is some subtle aroma of the grass that tickles the dog's palate, and seeing how much misery a dog suffers in the prospect and act of vomiting, I am loth to believe that he entertains any therapeutic purpose.

The origin of venesection was another matter that excited speculation in the ancient world. Pliny says that the hippopotamus invented blood-letting and taught the art to man. This animal, he says, when surfeited with blood, seeks out the sharp point of a newly-cut reed, and pressing against it opens one of the veins in the thigh, to its immediate relief; then it plasters the wound over with mud. Buffon, quoting P. Labat, repeats the story, substituting a sharp-pointed rock for the reed, and with the picturesque addition that it agitates its body to promote the flow, and when it thinks it has lost a sufficient quantity, rolls in the mud so as to close the wound. With respect to the various

observations involved in these statements, it is the case that the hippopotamus is very prone to exhibit wounds, in spite of the fact that in most parts the skin is two inches thick. These wounds seem to arise in two ways: most commonly as the result of fights, as male hippopotami in the wild state are constantly fighting, regardless of the pairing season, and a wounded animal is often furiously attacked by a comrade; but also as the result of rubbing themselves against any convenient projection, a habit that is common among animals in confinement, and even among such domesticated animals as cows, sheep, and pigs. Again it is a habit of both hippopotamus and rhinoceros to roll in the mud of river-banks till they are plastered all over, and the mud would certainly adhere more readily to the abraded surface of a wound, giving the semblance of a purposive dressing of mud. The Andaman Islanders use mud in similar fashion as a protective covering, as some think to guard them from the attacks of mosquitoes, but this is probably only part of the truth, as mud is used as a dressing in countries where there are no mosquitoes, probably for no better reason than that it is always handy. In the case of the Andaman Islanders, dwelling as they are almost on the Equator, it is more likely used as a protection against the direct rays of the sun. It is not without interest, in considering the association of the hippopotamus with venesection, that the hippopotamus is one of the animals that has a coloured sweat, carmine in its case.

The vampire bat has shared with the hippopotamus the credit of having taught mankind the art of venesection: such was the belief of the natives of Peru at the time of the discovery of that country. Peter Martyr describes the rescue from death of a man grievously stricken with pleurisy by the agency of one of these bats, after the human phlebotomist had failed to strike blood. Buffon cites several travellers who assert that large vampire bats suck the blood of men and cattle when asleep; they were believed to render the victim insensible to their bite by agitating the air with their wings and throwing him into a deep sleep. Doubt was thrown on these observations, till Darwin, in his "Voyage of the *Beagle*," described how he actually saw a vampire bat sucking blood from the withers of one of the camp horses. These bats attack man less frequently than cattle, and usually on an exposed foot when asleep. They have a capricious taste, for while one person may sleep in the open air with perfect immunity, another will be bitten almost nightly. This trait is common among blood-sucking animals, such as fleas and lice. Whether it is due to differences in the blood, or to the relative thinness of the skin, or to some other cause, is

obscure. Females, on the whole, seem to be more susceptible to both lice and fleas than males, and I can testify that not one of the least blessings conferred on myself by a long period of connubial felicity has been a complete immunity to fleas. The habitual food of these bats is insects, and blood is a prized but infrequent luxury. It is popularly believed in Peru that bites sometimes prove fatal, as the blood continues to flow after the bat has relaxed its hold; maybe that, like the leech, it secretes an albumose in its saliva. J. G. Wood says that the wound is sore and inflamed, but never fatal, but he appears to base this conclusion on an estimate of the amount a bat can imbibe, and takes no account of the subsequent loss.

Charleroix, in his "History of Paraguay," refers the discovery of blood-letting to the anta, an animal, "who, when he finds himself overstocked with blood, opens one of his veins with the point of a reed." The anta is the South American tapir, which Linnæus described as a sort of terrestrial hippopotamus. Lyddeker says that their hides are often thickly plastered with mud, probably as a protection against insects, and he adds that they are often found wounded by the claws of jaguars, which are their worst foe: these circumstances taken together show how this legend may have originated.

Yet another ascription of the art of bleeding is to the leech, of which Pliny says: "The action of leeches is looked upon as pretty much the same as that of the cupping-glasses used in medicine, their effect being to relieve the body of superfluous blood, and to open the pores of the skin."

He is a bold man that would venture to assign to any individual, man or animal, the honour, if such it be, of having introduced venesection into medicine; but we must not forget that it may well have originated out of all connexion with medicine as a part of primitive ritual, and that its place in medicine may be merely that of an ancient ritual survival. There is a well defined stage in the evolution of sacrifice, at which instead of sacrifice of the man himself, blood only is taken from him and his life is spared. He is brought to the altar and gashed by the priest. A modification of this is met with in the worship of the Phrygian Attis, where the priest draws blood from his own arm and presents it as an offering to the god; he is both priest and victim. In later stages an animal and then an animal's blood is substituted for a man and a man's blood, and later still a plant or the juice of a plant, for red wine is the blood of the plant. Human sacrifice and ritual cannibalism seem to have reached their zenith among the ancient

Mexicans. The ordinary mode of sacrifice was for the priest to slash open the breast and tear out the heart, then to fling the corpse to the worshippers to feed on in their homes. But at many of their religious fasts it was customary to draw blood from ears, eyelids, nose, lips, arms, legs and body, by thrusting the thorns of aloes into their veins, and by passing sharp sticks through the tongue. With such practices prevailing among the priesthood, and with medicine in their hands, one may discern a portal by which blood-letting may have found its way into medicine. The rationale of ritual blood-letting lies in the belief that the soul and the life of man reside in his blood. Even Harvey regarded the blood as the seat of the soul, and we find him in his "Prælectiones" refuting Aristotle, who had asserted the primacy of the heart, and laying it down dogmatically that the "soul is the blood." By eating the flesh or drinking the blood of the victim, the worshipper eats the body or drinks the blood of the god, for by the act of sacrifice the victim becomes one with the god; the desire for complete union with the god, by commingling with his substance, is a dominant feature of early worship. The gods themselves are constantly rejuvenated by the blood of sacrifices so that they may perform with unimpaired activity the vitally important functions incumbent on them. The Mexicans and Nicaraguans used to make effigies of their gods out of meal kneaded with human blood, and devoured them sacramentally. Blood was also used as a medium of divination, the Babylonian physicians drawing important conclusions from the behaviour of the blood drawn in blood-letting. It is interesting to find modern pathologists adopting hæmatoscopy afresh as a means of divining morbid conditions of the subject.

Whatever the origin of blood-letting we find the practice firmly established anterior to the earliest medical literature; though not mentioned by Homer it was well known to Hippocrates, and was an habitual remedy in veterinary medicine in the time of Virgil.

The Egyptians are said to have learnt the use of the clyster from the ibis. Pliny says: "The ibis, by means of its hooked beak, laves the body through that orifice by which it is essential to health that the residue of the food should be discharged." The sacred ibis, though so common in the land of the Pharaohs during its times of greatness, has now disappeared from Egypt; it had a very long curved bill. No doubt the legend was derived from the habit the bird has of collecting with its beak the oil from the preen gland, situated in most birds above the rump; the oil is used for giving a gloss to the feathers and in water

birds is very abundant. When preening birds dip their bills into water and shake them, probably so as to rid them of any residue of oil. These facts would seem to explain the statement of Cabanès, that his friend Dr. Ballion had seen wader-birds suck up water with the beak and carefully transfer it to their anus, and then shake their feathers and stand erect as though greatly relieved. Poultry, swallows, and other birds do at times remove fæces from their young with their beaks but only if the fæces are obtruded and retained *in situ*; this, however, is probably merely an instinct of cleanliness. It is possible that the story of the ibis may have arisen from the coincidence that the hieroglyph for the ibis is identical with that for a Moon-god, in which character Thoth, the divine physician, was often represented. It deserves also to be mentioned that the use of the clyster for evacuating the bowels was a late development; its earlier use was as a means of introducing drugs and aliment into the body.

The hibernating bear inevitably excited the curiosity of the speculative, but far greater interest was displayed in the associated phenomena of hibernation than in the causes that induce the state. Topsel believed that it was the direct result of eating certain herbs that imparted the faculty of sleeping the whole winter without food and without sense of cold; in support of his belief he states that a cow-herd who observed a bear eating a certain root, ate some of it himself and was immediately seized with such a desire to sleep that he lay down and slept all through the winter waking again only in the spring. Modern research suggests that the state is excited rather by the recurrent failure of the general food supply than by the ingestion of any special food-stuff, and is limited to those species in which the power to maintain temperature at a permanent average height during sleep has been lost or never acquired. Aristotle says that after hibernation bears eat arum so as to open the bowels which are empty and collapsed, and that they chew sticks of wood as though they were cutting teeth, or as Pliny suggests, so as to sharpen their teeth; the latter adds the observatiou that their eyesight is dimmed, and that to relieve this they invade the nests of bees so that the stings they receive in their throats may draw away the oppression from their heads. It is not easy to obtain reliable evidence of the behaviour of hibernating bears, as they do not hibernate in captivity. We do know, however, that in the wild state the bowel is mechanically obstructed during hibernation by a plug technically known as "the tappen," which is composed almost entirely of pine leaves and the various substances

which the bear scratches out of ants' nests; this tappen is discharged on the return of activity in spring. This, no doubt, is the basis of the tradition that the bear seeks special herbs to excite purgation. Ælian holds that the purgation restores the bear's power of taking food, but Topsel conceives it to have the further purpose of improving their general health, so as to render them highly savage during all the time that they are guarding their young. The eating of ants and of honey was believed to be also designed for the purpose of opening the bowels. Pliny, however, fancies that ants are eaten to counteract the effects of the mandrake, when it has been taken inadvertently. This whole sum total of credulity has provided practical medicine with no hypnotic at all, and no laxative more active than honey, even if its use could be referred to this source. In raiding a nest of bees for honey the bear is quite regardless of the numerous stings it receives, which seems to have induced Pliny's belief that the stings were solicited for a definite purpose. Perhaps some colour was lent to the belief by the knowledge that bears, especially in confinement, are very prone to ophthalmia and are often totally blinded by it. The grizzly bear's habit, when coming out of hibernation, of standing upright against a tree and scoring the bark with its claws calls for passing notice as affording a clue to the legend of the chewing of sticks. This primitive method of manicure is doubtless directed to reducing the redundant growth of the nails to serviceable proportions.

When Æneas was wounded by an arrow, Venus sped to Crete to fetch the herb dittany, wherewith Iapyx should heal his wound. Deer and goats well know its value as a vulnerary to remove arrows from their wounds. It is difficult to conceive a stage of mental development at which man employed herbs in preference to his hands for the extraction of arrows, as even animals possess this power in a considerable degree. Elephants have been seen to extract weapons with their trunks, monkeys extract thorns with considerable skill, and dogs remove splinters and thorns, on occasion, with their teeth though they often present the damaged part for the attention of their master, when they cannot effect its remedy with their teeth.

Such a wealth of legend has gathered around the disposal of the secundines, and, indeed, around all the phenomena of parturition, that a cursory review of the whole subject will best serve our present purpose. Pliny says that hinds eat the herb seseli and the leaves of the hart-wort before parturition, to make labour easy; and that immediately after delivery they eat the two herbs seseli and aros together, so that their

milk may be impregnated with their juice before they suckle their fawns. Aristotle adds, that the hind swallows the afterbirth immediately after parturition, and that it is impossible to prevent her as she catches it before it reaches the ground. He surmises that the placenta has medicinal properties. Apparently it was in virtue of its purgative action that seseli was believed to aid parturition, but on the other hand, Pliny says that the action of hart-wort is to arrest looseness of the bowels. It is impossible to reconcile these contradictory therapeutics; suffice it to say that the oral transmission of medical lore is not conducive to its accuracy.

With regard to the mare, Pliny writes that "the colt is born with a poisonous substance on its forehead, known as hippomanes, and used in love philtres; it is of the size of a fig, and of a black colour. The mare devours it immediately on the birth of a foal, and until she has done so she will not suckle it." According to Cuvier a concretion is sometimes found in the liquor amnii of the mare, which she instinctively devours, as other quadrupeds do the afterbirth, but I have failed to obtain corroboration from horse-breeders, and incline to Aristotle's opinion that all the stories about hippomanes are old wives' tales. Probably the whole fiction arose out of the mare eating the placenta. Virgil seems somewhat uncertain as to the exact nature of hippomanes, for in one passage he accepts the description given by Pliny, while in another he describes it as the vulval discharge of a mare in season. The belief in the galactagogue properties of the placenta clearly prevailed in Pliny's time, as it does even to-day, in spite of a good deal of experimental evidence in a contrary sense. Lederer and Pribram claimed to have produced, within a few minutes of the injection of placental extract, an enormous increase in the amount of milk, secreted during a period of two or three minutes, soon after the injection. This may, however, have been produced merely by a temporary and more rapid evacuation of the mammary reservoir by rise of pressure from increased muscular tonus. Dixon and Taylor claimed to have found such pressor substances, but Rosenheim showed that they were products of putrefaction in the placental extracts employed in their experiments.

Lane-Claypon and Starling obtained negative results, as regards increased activity of the mammary glands, from injection of placental extract. Their experiments, however, seemed to show that the growth of the mammary glands during pregnancy is due to the action of a specific chemical stimulus produced in the fertilized ovum, which is increased in amount *pari passu* with the growth of the fœtus. They

suggest that lactation is due to the removal of this substance, which must be regarded as exerting an inhibitory influence on the gland cells, hindering their secretory activity, and furthering their growth; in that case it is probably also a chemical product of ovarian activity that stimulates growth of the breasts. Biedl and Koenigstein in the main confirmed the findings of Lane-Claypon and Starling. Frank and Unger's experiments led them to think that the changes in the breasts, both at puberty and in pregnancy, depend entirely on ovarian activity, and have nothing to do either with fœtus or placenta. They consider that the changes in the breasts, found by Lane-Claypon and Starling after injection of fœtal extracts, do not exceed the physiological degree of variation in size occurring normally in the breasts of virgin rabbits. Basch suggested that some substance elaborated in the placenta activates the ovary, which, in turn, stimulates growth of breasts, with ultimate lactation, but other experimenters have not confirmed him. The general tenor, then, of recent experimental evidence is decidedly opposed to the ancient belief that animals devour the placenta so as to excite or to increase secretion of milk. If, then, primitive man acquired the habit from observation of animals, he was following a blind guide.

Much interesting evidence is available as to the primitive method of disposing of the placenta. In 1556 the missionary, Jean de Léry, reported that the Brazilian natives devoured the placenta, and this was subsequently confirmed by Engelman and Rodet. Raynal says that in America the Topinamboos and the Tampuya devour the after-birth, the same practice persists in certain parts of the Soudan. About the year 1700 A.D. the traveller, Gemelli Carreri, observed that among the Yakouts, a people of Russian Asia, the father took the placenta immediately after delivery, had it cooked and regaled his parents and friends. But there is no need to go so far afield for evidence of this mode of disposal of the placenta, for Reverdin records an instance in his own practice. A woman, immediately she had been delivered, begged to be permitted to see the placenta, which was lying steaming on a cloth. Reverdin handed it to her unsuspectingly, and she expressed surprise at its appearance, and examined it closely. Suddenly she seized it and with a wild cry devoured it. Next day he asked her why she had done it, and she told him that a wild desire to do so had overcome her. "Do you still crave for it," asked Reverdin. "Phew! what a disgusting idea!" she replied, "I cannot conceive how I came to do it." Other instances of the kind are to be found in contemporary medical literature, and no doubt the occurrence would be much more frequent were not the

placenta so sedulously secreted and so expeditiously disposed of by the midwife.

It is difficult to appreciate how the custom arose of the husband devouring the placenta instead of the parturient wife, but the same might be said of the couvade ; here again tradition seems to afford a clue. In Chinese medicine placenta is used as an aphrodisiac. Suetonius says that Cesonia excited the sexual passion of her husband, Caligula, by administering it to him in a potion, and in recent times women used to keep the umbilical cords of their female children, so as to excite their lovers. Placenta figured in the pharmacopœias of the seventeenth century for use as a galactagogue, an aphrodisiac, a laxative, and a remedy against sterility, chlorosis, and uterine disease. In all these uses one detects the trail of sympathetic magic, and no trace of rational pharmacy. For a similar reason powdered egg-shells found a place in the old pharmacopœias, regardless of the fact that they are of extra-embryonic origin. They do, however, appear to exercise some beneficial physiological influence, for there is some evidence that they help fowls to put on weight ; maybe that they supply calcium salts in a readily assimilable form. A French writer has suggested that they act on the fowl by stimulating its generative system, and he has no doubt of the aphrodisiac action of the placenta when devoured by the female rabbit and guinea-pig. If we admit the existence of such aphrodisiac properties, even in imagination, it suggests a reason for the male feasting on his wife's placenta. In India, where fertility is more prized than in this country, aphrodisiacs occupy a prominent place among medicinal remedies ; it is only in the last few years that they have fallen out of the English text-books.

With wild mammals it is the rule for the mother to eat the placenta. Dr. Chalmers Mitchell assures me that this is the case with deer, wild asses, zebras, wild cattle, marmosets and rabbits. Apes do not breed in captivity, so that nothing is known of their placental habit. There is a striking contrast with many domestic animals in the treatment of the placenta. Mares and ewes hardly ever eat the placenta, but instances of each are known. Cows vary greatly, thus a breeder of South Devons tells me that left to themselves 50 per cent. eat it, while an owner of cross-bred stock speaks of it as exceptional. The evidence of rustic cowmen is tainted by their ingrained conviction that every cow would eat it, given the opportunity. Sows, cats, and bitches in the vast majority of cases eat the placenta. In the case of the sow the last two or three pigs are often discharged along with the placenta, and she is

very apt to swallow them along with it, no doubt mistaking them for placenta. Sows sometimes eat their whole litter; it is said that they never do so after they have once suckled their young pigs. This throws a light on the curious mental process by which a sow becomes aware of the identity of her young. Cats, marmosets, and rabbits, all of which eat the placenta, are also liable to eat their young. Birds, at times, eat the shells of the hatched egg, though the more usual procedure is to eject them from the nest, and often to carry them to a distance from the nest.

It is interesting to note how much more frequently the placenta is eaten by wild than by domestic animals, and I would suggest that the purpose is to conceal all trace of the existence of their young from predatory foes. Secretiveness with animals, and with wild animals in particular, dominates every detail of parturition. They hide themselves away, and if watched curl themselves up so that it is exceedingly difficult to follow their procedure; as soon as the young are born they endeavour to keep them out of sight of an intruder by interposing their own body. Can this, too, be the explanation of the habit among the carnivora of eating the dung and lapping the urine of their young, a habit that persists in the domestic cat and dog? Animal motives, like human motives, no doubt are often mixed, and what serves the end of cleanliness may equally serve other ends as well. I had expected to find some trace of this habit among puerperally insane women, but have not succeeded, and one would hardly look for it among the sane at present standards of cleanliness and civilization.

This is but one instance of a striking uniformity of habit among primitive men and animals in the procedure of parturition; in the matter of posture we shall find the same. The recumbent posture in parturition is by no means universal with the human female. Only a few years since, and perhaps still, it was a common practice in some country districts for a woman to be delivered standing, and supporting herself with her arms on the end of the bed; some midwives indeed insisted on the erect position. I retain a most vivid recollection of my first attendance on a woman in labour, and her restless promenade of the room, for all the world like a hungry lioness pacing to and fro inside the bars of her cage: rupture of the membranes occurred, and the head of the child was actually protruding before I could persuade her to lie on the bed. In native races it is common for women to be delivered on the march with supreme ease and with little or no pain. Peter Martyr has a brief picture of parturition, which illustrates this matter:

"When the women know it is time to be delivered of the child they go into the neighbouring wood, and there taking hold of the boughs of any tree with both their hands, they are disburdened without the help of any midwife, and the mother herself, speedily running, taketh the child in her arms, and carrieth it unto the next river. There she washeth herself and rubbeth and dippeth the child often, and returneth home again, without any complaint or noise, and giveth it suck; and afterwards, as the manner is, she washeth herself and the child often every day." This helps us to understand why Leto clasped a bay-tree when about to give birth to Apollo and Artemis, and the prevalence of the same practice in Sweden in former days. Frazer suggests that the parturient woman was seeking moral rather than physical support in an appeal for an easy delivery and avoidance of the perils of childbirth addressed to the tree-spirit embodied in the tree, who was credited with power over the fertility of women. Our museums also and illuminated manuscripts testify that the recumbent posture in parturition has only been arrived at by a gradation of stages, the sitting posture having been long in vogue.

Passing to animals we find a general statement of Aristotle that "quadrupeds, as a rule, lie down for parturition, and in consequence the young of them all come out of the womb sideways. The mare, however, when the time for parturition arrives stands erect, and in that posture casts its foal." As a fact, mares usually foal lying down in this country, but it must not on that account be assumed that Aristotle's statement is necessarily wrong. For example, among cows posture seems to vary to some extent with the breed; as a general rule they calve lying down, but whereas this is the case with South Devons, Devons often calve standing. In difficult labour a cow often gets up from lying down when the calf is half-born, probably so as to facilitate labour. Ewes, as a rule, lamb lying down on one side; exceptionally they stand, and the same is the case with sows, bitches, and cats. Speaking generally, then, the domestic animals, whose habits would have been most likely to influence man, adopt a recumbent posture, like the human female, but there are many exceptions to the rule.

In animals such as mares, cows, ewes, and sows the cord is practically always severed as the animal rises, or, if she is delivered standing, by the descent of the young; with mares the cord not infrequently breaks close to the navel, and there is danger of losing the foal from hæmorrhage. With carnivora the cord is frequently intact, and the animal divides it with her teeth before she rises. Wild animals are so

secretive over parturition that even if one be present it is difficult to see what they are doing; rats certainly, like bitches and cats, divide the cord with their teeth, and Mr. Pocock, of our Zoological Gardens, informs me that he is under the impression that this is true of most carnivora. A primiparous bitch is less expert than one that is multiparous in severing the cord, and is apt to sever it too close to the pup's body. Foals are not infrequently born in their membranes and die of suffocation, but in the case of a calf so born the mother ruptures the membranes by licking them with her rough tongue. This is only one illustration of the far stronger maternal instinct of the cow than of the mare, and does not bespeak a purposive intelligence.

In the treatment of the umbilical cord I have succeeded in finding in man a vestige of approximation to the animal habit. One of the members of this Section, who for many years conducted a large general practice in South Wales, tells me that he was cognizant of an instance in which a young idiot woman divided the cord of her infant with her teeth. I have encountered no other instance, either in literature or by personal inquiries, and I should value greatly other information on this head, and, indeed, on any matters touched on in this paper. Lettsom says that native South American women commonly divided the cord with a firebrand, which cauterized the vessels, so that they needed no ligature. To the ancients it seemed reasonable to explain similarity of practice by the assumption that man had taken a lesson from animals, but for us it is more reasonable to suppose that we have to do with the maintenance of a habit acquired before the partition of the common ancestral tree.

The toilet of the mouth has its legendary origin in the story of the trochilus bird, which serves also to illustrate the danger of incorrect inference from correct observation. Herodotus, Aristotle, Ælian, and Pliny all tell it in much the same terms. Ælian says that leeches invade the mouth of the crocodile, as he swims with it open, and cause him much discomfort. Feeling the need of the medical aid of the trochilus he swims to the bank and lies there with his jaws agape, whereupon the bird enters and removes the leeches, while the crocodile remains perfectly still so as not to harm it. Pliny suggests that the animal opens its jaws to admit the trochilus, because it enjoys the titillation of its movements. Buffon cites a traveller who actually saw the bird enter the crocodile's mouth, which closed behind it each time, and opened again after a while for the bird to emerge. Topsel says that the beast reopens its mouth because the bird carries sharp thorns

on its head, which prick its palate, and the modern Egyptian, according to Leith Adams, says that the reminder is conveyed to the crocodile by horny spurs of the bird, which Brehm identified as the Egyptian plover. One can scarcely doubt that the performance is due to the bird's quest for palatable food, and not to the intelligent demand of the crocodile for the effective scavenging of his teeth.

A rich crop of legend grew up around the properties and disorders of vision. Men were puzzled by the closed eyelids of the young, and suggested various explanations of the mode by which they acquired sight. Pliny says that puppies are born blind, and that the length of time before they acquire sight depends on the amount of mother's milk, so that the more numerous the puppies the later do they acquire it. In another passage he states that the swallow employs celandine as an eye-salve to give sight to her young, and that it can restore vision, even when the eyeballs have been plucked out. The plant blossoms, he informs us, at the time of the swallow's arrival and withers at its departure; probably it is from these facts, approximately correct as they are, that it acquired its botanical name "chelidonium," from the Greek χελιδών—a swallow. Pliny recommends its juice blended with Attic honey as a sovereign remedy for films of the eyes, and its virtues have been perpetuated in the use of the term "chelidonia" for eye-salves. The almost dazzling brilliancy with which the flower in early spring peeps out from its sombre green cushion of leaves may well have suggested its sympathetic usefulness for dim eyes. Pliny asserts categorically that it was from the swallow that man learnt the use of celandine for affections of the eye, while Ælian regrets that all their labour to prepare such an eye-salve as swallows use has been in vain.

Hawkweed also enjoyed high repute in diseases of the eye. Pliny says that the hawk distils its juice into her eyes to prevent any dimness of vision, and, mixed with human milk, he recommends it as a cure for all diseases of the eye. It was necessary to postulate the employment of some such specific remedy to explain the marvellously keen sight of the hawk's eye. Ælian has it that "physicians use this same remedy for eye diseases, nor do they deny that they have learnt it from birds, but prefer to profit by the knowledge."

Fennel, too, stood high in favour in the treatment of diseases of the eye. Both Ælian and Pliny were aware of the fact that, when a snake changes its skin, the disks over the eyes peel off with the rest, and appear as dry lenses in the slough. Pliny seems to say that the snake takes fennel internally to aid exfoliation, and also applies it locally to

the eyes to sharpen its sight, and it was this knowledge that led to its use by man as an eye-salve. In later times euphrasia was used similarly in eye-diseases, but according to the doctrine of signatures, as it has a black spot on its corolla, resembling the pupil. When Michael, in "Paradise Lost," opens the eyes of Adam to see the future of the world and of his own progeny therein,

> He purged with euphrasie and rue
> The visual nerve, for he had much to see,
> And from the well of life three drops distilled.

Ambroise Paré asserts that the operation for cataract was discovered by reason of a goat with a film overlying the pupil tearing off the film accidentally when scratching itself against a thorn, and in consequence recovering its sight, but Ælian and Pliny reject the accidental character of the goat's discovery. Pliny says that the female goat uses the point of a bulrush, and the male the thorn of a bramble for its manipulations, but neither he nor Ælian appreciates the nature of cataract, as Pliny conceives that the cure is due to escape of blood that has surcharged the eyes, and Ælian that it is due to the escape of some morbid humour.

The phenomenon of nyctalopia in man was known to the ancients, and goat's liver was eaten for its cure, as it was believed that goats could see by night as well as by day; but in the discussions of the non-medical writers of antiquity there already exists a confusion as to the meaning of the term, some taking it to mean "able to see only at night," and others "not able to see at night." It is an interesting example of the widely prevalent organotherapy of early medicine.

Melampus is said to have learnt the purgative action of hellebore from observing its action on goats. He used the drug with success in the cure of the daughters of Proetus, King of Argos, who fancied themselves metamorphosed into cows, to gratify the anger of Hera—accounts differ as to whether Melampus sent them the milk of goats that had eaten hellebore, as nowadays one might send rodagen, or gave hellebore itself, joined with certain superstitious remedies. Having regard to the action of the drug, the wonder is that the ladies were not still further confirmed in their hallucination. In this fabulous setting we catch a glimpse of the old-time belief in animal transformation, which found its most extravagant expression in the state of lycanthropy. Ovid describes the transformation of Lycaon into a wolf. Ætius, Avicenna, Oribasius and others speak of it as frequent. Burton descants

on the wide prevalence of this mental disorder in olden times. The idea of the were-animal struck its deepest roots in Scandinavian and North German soil: they fancied that by donning the skin of a wolf they could become wolves at pleasure. It is less common in Greek and Latin folk-story, but a familiar example is to be found in the Eighth Eclogue, where the lover, by using Pontic herbs, is enabled to see Moeris transformed into a wolf. Lycanthropes, like wolves, often lay hid during the day and at night wandered abroad, haunting the graveyards and barking and howling. Knowledge of the action of hellebore was not the only gift bestowed on Melampus by animals. His gift of divination he owed to some serpents that he rescued from death and reared; they in return cleansed his ears with their tongues while he slept, so that when he awoke he understood the language of birds, and so learnt the mysteries of the future that it is their privilege to foreknow. Melampus had at least one valuable remedy in his armamentarium, for he cured one of the Argonauts of sterility with steel drops, administering the rust of iron in wine for ten days.

In ancient medicine prognosis was esteemed as highly as healing, and man desired to share with animals, and especially with birds, their faculty of foreknowledge. Nature seemed to have endowed them with power to presage the weather by observing the skies—a gift of first importance to a primitive agricultural community. Virgil mentions, as foretelling the coming of rain, the flight of cranes and swallows, the heifer scanning the face of the heavens, the clamour of rooks and of ravens, and the plaintive chatter of frogs in the fens. In Germany even now green-frogs in tall glass bottles are used as barometers: in fine weather they ascend, in wet weather descend little wooden ladders, each step of which marks a degree. Buckland succeeded in using leeches similarly for the same purpose. Many an old maid to-day in London will tell you that her cat keeps close curled up before the fire when snow is coming.

Besides foreknowledge of the weather, man was believed to have acquired much other useful knowledge directly from animals. The ancient Egyptians believed that the art of writing had been learnt from the sacred baboons that were kept in their temples; when a fresh baboon was admitted, his fitness was tested by the priest putting writing materials into his hand and calling upon him to write. Topsel suggests that the practice of circumcision may have been learnt from them, as the young are brought forth "circumcised, at the least wise in some appearance; whereunto the priests give great heed to

accomplish and finish the work begun." There is no obvious anatomical peculiarity in the prepuce of the young monkey, unless it be its great laxity permitting ready protrusion of the glans penis, to explain this idea, and such evidence as we have seems to point to the origin of circumcision not as a medical or sanitary measure, but rather as a feature of primitive sacrificial ritual. These sacred baboons were held to have originated the division of day and of night into twelve equal parts, so as to afford a model for construction of the water-clock. There was a widespread tradition that music had been learnt from birds, and chief among them the nightingale: in Hindoo poetry it is the kokilas, the Indian cuckoo, that teaches melody. Birds are often credited with bringing down fire from heaven: in Polynesia it was a red-pigeon, in French folk-lore the wren. The tale runs that all the other birds, except the owl, contributed a single feather apiece to replace the scorched plumage of the wren, so as to keep it warm in the coming winter. For its ill nature the owl was condemned to eternal seclusion during the warm day, and to perpetual suffering from cold during the night, and the other birds maintain the punishment by pestering it if it appears in sunshine. Every nation has its Prometheus: in Greek legend it was a man, among the North American Indians a stag, but the myth never tells how the fire was produced, in conformity with the circumstance that the maintenance of fire accidentally produced seems always to have antedated the discovery of how to produce it.

It would be interesting to hear the chatter of the native East African over the domestic meal after watching an aeroplane soaring in the sky: he would probably recognize in its movements, its outline, and its mode of propulsion another gift of knowledge from birds to man, confusing what is in part due to imitation with what appears to be due to instruction.

Ælian asserts that birds know all the remedies that man employs: he cites the application of marjoram to their wounds by partridges, storks, and pigeons. Pliny, too, gives a list of purgative herbs used by birds, but, if we can be sure of their identity, few, if any, possess the properties claimed for them. The sedative properties of lactucarium or lettuce were widely accepted. Aristotle and Ælian state that it is used by the dragon to relieve a distended stomach. Venus was said to have lulled her grief at the death of Adonis and repressed her desires by throwing herself on a bed of lettuces. Experiment has failed to show any sedative action in the juice of lactuca, and belief in its hypnotic properties is probably due to the similarity in appearance and smell to the juice of the poppy.

Falloppius asserts that the ape taught man the laxative properties of cassia, and in view of the fondness of monkeys for fruit it is not impossible that man may have observed its laxative effect.

Cytisus was believed to possess important galactagogue properties, and was given to cattle for this purpose. Aristomachus, an Athenian medical author, recommended that an infusion of the plant, mixed with wine, should be given to nursing women when their supply of milk was deficient. The active principles of cytisus are sparteine and scoparin, neither of which is known to possess galactagogue powers. Scoparin produces some amount of diuresis, probably by action on the renal epithelium. Possibly the flow of milk might be increased, if the juice were given in great dilution, or if its bitterness induced free drinking of water.

Dogs were said to have taught man the use of pellitory for expelling calculi. One wonders whether the belief may have arisen in an attempt to explain the name.

If man learnt the use of drugs from animals, he must have paid a price in some cases for his knowledge. Thus horses eat aconite with impunity; birds, rabbits, and many herbivora, belladonna, and its effect on horses and donkeys is but slight. Goats, sheep, and horses eat hemlock without ill effect, yet it poisoned Socrates. Henbane has little effect on sheep, cows, pigs, and pigeons, and ipecacuanha does not excite vomiting in rabbits.

Animals seem to acquire their knowledge of poisonous plants from experience, and not by any innate instinct. Our domestic animals, when transported to other countries at first eat poisonous plants, which they learn afterwards to avoid. Snell observed that strange sheep frequently fell victims to the poisonous hellebore that grows abundantly in the valley of the Ahn, but that it is carefully avoided by the sheep of the neighbourhood. Pliny was evidently aware that cattle did avoid certain poisonous plants, and he instances the anagallis as one, but he goes further and asserts that, if they eat it by mistake, they have a remedy at hand in a plant that is an antidote. Lambs and calves, grazing in the same field as their mothers, are far more prone than they to eat poisonous plants. Morgan concluded from observations of feeding young birds with various caterpillars, beetles and worms that, in the absence of parental guidance, young birds have to learn by experience what is good to eat and what is not, and that they have no instinctive aversions. At first they pick at everything, but once they have found that a particular thing is distasteful or harmful, in future

they entirely avoid it. Tegetmeier found that pigeons reared exclusively on wheat or barley would starve before eating beans; but if a bean-eating pigeon were introduced among them, they would soon imitate and adopt the habit, instinct yielding to experience; in the same way fowls sometimes refuse maize, until they are placed with maize-eaters, when they not only eat it, but become exceedingly fond of it. Newly-caught birds are apt to starve unless the cage-food is mixed with food to which they are accustomed. Each bird seems to make its own observations, its own experiments, and its own discoveries in the matter of food, and the rapidity with which knowledge acquired may be propagated is shown by the readiness with which the habits of other members of the species are imitated.

The taste for meat is not infrequently acquired by herbivorous animals, and once acquired may amount to a veritable passion. Parrots, that have lived on insects and berries, have been known to acquire such a liking for meat that they have actually pecked to death animals as large as sheep and porpoises, so as to eat their flesh in place of their habitual food. Horses will take to flesh-eating, and have been known to snap up young pigeons and chickens. Meat diet is said to render them savage. Reclam saw squirrels and rabbits gnaw greedily bones thrown to them, though they had abundance of vegetable food. Both Darwin and Brehm record instances of cattle taking to a diet of fish on certain islands when pasture failed them.

Man, doubtless, will have acquired much of his knowledge of the nutritive and medicinal value of plants by the same method as the lower animals, by experience. Like them, too, he will have profited by imitation, and imitation embracing his observation of the habits of the lower animals. It must have been of immense importance to man, when he depended largely for food on wild animals captured in the chase, to watch them closely so as to know their habits. Baited trap-holes, being one of the chief modes of capturing game, a knowledge of the favourite foods of animals must have been a first condition of success. This, together with his passion for their domestication, to be at once his companions and his servants, must have given him an intimate knowledge of their foods and their effects. Further, there is not wanting evidence that he did use animals, and even his fellow-men, as subjects of research in this field, for the Philippine pygmies, if they fancied a fruit were poisonous, would soak it for two or three days and then give it to one of their dogs; if he seemed none the worse they would eat it themselves; the American Indians first tested arrow poisons on the old women of the tribe.

That a good deal of man's medicinal knowledge arose accidentally in his efforts to extend the range of his food supply is suggested by the prominent place occupied by food-stuffs in primitive pharmacy. Honey, milk, butter, and cheese appear and reappear in every conceivable combination. We shall be less likely to under-rate the importance of honey in the dietary of primitive man, if we bear in mind that it was his only readily available supply of sugar. Honey and the honey-bee were surrounded with a sacred halo of mystery in the mind of the primitive zoologist. He believed the bees to be generated from putrid flesh, just as fish and frogs seemed to be generated from the mud, when the rain filled the pools with water. This was not an unnatural belief, for he saw the maggot-like larvæ in the honey-comb, and they seemed to him for all the world the same as the maggots engendered in fly-blown putrid meat. Pure honey, engendered in the air, might be found in its elemental purity at dawn deposited on the leaves of trees and elsewhere; how else can we account for the deposit on the leaves of the lime! Pliny speculates as to whether this liquid is the sweat of the heavens, or saliva emanating from the stars, or an exudation from the air, purified in the process of distillation. Ah! if only it came to us limpid and untainted, as when it set forth on its downward path, before it is corrupted by exhalations from the earth, or by the admixture of the juices of flowers, and by elaboration in the stomach of the bee! Pliny describes various kinds of honey with the gusto of an up-to-date apiarist, but he awards the palm to summer honey, for "Nature has revealed in this substance properties most valuable to mortals . . . . for, after the rising of each constellation, and more particularly those of the highest rank, or after the appearance of a rainbow, if a shower does not ensue, so that the honey-dew becomes warmed by the rays of the sun, there is produced a medicament, and not real honey, a gift sent from heaven for the cure of diseases of the eye, ulcers, and maladies of the internal organs. If this be taken at the rising of Sirius, and this should happen to fall on the same day as the rising of Venus, Jupiter, or Mercury, as is often the case, the sweetness of this substance and the power it possesses of restoring men to life are not inferior to those attributed to the nectar of the gods." Long before the time of Pliny men were aware of the occasional poisonous properties of honey, and rightly attributed them to the flowers from which the honey was gathered. Xenophon records the toxic effect of the honey at Trebizond on some of the Ten Thousand, which has usually been referred to the *Azalea pontica* and *Rhododendron ponticum*, that abound in that neighbourhood;

poisonous honey is met with also in Pennsylvania, gathered from *Kalmia latifolia*. All the stories of Mithridates having been the discoverer of antidotes seem to be referable to the knowledge that many poisonous plants grew in his kingdom on the shores of the Euxine; as ducks were seen to feed on the herbage with impunity, it was not unnatural that their blood should be regarded as an antidote to poisons.

Mixtures of honey with milk or butter are favourite dishes of the Arabs and Hindoos, and recall the Biblical injunction, "Butter and honey shall he eat." Honey is much used in India in ointments in place of animal fats, which putrefy readily in hot climates. In this country it has been used as a vehicle for drugs, as in mel boracis, or as a palatable addition with mildly laxative effect in confectio piperis. For centuries it has been used as a local application to the aphthæ of children, and Soranus attributes this not to its inherent medicinal qualities, but to the fact that it was taken from hives near the tomb of Hippocrates. Milk was much used as a vehicle for drugs, and butter as an ointment or the base of an ointment, while Zoroaster seems to have anticipated Metchnikoff by living thirty years in the wilderness upon cheese, prepared in such a way as to render him insensible to the advances of old age.

Some substances probably found their way into medicine for no better reason than that they were generally ready to hand; any one who has lived among the sons of the soil or among fishermen on remote coasts must be familiar with the tendency to use both internally and externally whatever is first to hand. I have seen a farmer pick up a handful of dust and mix it with dung from the hen roost, and then apply the mixture to a sore on a sheep's back; cobwebs are used to staunch the bleeding of a cut, and so is tobacco; leaves are used in the West Indies as plasters and as dressings, and I am not at all sure that dung pharmacy, for all its later elaboration, may not owe its origin in part to this tendency.

Magic was another prolific parent of medicinal remedies; the Echeneis is a good illustration of one introduced in obedience to the dictates of sympathetic magic. This genus of sucking-fishes has on its head an adhesive disk by which it attaches itself to sharks, turtles, and other marine animals, and in default of these to ships. Its object seems to be to insure an ample supply of food by increasing its range of locomotion. Its retarding effect on the bodies to which it attaches itself seemed to suggest the desirability of its use for the same purposes in medicine. We learn from Pliny that it was used in love philtres,

apparently to prevent the affections from wandering, and in obstetric medicine to check bleeding from the womb, to prevent *procidentia uteri* and to delay premature birth.

In the matter of poisons and their antidotes, it was believed that man had learnt many valuable lessons from animals. The poisonous bite of the serpent and the source of its poison were bound to excite speculation. Ælian lays it down that animals acquire poison by feeding on poisonous things; thus the sting of wasp, he says, is far more dangerous if it has tasted a viper, and a fly, if it has touched anything of the kind, has a more harmful bite and causes greater pain. Homer depicts the dragon lying before his lair eating baleful herbs, and Virgil paints the same picture of the snake. Numerous antidotes to the snake's poison were in use. Pliny says that the plant chondrion, the identity of which is obscure, was employed with good effect, as field mice were known to eat the plant when bitten by snakes. The same writer asserts that the tortoise eats the plant known as cunile bubula so as to recruit its powers of effectually resisting serpents, and he recommends the juice of the plant in wine to be taken internally, and the bruised leaves to be applied locally for their bite. Aristotle says that the tortoise, when it has eaten a snake, feeds on marjoram, and he declares that one was seen to do so repeatedly during its repast; seeing this the observer rooted up the marjoram with fatal results to the tortoise. Some of the larger tortoises and turtles do prey on small reptiles, and this must have seemed to call for explanation to those who were familiar for the most part with the herbivorous diet of the smaller land species.

The tales of the weasel and the ichneumon in this connexion seem to be all referable to the mongoose. Lyddeker identifies the ichneumon with the Egyptian mongoose, and the similar appearance of the weasel not unnaturally led to confusion. Both Pliny and Ælian say that the weasel protects itself against the serpent by eating rue, but recent experiments tend to show that the weasel will not attack serpents. Wilkinson, in his "Ancient Egyptians," says that an Arab assured him that the ichneumon is very dexterous in killing serpents, and that, whenever bitten, it has recourse to a plant, of which it eats part and applies part to its wound, and then returns revived to the encounter. According to Lindsay, the Indian mongoose, when poisoned by a snake-bite, uses *Mimosa octandra* as an antidote; the mongoose plant, *Ophiorhiza mungos*, is used for the same purpose in India. Most countries have their particular antidotal snake-root. In North America and

Canada the roots of *Liatris spicata*, *Eryngium aquaticum*, *Eupatorium altissimum*, and *Asarum canadense* are all in favour; and elsewhere *Aristolochia serpentaria*, *Polygala senega*, and *Cimicifuga racemosa* have been all used for the same purpose. Most of these contain active principles which, in warm decoctions, induce diaphoresis or diuresis, the idea being that the poison will be eliminated by the skin or kidneys. Spider's venom was regarded with only less fear than that of the serpent. The tale was told that the toad, when bitten by a spider, ate the leaves of the greater plantain, and succumbed to the bite when these were not available. Deer were said to protect themselves from the poison by eating crabs.

The victims of that epidemic form of convulsive hysteria known as tarantism, which flourished over wide areas, more particularly in Italy, believed themselves to have been bitten by the tarantula spider. So ingrained was this belief that Constantine the African, the most learned physician of Salerno, stated that those who were bitten were liable to eject from their stomach and bowels substances resembling a spider's web. It was, nevertheless, not an animal venom, but a mental poison that was propagated from person to person in a human soil sensitized by long years of pestilence and warfare. The efforts of the victims to cure themselves by wild dancing, excited and maintained by the music of the flute, may be regarded as rational therapeutics according to the notion of the day, for clearly the chemical poison should find an exit in the sweating provoked by the violent exertion of the dance. Just so, when disease is conceived as due to a demoniac manikin within the sufferer, savages resort to violent bodily movements to effect its expulsion.

These various legends of the effect of spider's venom on animals and man rest on the most slender basis of fact. It is true that all spiders have poison-glands, which communicate with fangs in the mandibles, but the smaller species are quite harmless to man and the larger animals, and it is open to question whether their poison plays an important part in the killing of insects. With regard to the tarantula there is little evidence to show that its bite can cause more than local irritation in man and other large animals; this, of course, in rare and unfavourable circumstances, may be the starting-point of a secondary generalized poisoning. Some of the larger spiders can undoubtedly kill small birds and animals, but Warburton holds that this is due to the unerring accuracy with which they drive their jaws into the vital nervous parts of their victims. Fabre, on the contrary, insists that death does not

depend on the bite involving a vital part. There were some apparently, even in the seventeenth century, who did not believe in the poisonous effect of the tarantula's bite, for Ferdinando, a physician of Messapia, considered the symptoms to be those of a melancholy dependent on the imagination.

We may now pass from the domain of legend to the more prosaic consideration of such substantial evidence as there is of the existence of healing practices among animals, and we shall find it disappointingly slight. Livingstone left it on record that some of the anthropoid apes staunch bleeding by pressure with their hand on the wound or by stuffing it with leaves, turf or grass. Dr. Thomas Savage has recorded the same practice of the chimpanzee. Moore extends the range of action to the young, in the case of a female monkey who tore leaves from the trees as she fled with her wounded baby in her arms and stuffed them into the bullet wound to staunch the bleeding. Darwin notes that the long-armed apes sometimes use their arms as crutches, swinging their bodies forward between them. The smaller monkeys exhibit considerable skill in the extraction of thorns; they search themselves for parasites, and assist their comrades by searching the head and such other parts as each is unable to reach for himself; when a flea is caught they examine it critically, and then swallow it. Darwin mentions an idiot, who, in searching for lice, supplemented the use of his hands with his mouth. Frobenius describes a family party of natives in North Congo seated round a fire searching each other for pediculi; when one was found the captor grimaced cheerfully, and hurriedly conveyed it to his mouth. Moore tells of an orang-outang that was once bled for some illness, who, whenever afterwards he was ill, would point to the vein in his arm, as though he wished the operation to be repeated; we have already remarked on the monkey who similarly appealed for administration of a clyster. These records serve at least to show how readily monkeys learn to appreciate a remedy by which they have benefited. As we descend in the scale of creation we cease to find any sure indications of the existence of a therapeutic sense; it is more than doubtful if we can refer the licking of wounds or the eating of grass by dogs to this category. Cats exhibit a keen sense of maternity, and a mother cat will sometimes attend fondly on her daughter during the birth of her kittens. Cabanès says that they have been known to bite the cord, and carry off the kittens to their own nest, and there lick them clean. I myself can testify to what appeared to be an exhibition of the keenest sympathy of one female cat to

another in these circumstances, anxiously brooding over and licking the expectant mother, but it may well be that this was no more than the prompting of a confused maternal instinct.

Cabanès has some wonderful tales of avian surgery, so wonderful that, for all his sincerity, they would seem to call for further investigation before acceptance. He cites M. Fatio as authority for the statement that woodcock, corncrakes, and snipe have been known to pluck their feathers and make most elaborate dressings for wounds and for fractured legs. In another instance he alleges that a blackbird with a broken leg, failing in its own efforts to dress the fracture, summoned its companions with a cry, and then ligatured it admirably. It is not difficult to see how a wounded and bleeding leg may acquire an adhesive dressing of feathers without postulating the purposive use of the bird's beak in providing it, and still less a summons to other birds to assist at the operation. Anyone familiar with wounded game knows how frequently a matted tangle of blood and feathers is found adhering to a wound. In another case Cabanès mentions a quail found in Savoy with its left leg broken above the ankle. It was wearing a dressing composed of blades of grass rolled circularly round the site of fracture, and bound to one another; the whole formed a lump the size of a hazelnut, and in spite of the riding of the ends of the bones good union had been obtained. Here, again, it seems easier to imagine an accidental winding of grass blades round the broken ankle in the bird's frantic efforts to liberate the hanging foot caught in the grass.

That auscultatory percussion is practised by birds and other animals is beyond question, yet it can hardly be argued that man adopted the practice from them. Woodpeckers, nuthatches, and tree-creepers tap the bark of trees to see if it emits a hollow sound; if hollow, they continue to tap till any insects emerge: in the same way the aye-aye lemur taps the bark in search of caterpillars, and employs its finger as a probe to search for them.

From all this there seems to emerge a strong presumption that below the level of the monkey tribe a healing sense is not existent, and only among the anthropoids is there unmistakable evidence of even a lowly stage of its development. In what I have said I have intentionally limited my remarks to curative medicine, but there remains a further fascinating chapter of animal anticipations of sanitary science, with which we will deal on a future occasion.

Section of the History of Medicine.

President—Dr. Raymond Crawfurd.

(*November* 15, 1916.)

A Revised Chapter in the Life of Dr. William Harvey, 1636.

By Lieutenant-Colonel D'Arcy Power, R.A.M.C.(T.), F.R.C.S.Eng.

It has long been known that Harvey travelled abroad in the year 1636; that he left England in the train of Thomas Howard, the second Earl of Arundel—the father of Vertu in England—when that nobleman was sent to Vienna by King Charles I as Ambassador Extraordinary to the Emperor Ferdinand II in the final stage of the Palatinate discussion; that Harvey quitted the Mission at Ratisbon, travelled to Italy, rejoined the Mission later in the year, and returned with it to London in December.

It must often have been a matter of surprise to those who thought about the matter why Harvey should have been attached to the Mission at all; why, being attached, he should have left it at a time when travelling was so perilous, and why—strangest of all—when he visited Venice, Florence and Rome, he did not go to Padua, where he would have been welcomed by some of his old fellow-students. It is like an Oxford man, who, going to Banbury and Warwick after an absence of thirty years, should fail to take his Alma Mater on the way.

Accident has recently led me to read three books which throw light upon Harvey's movements during the year 1636, and the information I have thus obtained is worthy of record. The first work is "A | Trvè Relation | of All | The Remarkable | Places | And | Passages Observed In | the Travels of the right honourable | Thomas *Lord* Howard, *Earle* of | Arundell and Surrey, Premer Earle, and Earle | *Marshall of* England, | Ambassadour Extraordinary to his sacred Majesty |

Ferdinando the Second, Emperour of *Germanie.* | *Anno Domini* 1636 | By *William Crowne,* Gentleman." It is a quarto pamphlet of 70 pages, printed in London in 1637, and is the diary of William Crowne from the day the Embassy left Greenwich on April 7 until it returned safely to Hampton Court on December 30. It may be noted that Crowne's dates agree exactly with the few dated letters written by Harvey whilst he was abroad.

In 1911 the Historical Manuscripts Commission issued a report on the manuscripts of the Earl of Denbigh, preserved at Newnham Paddox, Warwickshire.[1] Amongst these manuscripts was a packet of letters written by Dr. Harvey to Basil Feilding afterwards the second Earl of Denbigh. The letters were written after Harvey had left England in the train of the Earl of Arundel, and show *inter alia* that he visited Italy with a direct commission from Kin Charles I. These letters were bought by Sir Thomas Barlow, who generously presented them to the Royal College of Physicians of London.

In 1915 Cecilia, Countess of Denbigh, published an interesting account of the first and second Earls of Denbigh under the title of "Royalist Father and Roundhead Son."[2] She gives a detailed account of Basil, Lord Feilding, to whom Harvey addressed these letters, and shows that, like the Earl of Arundel, he was a collector of art treasures and more especially of pictures. I am indebted to her kindness for the use of the portrait on page 42.

It is a little difficult at first sight to explain why Harvey was chosen to accompany the Earl of Arundel, that stately nobleman of whom it was said that he "resorted sometimes to Court because there only was a man greater than himself and went there the seldomer because there was a greater man than himself." A little reflection will show that Thomas Parr was in all probability the link between the two men. The Earl had caused the old man to be brought up from Shropshire as a curiosity to show the King, his age being reputed to be 152. He died in London, and Harvey made an examination of his body on May 16, 1635. Harvey thus became known to the Earl, who conceived an affection for him, found that they had some artistic tastes in common, applied to have him attached to the Embassy, and eventually submitted to become his patient for the relief of an osteo-arthritis from which he suffered.

The Embassy started from Greenwich by barge at 3 a.m. on

[1] Part v, Lond., 1911 (*cd.* 5565).

[2] Methuen and Co., Ltd., pp. xi and 323.

Thursday, April 7, 1636, dropped down the river to Gravesend, took coach there, and slept the night at Canterbury.

Leaving Canterbury on Friday, April 8, they reached Margate in time for dinner, and at 3 p.m. embarked on the King's Ship "The Happy Entrance."

Saturday, April 9, was spent at sea and a landing was made at Helvoets-sluis in Holland on Sunday morning. No time was lost in getting ashore, and after passing Briel the party crossed the river at Maas-sluis, went on by wagon to Delft and reached the Hague the same evening. The Queen of Bohemia, sister of King Charles I, sent her carriages a mile out of the town to meet the mission as the Earl Marshal was well known to her. He had been sent to the Hague in 1632, just after the death of her husband, to persuade her, but ineffectually, to return and settle in England.

The Embassy remained at the Hague from Sunday to Thursday, the time being spent in a series of visits to the Queen of Bohemia, the Prince of Orange, the States, and to the French, Venetian, and Swedish ambassadors.

The journey was continued on Thursday, April 14, through Leyden and Woerden to Utrecht, where the night was spent. Harvey, however, was left behind at Leyden to look after Thomas, son of Mr. Secretary Windebank, who had fallen ill. The illness must have proved trivial, for Harvey had rejoined the party at Cologne on April 21. Lord Arundel speaks of a visit to the Jesuit College and Church on that day, and says "they received me with all civility," and then adds, jokingly, "I found in the College honest little Doctor Harvey, who means to convert them." If Harvey followed the mission from Leyden to Cologne he went to Utrecht, crossed the Rhine at Rhenen, slept at Arnhem, took boat at Emmerich, and slept at Wesel without landing, as the inhabitants were dying of the plague at the rate of thirty a day. He dined the next day at Duisberg, slept at Düsseldorff, and reached Cologne on the Wednesday or Thursday in Easter week. Crowne says of the Church: "The Jesuits have built them a very stately Church and richly adorned it with gildings and erected an Altar, one of the stateliest I ever saw; in the City likewise there is a great Church called the Dome." The Jesuits' Church is still standing in Cologne. It was built in 1618-29 and, to our taste, the decoration is overdone. The party stopped at Cologne from Friday, April 22, until Thursday, April 28, when they proceeded up the Rhine in a boat drawn by nine horses past many villages which had been recently pillaged. The boat

anchored the first night off Drachenfels and the party slept on board. On Friday they anchored for the night off Andernach. Saturday proved an eventful day, for the boat was shelled as it passed Coblenz but without injury, and the night was spent at Boppard. The journey was continued on Whit Sunday, May 1, past Baccarach, "where the poor people are found dead with grass in their mouths," to Assmannshausen and on Whit Monday to Mainz "where we anchored. Heere likewise the poore people were almost starved, and those that could relieve others before now humbly begged to bee relieved, and after supper all had reliefe sent from the Ship ashore, at the sight of which they strove so violently that some of them fell into the Rhine and were like to have bin drowned."

The whole journey so far was hazardous, for active hostilities were in progress, and the Ambassador's boats were accompanied at one time by a guard of soldiers on the bank, whilst at another the party went in deadly fear of the enemy's "outliers," the freebooters of a guerilla warfare. "From Collein hither," says the chronicler of the voyage, "all the Townes, Villages and Castles bee battered, pillaged, or burnt, and every place we lay at on the Rhine on ship-board we watched, taking every man his turne."

Coryat has left some interesting particulars of the Rhine journey in the seventeenth century.[1] "All barkes or boates that come downe do goe very easily, because it is with the streame; which is the reason that all passengers which descend do pay but a small price for their passage: but on the contrary side all that doe ascend strive very painfully against the streame. So that all their vessels are drawen by horses with great might and maine. For this cause all passengers that ascend into the higher parts of Germany doe pay much more for their carriage than those that descend."

The stopping places, too, for each night were not arbitrary, but were governed by the position of the town where toll had to be paid. Coryat[2] says, "I observed many custome towns between Mentz and Colen, which are in number eleven. They belong to divers Princes Spirituall and Temporall, who receive a great yearlie revenue by them. All passengers, whatsoever they are, noble or ignoble, must arrive in each of these places and stay awhile till the boatman hath paid custome for his passage. To the passenger it is no charge at all, but only to the

[1] "Crudities," ed. 1905, ii, p. 361.
[2] "Crudities," ii, p. 295.

master of the boate. If any should dare in a resolute and wilfull humour to passe by any of these places, and not pay the stinted summe of money, the Publicans that sit at the receipt of custome will presently discharge a peece of Ordnance at them and make them an example to all after-commers."

The Main was reached on Thursday, May 3, and Frankfort the same day, "in much anxiety all the way 'from freebooters,' which are commonly called the Boores (a name that is given unto the lewd murdering villaines of the country that live by robbing and spoyling of travellers, being called Freebooters because they have their booties and prey from passengers free, paying nothing for them except they are taken) do commit many notorious robberies near the Rhene who are such cruell and bloody horseleaches (the very hyenae Lycanthropi of Germany) that they seldom robbe any man but forthwith they cut his throat. And some of them doe afterward escape by reason of the woodes nere at hande in which they shelter themselves free from danger."[1]

Hollar, the engraver, probably referred to this part of the journey when he told Aubrey that "Dr. Harvey would still be making observations of strange trees and plants, earths, &c., and sometimes he was like to be lost, so that my Lord Ambassador would be really angry with him for there was not only a danger of thieves but also of wild beasts." It will be remembered that the Earl of Arundel brought Hollar back with him to England and married him at Arundel House to "my ladie's wayting maid, Mistress Tracy, by whom he had a daughter that was one of the greatest beauties I have seen," says Aubrey, "his son by her dyed in the plague, an ingeniose youth; drew delicately."

The party rested at Frankfort from May 3 to May 8. A fresh start was then made past Klingenberg to Neuenkirchen, "guarded by a company of musketiers, but even then the whole night was spent in walking up and down in feare with carabines in our hands."

Würzburg was reached on the following day, Monday, May 9. The accommodation was so bad at Marckbibrach [Markt-Bibart] on May 10 that the night was spent "on the plancher, for the village was pillaged but the day before." It was no doubt unpleasant to have to sleep on the floor, but later in the journey as the party travelled into Austria it became a common experience. "Earely the next morning wee went away and passed through Neustadt, which hath beene a faire

[1] "Crudities," ii, p. 308.

City, though now pillaged and burnt miserably, heere we saw poore children sitting at their doores almost starv'd to death, to whom his Excellency gave order for to relieve them with meat and money to their Parents."

Nuremberg was reached on Wednesday, May 11, and the party remained there until Sunday, May 22, to refresh themselves after the fatigues of the journey, and to see the many objects of interest in the town.

Harvey wrote from Nuremberg to Caspar Hofmann, dating his letter May 20, 1636. I quote Dr. Willis's translation :—

"Your opinion of me, my most learned Hofmann, so candidly given, and of the motion and circulation of the blood, is extremely gratifying to me; and I rejoice that I have been permitted to see and to converse with a man so learned as yourself, whose friendship I as readily embrace as I cordially return it. But I find that you have been pleased first elaborately to inculpate me and then to make me pay the penalty, as having seemed to you, 'to have impeached and condemned Nature of folly and error; and to have imputed to her the character of a most clumsy and inefficient artificer, in suffering the blood to become recrudescent and making it return again and again to the heart in order to be reconcocted, to grow effete as often in the general system; thus uselessly spoiling the perfectly made blood, merely to find her in something to do.' But where or when anything of the kind was ever said, or even imagined, by me—by me, who on the contrary, have never lost an opportunity of expressing my admiration of the wisdom and aptness and industry of Nature—as you do not say, I am not a little disturbed to find such things charged upon me by a man of sober judgement like yourself. In my printed book I do, indeed, assert that the blood is incessantly moving out from the heart by the arteries to the general system, and returning from this by the veins back to the heart, and with such ebb and flow, in such mass and quantity that it must necessarily move in some way in a circuit. But if you will be kind enough to refer to my eighth and ninth chapters you will find it stated in so many words that I have purposely omitted to speak of the concoction of the blood and of the causes of this motion and circulation, especially of the final cause. So much I have been anxious to say that I might purge myself in the eyes of a learned and much respected man, that I might feel absolved of the infamy of meriting such censure. And I beg you to observe my learned, my impartial friend, if you would see with your own eyes the thing I affirm in respect of the circulation—and this is the course which most beseems an anatomist—that I engage to comply with your wishes, whenever a fit opportunity is afforded; but if you either decline this, or care not by dissection to investigate the subject for your self, let me beseech you, I say, not to vilipend the industry of other, nor charge it to them as a crime; do not derogate from the faith of an honest man, not altogether foolish or insane, who has had experience in such matters for a long series of years.

"Farewell and beware! and act by me as I have done by you; for what you have written I receive as uttered in all candour and kindness. Be sure in writing to me in return, that you are animated by the same sentiments."

Caspar Hofmann (1572-1648) had been a fellow-student with Harvey at Padua and was Professor of Medicine at Nuremberg. He had already interested himself in the anatomy of the heart, for he acknowledged the passage of the blood by the pulmonary artery and veins from the right to the left side of the heart instead of by the septum, and modified the idea of the mere to-and-fro motion of the blood in its respective vessels, by likening it to what we see in a lake ruffled by the wind. The veins, however, in conformity with the physiological views of the day, he still held to be the special conduits of the nutrient blood; the arteries the channels of the vital spirits.[1]

As the Embassy left Nuremberg on Sunday and the letter was written on Saturday, it does not seem likely that the promised demonstration was given at this time. I believe that it took place during the homeward journey in November, when the Mission stayed at Nuremberg from November 10 to November 13. The most likely days are Saturday, November 12, or Sunday, November 13, when the rest of the party were engaged in sight-seeing. Tradition says that the demonstration was given in public and proved satisfactory to everyone except to Hoffmann himself. The old man—already past the grand climacteric, with hardened arteries and about to suffer from the stroke which paralysed him for several years before his death—remained unconvinced, and, as he continued to urge objections, Harvey at last threw down his knife and walked out of the theatre.

Leaving Nuremberg for Regensburg on Sunday, May 22, the party slept at Neumark on Monday, and at Hemmaw on Tuesday. Hemmaw had been pillaged twenty-eight times in two years and twice in one day. Regensburg (Ratisbon) was reached the next day—May 24—and the Mission remained there until May 28, when they started off again and were received at a Jesuit's monastery, where they stayed until Tuesday, May 31. On this day they travelled down the Danube in four boats, sleeping at Straubingen and arriving at Vilhofen on June 1. "The next morning as his Excellency was taking Boate he spied a poore Boy standing among other poore people begging for reliefe, who looked very strangely and could neither speake nor heare but a little at his mouth and nose, having neither eares nor passage to heare with, and

[1] Willis's "Harvey," p. 214.

his face very thin & drawne aside, yet when one hallowed hee heard and answered againe with a noise. There was with him his sister, a pretty girle who, when one spoke to him, made him understand by signes. These two his Excellency tooke along with him in his Boate to a City called Passaw, seated on the right side of the Danuby, where we landed & lay, and there commanded to have new clothese made for them & gave them monie and sent them home to their friends."

Passau was reached on June 2, and after three days' rest the journey was continued until Linz was reached on Sunday, June 5.

The Mission remained at Linz until June 25 and the Earl was received several times in audience by the Emperor and Empress.

Harvey wrote twice to Basil Feilding from Linz, once on June 9 and a second time on June 16. The first letter runs as follows:—[1]

DR. WILLIAM HARVEY TO LORD FEILDING.

[1636,] June 9-19. Lintz.—"Right honourable, My sweete Lord, Soe much the more I now condemn my self (having att this hower receyved such sweete and loving lines from you) in that I did not send those letters I intended by the bearer heareof. His suddayne and unexpected departure was the cause that from Nuremberg I did not by writing present my humble service, which I beseech you to accept in excuse, and not lay on me soe fowle a fault as neglect of one soe extreamely well desearving, and to me ever soe kind and frendly.

"I thank your honor that you vouchsafe to advertize me of one whome I hard before would write agaynst me, butt till now never heard he did, or ever yett saw that book. We are heare lately arrived thorowgh that ruined desolat country of Germany into Austria, and att Lintz have had only twise audience. Our bysenes, to expecte the delivery of the Palatinate, is not unknown to your Excelency. My lord will omitt noe dilligens or labour to effect it. This day sum of us accompanyed his Majesty the Emperor a huonting, which was the killing of too deere encompassed by a toyle in a little wood, and soe putt forth for the Emperor and Empres to shoote with carabines, which they performe with greate dexterity.

"The post stayeth for this letter upon thornes, and therfore I must deferr any farther untill the next occasion. Yf ever I have done and may be able to doe service to you, ther is nothing wilbe more comfort and joy unto me, wheare all good endeavours bring forth soe much good frute, and all service is soe plentifully acknowledged.

"I should be glad of any occasion to see Venis once more, soe much the rather to have the happiness of your conversation, untill which time I will live

[1] Hist. MSS. Commission, p. 28.

in hope to see your Ecellent lordship, and in certenty ever to remayne your Excellent lordships humble at command, Will. Harvey."

Postscript.—" Your letter receyved by James Quirke." 1 *p.*

The second letter is also dated from Linz a few days later :—

DR. WILLIAM HARVEY TO LORD FEILDING.

1636, June 16-26. Lintz.—" Not to lett slipp any occasion of presenting my service and thanks to your Excellency for your letters, att this time I am bould to write, and to congratulate with your Excellency of the honorable fame and esteme of your dispatches and abilityes, whereof I heare in that honorable employment you are in, with the expectation of your future increase and perfection therein, as wilbe to our Master and the Kingdom of greate and beneficiall use, and to your self honnour.

" My lord here hath not yett had answeare. We hope it wilbe good and satisfactory, though we are not out of feare of delayes. Our greatest certenty groweth from the necessity they have here of making peace on any condition, wheare ther is noe more meanes of making warr or scarce of subsistence ; and this warfare in Germany without pay is rather a licence to prey and of oppression, and threateneth in the ende anarchy and confusion, then a just and laudable warr to establish peace and justice. I have been twise or thrise a hunting with the Emperor, who certenly in his owne disposition is a pious good man, desierous of all love, quietnes, peace and justise. How the concurrents and interests of the times will permitt him I know not.

" Yesterday my lord was feasted by the nobility att the house of the Count of Melan, the cheife major-domo of his Majestie. We drunke hard, and had many expressions and many good wishes. What will succeed is of noe less expectation and consequences then our desiers are to know it.

" We heare from Ingland the plauge increaseth not much, yet is soe feared as the tearme is for that cause put off. James Querck earnestly desiers to have his service remembered to your Excellency, and hath done well, though he lost his *fede*. My sweete lord, with all the commendation I can, I desier to remain your Eccellencys humbly at command, Will. Harvey." 1 *p.* *Seal* [*with interlaced triangles*].

Neither of these letters wants much comment. The audiences mentioned were those given by the Emperor and Empress to the Earl of Arundel as Ambassador from the Court of St. James's. The first audience was granted on Monday, June 6, and the second on Wednesday, June 8, the day before Harvey wrote the first letter. The banquet, as Harvey states, was given on Wednesday, June 15, when " we were nobly entertained at the Count Megaw's. Perhaps the second letter was written on Thursday, June 16, to while away the time because

"as we were at dinner there came a mightie clap of thunder and lightning which burnt downe three houses presently, being not above an English mile off, on the other side of the water, . . . and about foure of the clocke in the after-noone, his Excellence had audience the third time and we all invited to a Balto by the Empresses command, to the Count Slavataes, who is Chancellour of Prague, where all the Ladies assembled and there spent the time in dancing." Of the *fede* or bill of health needed as a passport, Harvey still had much to learn as will be gathered presently.

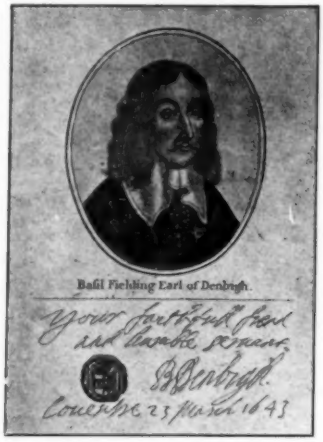


FIG. 1.

Basil Feilding, 2nd Earl of Denbigh. From a rare engraving. [Copied by the kind permission of Cecilia Countess of Denbigh and of Methuen and Co., Ltd., from "Royalist Father and Roundhead Son."]

The letters are addressed to Basil, Lord Feilding, who had been created a Viscount at the coronation of King Charles I. He was the eldest son of the first Earl of Denbigh by Susan, sister of George Villiers, the first Duke of Buckingham. Basil Feilding, after acting as Master of the Robes and Gentleman of the Bedchamber to the King, was sent to Venice in 1634, ostensibly as Ambassador but really to collect art treasures in Italy. Lady Denbigh (op. cit., p. 108) quotes a letter from Mr. Secretary Windebank, dated from Westminster, April, 1635, in which, after some general instructions, he adds: "I am likewise commanded by His Majesty to let you know that whereas Daniel Niz, a merchant, has a cabinet of curiosities of great value, at Venice deposited

with certain merchants of Holland whose names are Vlop and van Noodon that you do your best with that state, that the cabinet be not opened but reserved closed till the return of Daniel Niz, His Majesty intending to buy it off him for his own use." In like manner Lord Arundel and Surrey writes to Feilding from Arundel House in November, 1635: "Noble Lord, I am glad to find my Lord Hamilton and others of our country incline so much to make collections of matters of art, to which it would give so propitious a help and I shall be the more glad to see England increase in them, because I grow so lame that I may have more use of my eyes though I shall have less of my feet." Lord Feilding appears to have been a very lovable person if we may judge from the letters written to him by his mother and his wives. He held high rank in the Parliamentary army during the Civil War, whilst his father and mother remained staunch Royalists. He died at Dunstable in 1675. The exact year of his birth is unknown, but it was before 1608. He was, therefore, aged about 28 when these letters were written and Harvey was aged 58. The Court Medical Department —physicians, surgeons, and barbers—formed a part of the Wardrobe and Harvey would thus have been brought into touch with Lord Feilding when he was Master of the Robes.

The Mission left Linz on Thursday, June 23, going down the Danube by boat and sleeping the night at "a little poore Dorp called Aspagh," probably Aggsbach. Vienna was reached on the following day and the party remained there until Friday, July 1, when the journey to Prague was commenced. Harvey made use of the time by visiting Baden, situated seventeen miles from Vienna on the Schwechat. It is beautifully placed, and is still a favourite resort on account of the warm sulphur springs. He wrote the following letter from Baden to Lord Feilding. The date given as July 9 should be June 29, Wednesday:—

DR. HARVEY TO LORD FEILDING.

1636, July 9. Baden.—"So greate is my desier to doe your Excellency all service as I cannot lett slip any occasion whereby I may give any testimoney thereof. This gentilman, whoe is now comming for Venise, although I love, yett I a little envey, that he should enjoye the happines of that place and your Excellencys sweete conversation and that I cannot. My lord embassador, heare now att Vienna, did receyve att Lintz such an answeare to his demands as caused him to send an express to Ingland, before whose retorne I thinke we shale not see the Emperor agayne. Yesterday we visited at Vienna the Queene

of Hungary and the Archduke, and too very fine little babyes her children. To-morrow my lord intendeth to retorne by Prage in Bohemia to Ratisbone, wheare is expected the diett wilbe; wee finde heare greate expressions and many wishes for the success of my lord his embassadg; how the effects will prove we hope well, butt cannot certeynly assure our selves. I thinke the miserable condition of Germany doth more then requier it. I am this night heare by chance with this gentleman, to see these bathes, wheare such is my bad pen and inke and the shortnes of my time as I am humbly to intreat your Excellency his pardon for this hasty and rude scribblinge and soe, your Excellency his assuredly devoted servant, Will. Harvey." 1 *p.* *Seal of arms* [*but not his own*].

The Earl did not start from Vienna on Thursday, as he had intended, but on Friday, July 1, the party took the road to Prague in wagons, and spent the first night at Holebrun, "a poore village, where we lay all night on the straw, having travelled seven Dutch miles, and every Dutch mile is foure English. The next day early from hence . . . to Swamb, a prettie town where we dined, having past that fore-noon in danger neere a great company of Crabats, who were thereabouts, who frighted the towne, for when his Excellencies harbenger entered the gates an hour before us, they were all shutting up of their shops and running out to defend the towne . . . After dinner wee came to Bodewich, a poore village, where we lay on the plancher, and travelled that day seven Dutch miles." Bodewich is Maihaich-Budwitz, close to Brunn, where the Abbé Mendel recently did such good work in furthering Darwin's theory of Variation. The journey was continued through "a part of a wood called Hertz-waldt . . . to Iglo, a beautifull built towne seated on a little hill, where we lay that night. Earely the next morning . . . thorow Haybeireitz, a village, in which an Oast killed at several times of his guests ninetie men and made meat of them . . . to Holebrum, where we lay that night on the plancher, which was a most fearful night of thunder and lightning, having travelled seven Dutch miles. The next morning wee departed . . . past a silver mine of the Kinge of Hungaries, which was by the way side on a little hill, into which wee entered to see their works, the oare being two hundred and fiftie fathom deep . . . and thence to Colen, two English miles off, where we dined, and so to Bemishbrade, where wee lay on the plancher againe. The next morning earely being the sixth of July, from thence to Prague to dinner, being five Dutch miles," and in the afternoon a Rabbi circumcised a child, no doubt Harvey was present at the ceremony. The party remained at Prague until July 13,

the time being spent in seeing the numerous sights for which the town was celebrated. A masque was performed at the Jesuit's College, over which an Irishman was presiding.

The return journey to Regensburg was begun on Wednesday, July 13. The party left in wagons, and slept the first night at Beroum, the second night at Pilsen, the third night at Bishopsteine, having travelled only four Dutch miles as the road lay over the Böhmerwald mountains. Redtz was reached the following night, and the party was safely back at Regensburg on Sunday, July 17.

The Embassy remained at Regensburg until July 21, when his Excellency determined to visit Augusta (Augsburg), where there were many art treasures, in order to kill time, as the Emperor had not yet arrived at Regensburg. The first night was spent at Neustadt. "My Lady Abbesse gave his Excellence a banquet" the next day at Bezanzon [? Münchsmünster]. The night was spent at Palermo [? Ingolstadt], and Augsburg was reached the following day, Saturday, July 23.

Harvey left the main party at Augsburg, and travelled into Italy to see Lord Feilding at Venice, and "execute a commission for the King," he says in his letters. The nature of the commission is explained in a letter written from Ratisbon by one of the Embassy, which says, "Honest little Harvey whom the Earl is sending to Italy about some pictures for his Majesty." The distance from Augsburg to Venice is about 460 miles, and the journey was made on horseback with one or two attendants. There is no indication as to the route followed from Augsburg to Villach, but it is probable that he went from Augsburg to Münich (67 miles), from Münich to Innsbruck (105 miles), from Innsbruck to Franzensfeste, over the Brenner pass (49 miles), from Franzensfeste to Villach (13 miles). The letters show that he travelled from Villach to Pontebba (30 miles), from Pontebba to Sacile, from Sacile to Conegliano (9 miles), and from Conegliano to Treviso (17 miles). At Treviso he was only 18 miles from Venice, and must have thought his journey ended, only to be woefully disappointed. His first letter from Treviso is dated Wednesday, August 3, and if he left Augsburg as soon as the Earl of Arundel arrived there on July 23, he covered the whole distance at an average rate of 40 miles a day. The only news of him during these ten days is contained in a letter from Sir Thomas Roe to the Queen of Hungary saying that Dr. Harvey assured his private friends of great hopes of justice and equity from the Emperor, but he believes the Doctor judges by symptoms like a physician.

At Treviso he met with a serious check, which, for a time, spoilt both his health and his temper. His own letters tell the tale.

Dr. Harvey to Lord Feilding.

1636, Aug. 3-13. Treviso.—"My sweete lord, I came this morning to the gates of Treviso with greate joy, and hoped this night to have had the happines to have beene with you att Venise, butt I have receyved heare a very unjust affront, being stayed and commanded by this podesta to have gone into the lazaretto, without any cause or suspition alledged. I tooke my first *fede* under the seale of Ratisbone, a place free, and now destined, as your Eccellency knoweth, for the meeting of the Emperor and all the rest of the princes, which yf it had not beene soe, they would not have com thither, it being infected or suspected. Since, in every place as I came, I caused my *fede* to be underwritten, soe that there is no ground for them to lay any suspition upon me. And att this sentence on me by the podesta (that I should goe to the Lazarett) I absolutely refused, and sayd and offered to shewe that I had the pass and recommendation of his Majesty the King of Greate Brittane and of the Emperors Majesty and of my lord Embassador his Excellency, and that I had to goe to princes and men of quality, and that my busines required expedition, and desier'd they would not hinder me, butt, as my passes required, further me and that I mought not bring that suspition and infamy on me, besides my own security, to goe to such a place as lazaretto, whear they use to putt infected persons, and that I had shewed them sufficient *fede*. Notwithstanding all this, heare I am to lye for ought I see in the open base [*sic*] feilds, God knows how long. The podesta refuseth to see or reade my passes, and I cannot cum att him to speake and use my reasons. I am afrayd this lying in the feild will doe me hurt in my health. I beseech your Eccellency to lament hearof. It is unjust to proceed with any man thus without cause and otherwise then Venetians are used in Ingland or soe merrit to be used heare, and otherwise then is fitting for the respects there shold be used to the passes forenamed.

"I pray pardon this scribling on the grass in the feild, and procure with all expedition my freedom from this barbarous usadg. Your distressed frend and humble servant of your Eccellency." 1½ *p.*

The Same to the Same.

1636, Aug. 6-16. Saturday.—"I perceyve heare by there behavier to me how much your Eccellency is pleased ther to stirr and laber for me, for yesterday after I had sent my letters to your Eccelency, they sent sum in a coatch to me, as from the podesta, that I should goe to the other place, wheare I was before (yf I would) or that I should have heare a bed or that he would doe for me what he could, to which I answeared, that since it had pleased him with soe much rigour and cruelty to inflict upon me the greatest misery

he could and had brought soe much infamye upon me as to putt me into this lazaretto without any just cause, without any respect of the recommendations I had from my lord Embassador his Excellency or from the Emperors Majesty or from his Majesty my master, not soe much as to reade them or give notice of them in his first dispatch to Venis, nor to make any difference of a servant of his Majestye the King of Greate Brittan, butt by force and threatning of muskets to compell me into the very nasty roome wheare the vitturin and his two servants and saddels lay and not att my request granting me a bed or any commody scarce straw; his offers now weare unseasonable and like phisick when a man was ded and that I had now hardened my self and accomodated as I did content myself and resolved, since it had pleased God by his hands to humble me soe low, I would undergoe it as a pennance and that I had written to your Eccellency and hoped by your intercession within sum few days to have release, and therfore determined to receyve and acknowledg all my comfort from you and to troble the podesta with noe other request but that he would with all expedition free me and shew a respect to my master and my bysines; and debating the bysines and urging them for a reason of all this and that it was unjust to detayn any man and not shew him the cause, or to receyve a man into ther territoryes and then imprison him, they should have denied me entrance att the first and then I had gone sum other way for they should have putt those townes they suspect into ther bands and then I had shunned them or make known att his entrance to every man what he was to doe, otherwise this was to surprize and catch men: and they knowing not well what to answear sumtime alledged that Villach was suspected, sumtime I had not gotten my *fede* subscribed att Conian or Sacile, sumtime that the vitturin had brought a boy with him, his son to gett a master, whose name was not in the vitturins *fede*, soe sumtime I was stayed for him, sum time (they sayd) he and his horses stayed for me.

"Touching ther suspition I answeared Villach took as great care and examined my *fede* as striotly as they could and had given me *fede* of ther safety which they ought in civility to trust, and that the Duke of Alkalay [Alcala] viceroy of Naples with 100 persons choosed to stay there. And that upon bare suspitions of ther owne without any just ground, ought not to be thougt cause enough, to use me in all respects as if I had the plauge for certentye on me, and that if I had had it would they not have granted me in charity a house, bed and succour for my money though all had beene burned after, and I have payd for it. It was agaynst all manhood and charity. And for not having my *fede* subscribed in ther own towns as we passed, they knew well I could com noe other way from Pontevi and that they weare all without suspition and that I was towld, and it was and is in every man's mouth ther was noe neede, and that it was upon accident for our vitturin whoe should have directed us being strangers gott his own *fede* subscribed att Connian, and for the horses we rood on, and did not tell us untill it was to late, thinking his was sufficient. Butt for all these cavills, I sayd the word of an honest man or his oth in this case ought to suffice. I write the larger to your Eccelency of those passages

because I know not what they may make of my conference in ther letters that ye may know the truth, and indeed my lord I am a little jealous of them, and to take anny beds now of ther sending, for since ther manners and cruelty hath been soe shamefull to me, and they have soe little reason for what they have done, it would be like the rest of ther proceedings yf they sent me an infected bed to make ther conjectures and suspitions prove true; therfor I choose to ley still to be redeemed by your Eccelency oute of this inocent straw. Yesterday likewise the patron that owed the howse wheare I first took my straw bed (a little poore garden howse full of lumber, durt and knatts, without window or dore, open to the high way att midnight) was to offer me that agayne, because I had chosen that to shun the infamy of this lazaret and the suspition I had that sum infected person had lately bene heare, and from which they forced me with terror of muskets, I write this to shew your Eccelency that all they doe heare upon your stirring is butt formal to salve ther own errors. I tell them I desier nothing of them, or expect or will except, but only beseech the podesta that I may be att liberty with all expedition, and that att last he will have respect to princes recommendations and to my bysines: and now as I am writing I humbly thanke your Eccelency, your servant is arrived and hath beene with me and is gone to the podesta according to your order. He will tell you of a trick to burn my pass and the injury they have offered me therein.

"When your lordship shale marke how tedious I am in writing I pray give it this interpretation, I have noe other thinge to doe and infinite greedy to be gone, and that I scrible thus, in hast and the want of good pens and inke, etc.

"Yf your Eccellency goe to the Colledg ye may justly lament the little respect this podesta hath given the recommendations I have from my lord Embass. and his Majesty, or the bysines I am sent on, whoe would not soe much as receyve it and read it being offered nor send information thereof to Venis, nor make difference thereupon betweene me and the vitturines servants, would give me noe releife or assistance, not soe much as a barne or stule free to myself butt force that infamy, danger, suspition and base usadg of ther lazarett upon me, not to suffer me to write to your Eccellency untill 5 or 6 howers past, that in the meane time he mought procure an order from Venis to countenance his act and injure me upon unequal relation; and your Eccellency may justly resent that the dispatches to you and bysines of yours should be thus used and not upon your letters released and that ye may have that respect therein which is due, and that I may have reparation and testimony for the burning my pass and for the clearing me of the suspition and infamy of having beene in the lazarett, and my unjust stay, and that I may have agayne my *fede* to make appeare to the world wheare soever I goe that I am cleere, or els that I may have a full *fede* from this state. Yf they make difficulty of my comming to Venis, I pray that I may have sufficient *fede* from hence and I will goe by Padua to Florence and see your Eccellency as I retorne. I pray pardon me for propounding this to your Eccellency whoe know better hearin what is to be done which I doute not but you will performe, that I may

be free and we rejoyse together hereafter; and in good sober truth I feare least this ill usadg and base place and the unquiett of my mind may not bring sum sicknes on me this extream hott wether therefor I beseech etc. Your Eccelencys humble servant." 3 *pp.*

DR. HARVEY to LORD FEILDING.

[1636, Aug. 9—19.] Tuesday afternoon.—"My sweete Lord, this place is soe incommodious to me, and affordeth me soe little comfort, as I beseech your Eccelency to pardon me yf I take the bowldnes herein to make my complaynts unto you. The great longing I have to be gon and free maketh me thinke these 4 days past (since I had the comfort to see your servant here) to appear so maney yeares, whearin I hoped ether they would have relented of ther cruelty or your Eccelency effected somethinge for my releife. I had thought with joy to have presented my service unto you, and now am sory instead therof to putt your Eccelency to the troble I knowe ye take for me.

"The ill diett I have heare, and the wors usadg hath produced this ill effect that now these two nights I have had a sciatique in my right thigh and legg that much discorageth me, and maketh me lame. I would fayne Signor Francesco [Vercellini] would come unto me. I will pay for his coatch and expence, to direct and advise me, and to deliver him the busynes I have to him from my lord Embassador and the letters I have els to Venis; and yf he bring my freedom with him, I shale have the more joy; yf not, he may gett me heare some garden house, with fier, bed and other necessaryes, least I fale wors. Yf his being there effect better for me, then that som man be hired theare to com and go between, by whom I may heare often what is or can be done, and may certefy me of the receyte of my letters att the least, that I may heare what I may hope or looke for. They tell me heare, yf there be any truth in them, that they have sent to the Duke for my liberty, and that they desier I would write this to your Eccelency, that by your joynt helpe it may be procured. I pray that Signor Francesco would come. Thus in hast, I pray pardon and releve. Your Eccelencys humble servant." 1 *p.*

DR. HARVEY to LORD FEILDING.

[1636, Aug. 12-22.] Friday. Treviso.—"Although I know your care and dilligence for my liberty, and make noe dowte butt your Eccelency doth what is possible and omitteth noe occasion, yett the longing I have to be out of this thraldom and the dayly hope from you maketh me soe often look oute as having not heard from you sinse your man was with me (on Satterday last). I desier much to know how the case standeth, what is the cause, what I may expect. Ther is nothing can beare any color of just objection butt that my *fede* was not underwritten att Conian [? Conegliano] and Sacile, which towns they know well enough are cleare, and by the computation of my journey from Pontevi [Pontebba] it is not possible I could take any other way, butt that I

passed those townes wheare it was tould me that it was not necessary for my *fede* to be underwritten since I had the seale of St. Mark att Pontevi, and yett the vitturin had his *fede* underwritte att Connian for him and the horses we rode on and owers had been underwritten too but that he which was to guide us tould us when it was too late, and sayd his underwritten was sufficient, and whearas it was sayd we had one in our company more then we had *fede* for, that was not soe, for that party had a *fede* for himself att Pontevi though after not underwritten.

"I feare lest there may be some other matter in it then I imagin and they meant to stay me, had I the best *fede* could be (as I thinke I have) and that they seeke butt cavills to colour ther intent, otherwise the word of an honest man or his othe would easily give satisfaction for such slight douts; they have since and before letten pass others upon as little testimony. I hoped much on your Eccellencys complaynt to the Colledg butt now because I heare not I dowte much least they neglect you too. I have now bene heare 10 dayes and my *fede* giveth me testimony of health for 40 days almost before that, soe that I cannot guess other then sum malis in this, considering with what cruelty and severity they have proceeded with me. My sciatiq which I gott heare by injurious lodging, I thanke God mendeth well. I beseech you my sweete lord lett me hear from you att least that I may know these letters com to your hands which I write, and what I may hope for, and what reason ther can be of the greate neglect they have used to the recommendation and the passe I brought from my lord Embassador, the King his Majesty and the Emperour. I would be glad since my stay is soe long to have a trusty messenger to send all my letters I have to Venis, and to that end I have sent to Signor Francesco [Vercillini] to whom the greatest parts are, that he would com hither, my lord Embassador in my last letter from Ausburg commendeth him unto your Eccelency, and sayth ther is nothing yett fallen out worthy of your knowledg, otherwise he would have written to your Eccelency er this. Even as this morning I had finished thes lines, came one from this podesta to vew us how we weare in health and sayth within these 2 days we shale have liberty, butt what trust may be given to there words I cannot tell. I feare it is butt a shuffel to deteyne me heare yett a weeke longer, which is the extremity they doe to the worst *fede* and meanest man; likewise it is tould me that Signor Francesco should write soe much to a frend of his heare who is restreyned to his howse, who sent, I thinke, him to me to excuse him. I wonder Signor Francesco, I having written so ernestly to him he did not write a word to me, I know not the passages of your Eccelency being in the Colledg, but suer I am they have used a neclect and contempt of his Majesty's recommendation in his pass and of the Emperor worthy to be hotely complayned of, and to me have done barbarous injustise and incivility. Ther is a post commeth every day from Venis. I beseech your Eccellency to be a comfort to me that I may have butt one word. Of your Eccellency an humble servant and faythfull frend."

Postscript.—"I humble desier to know when the soonest post goeth for Ratisbone, that I may provide letters." 3 *pp.*

DR. HARVEY TO LORD FEILDING.

[1636,] Aug. 13-23.—"My sweete Lord, becaus I see heare nothing, nothing butt injury, deceyte and jugling every day this eleven days, that to-morrow and att night and to-morrow and shortly I shall be released, and doe not heare from Venis any certenty by any hand; and I ley heare in a miserable case; I pray pardon me yf to your Eccelency I seme in this often sending importune; eccept by your Eccelency his means (in whom is my only hope to gett release from these barbarous oppressions) they delight hear soe to exercise there tirenny as I am like lye for every day they promise a weeke. I feare none of my letters com to your Eccelencys hand or to Signor Francesco; I make noe dout butt your Eccelency hath don for me what is fitting and have procured my releas long befor this time butt that your letters and your help is kept from me. Therefore I pray earnestly I may have but one word in answeare, that I may knowe my letters come to you and what is done, which was my chefest requeste to the gentleman your Eccelency pleased to send to me seven days agoe. The post commeth every day, and even to him that night this podesta sayd he expected from Venise, and soe will doe by his good will I feare this month, to your Eccelencys humble servant."

DR. HARVEY TO LORD FEILDING.

1636, [Aug.] 16-26. Treviso.—"I wrote to your Eccellency yesterday what a heavy messadg these of the Sanita hear delivered to me from the Senate att Venis, which was that I must stay heare yett untill farther order; and asking how long, they sayd seven or ten or twenty dayes, soe I perceyve they doe butt abuse your Eccellency, to beare you in hand that every day I shale have my liberty, and therin they betray me and make me loose my time, with whom yf they had delt playnly and rowndly, I mought have gone back att the first to Villach and from thence to Gorilia, and there gotten shipping and beene by this time at Rome or Florence, and sene your Eccelency and dispatched my bysines att Venis comming back. Now yf I stay a weeke or ten dayes more heare, I shall loose soe much time as the intent of my jorney wilbe broken, and I must retorne without going farther. Good my lord, I beseech you, putt them spedily and rowndly to it, ether that I presently goe (having now beene 15 days) or that I may retorne, which is a thinge is usuall heare, and a little while agoe they did it, sending ther officer with them untill they weare oute of ther territorye, and in justice they cannot deney your Eccelency one of these and indeed nether, yf ether they did respect any thinge your intercession or would doe justice. I perceyve I am fallen into the handes heare of most base and evel people, and now they begin to accuse one an other, and when I aske them the cause of my stay, they forge lyes, as that I was att Saltburg, and that Villach hath the plauge, and I know not what, and in this place they have talked soe much that to-morrow and to-morrow I should be free, and when they heard your Eccelency stirred in it, expected noe less than

present delivery, that now they begin to disesteeme what your Eccelencys favour can doe for me. My lord, I pray therfore urge further the disesteeme and neglect of his Majestys pass, and your intercession, that they stay me for comming from Villach and yett itt is nether a towne in ther bande, and they lett all others pass from thence but me, two having passed by *fede* from thence since I ley here.

"I beseech your Eccelency to pardon me and not thinke this often writing importune, for having soe often written and receyving noe answeare from you, which in all my letters I did soe ernestly requier, and it did so much concerne me to know the particulars as fearing my letters come not to your hands, I send this messenger of purpose to bringe me or write me, whether your Eccelency have any hope, or have or intend anything, and what answeare they give and wheather ye have or intend to complayne of the unjust and barbarous dealing with me att the first, soe much to neglect the King his Majesty's pass and recommendation as not to reade it, not therupon to have made some difference betwene the usadg of me and the vitturin and his servants, butt cheefly in staying me and putting me into ther lazarett, having brought sufficient *fede* and such as they lett others pass with all, butt yf of these they will not be sensible of, to give present reparation, then to demand my *fede* back agayne oute of the Sanita and a testimony of my being heare in lazarett, and my passe burned, and that I may goe back (which I now (yf I cannot goe presently forward) would be glad to doe with Signor Francesco). Ether to goe forward or backward presently they cannot in any justise denye, and I never longed for any thinge in all my life soe much as any way and on any condition to be gone from this base place and barbarous poeple and fearing lest I should be sick and then they would crye me into the plaug, and keep me and cheate and tyrunise over me, God knoweth how long. Signor Francesco was with me on Sunday last and tould me (I humbly thanke you) with what desier and ernestnes your Eccelency dealt for me, and that ye hoped every day, butt other perticulars I could not learne by him, nor since. I send by this bearer the letters I had to deliver att Venis both to your Eccellency and others and a packet for my lord Herbert which was caryed to Ratisbon by James Querk and my lord being not in those cuntryes, is retorned back. Your Eccelency please to pardon this troble which my unfortunat change hath inforced me to put you to. Your Eccelencys humble servant." 2 *pp.*

No more is heard of Dr. Harvey after this letter until he writes again and in good spirits from Florence:—

DR. HARVEY TO LORD FEILDING.

[1636,] Sept. 7-17. Florence. — "My sweete lord, with many thanks I humbly present your Eccelency for all the favour I have receyved att my being att Venise. Since I came safe to Florence, I have seene this fayer citty and enjoyed much contentment therin, with health and mirth. The Grand Duke his highness receyved my letters and me with greate curtesy, favour and respect; talked often long and familiarly with me, presented me with frute,

fowle, wine &c., gave order for one of his coatches to attend me whearsoever and whensoever I went abroade, shewed me himself many of his rarityes, would have given order for a gally to have carryed me from Leghorn to Naples and when I thanked his highnes for his affection and love to his Majesty and his affayres, sayd there was nothing in his court or power that was not at the King of Ingland his service, seemed to love and honor him very much, much enquisitive of him, his health and welfare, customs and vertues. I tould him, as your Eccelency commanded me, of your devotion and promptenes and order ye had to doe him all service, which he accepted very kindly, and commended him unto you, and certeynly yf ye came hither, would doe you all possible honor. It may be his marriadg is shortly to be consummated; it wilbe a fitt occasion to have order to congratulate. I perceyve heare myself to have much acceptance, access and familiarity, whereby it may be (att least I desier) to performe his Majestys service, or for your Eccelency or any your affayres.

"Here is a greate coort. The Duke of Loreyn and his Dutchess, to whom the Grand Duke giveth the hand; the Duke of Guise, his lady and his sonnes, Prince Janviel [Joinville], the Duke of Joyeux and too little ones, knights of Malta, and a daughter marriedgable, besides the Dukes sister, his too brothers, and the Cardinall and one of his uncles. Your Eccelencys humble servant." 1 *p.*

After this letter there is again a gap in our knowledge of Harvey's movements until October 5, when he was at Rome. The Pilgrim's book at the English College shows that he dined in the refectory and that Dr. Ent dined there the same night. The two travellers probably met by arrangement, for Ent was born at Sandwich near Harvey's birthplace—Folkestone, and had taken his degree at Padua on April 28, 1636. We owe the publication of the treatise "De Generatione Animalium" to Ent's care.

From this date there is again a gap until November 5. The King recalled Arundel on September 27, and Harvey probably received orders to return at once to Regensburg. He visited Lord Feilding at Venice on his way back, and wrote the following letter on his arrival at Regensburg:—

DR. HARVEY TO LORD FEILDING.

[1636,] Nov. 5-15. Ratisbone.—"Right honorable and eccelent my sweete lord, I have within the time prefixed at my departure from Venis, now safely atteyned my Lord Embassador att Ratisbone, where I find him ready within too days to depart for Ingland, having his letters of revocation, and yett visited and visiting as yf all weare on better tearmes then as yett it seemeth to me to be, but more certeyne perticulars your Eccelency, I presume, shall understand by himself. I only write thus much to be an introduction to present my humble and harty thankes to your Eccelency for my kinde enterteynement and the rest of your many favors to me, which as I can never forgett, soe will

I never omitt any occasion whearin I may (by performing to your Eccelency any service) testifie my gratuity or get any opportunity to wish and pray for your Eccelency all happy success and prosperity. Your Eccelencys humble servant Will Harvey." 1 *p.*

The actual journey homewards was begun on Tuesday, November 8, when the party left Regensburg early, arriving on the third day at Nuremburg, where "the Lords of the City came and presented their service to his Excellence in a long Dutch complement, and after dined with him. And the next day in the afternoone they came againe with a present of 40 Flaggons of wine, and three killots of Fish, which was brought in by thirty men all in red Coats, guarded (embroidered) on the armes, with white and red caps . . . and two pictures of Albert Durer and his Father, done by him . . . and then presented his Excellence with a Banquet. And from hence to the Castle where the Father of one of the Lords lived . . . and he presented us with another Banquet. The next day, which was Sunday, they all dined with his Excellence. And in the morning being the 14th day (of November) wee departed, having stayed heree three dayes, and tooke a convoy of 100 musketiers along with us to Neustadt—five Dutch miles. The first night travelling part by torchlight through the woods and there lay on the straw that night. The fifteenth day earely to Ketzen, and there lay on the planchers likewise; and the next day to Wirtzburg . . . staying that night there. After dinner the Lords of the Towne sent his Excellence a present of two and thirty Flaggons of wine, Fish and provision for his Horse."

It may be noticed that when the Embassy was received with lavish hospitality the Earl of Arundel had been making purchases. Thus at Nuremburg he bought the Pirkheymer library which had belonged to the King of Hungary. This library was afterwards presented to the Royal Society by Lord Arundel's son through the good offices of John Evelyn. At Cologne, on the outward journey the Bishop of Meyence "sent one of his Privie Counsell to invite his Excellency the next day to dinner. He then sent three of his Coaches for us and gave his Excellency very noble entertainement. The first night his Excellency came, were presented unto him twenty-foure flaggons of severall kindes of Wine, the next day twenty-eight and at every present there was a long speech made to his Excellency in Latine by one that came with the Wine, which came all from the Magistrates of the City in Flaggons with the City Armes on them."

"The next morning before his Excellence departed from Wirtzburg

he was visited by the Bishop of Wesburg, whom wee found in the habit of a Countrey Gentleman, setting aside his Order, which is an enamelled Crosse hanging on a blacke ribbon about his necke; who made very much of his Excellence and presented him with the picture of Our Ladie, done by Albertus Durerus, being one of his best peeces." The picture must indeed have been a masterpiece for the Earl says, writing from Frankfort on December 5: "I wish you sawe the picture of a Madonna of Dürer which the Bishoppe of Wirtzburg gave me last weeke as I passed by that way and though it were painted at first upon an uneven board and is varnished, yet it is more worth than all the toyes I have gotten in Germanye and for such I esteeme it, having ever carried it in my owne coach since I had it; and howe then doe you think I should valewe thinges of Leonardo, Raphaell, Coregio and such like?"

The seventeenth day of November taking a fresh convoy the party slept at Bishopsheim and started early next morning passing through a hilly wooded country in much danger of the Croats . . . to the number of 6000 or more who were pillaging and robbing. The 19th day in the morning taking a fresh convoy they came to Selgenstadt still travelling in danger of the Croats, "where as soon as his Excellence lighted the Grave Vandosme, Governor of the Country for the Bishop of Mentz sent his Excellence a present of half a wild Boar and likewise provision for his Horse, knowing that the town could not afford anything. The twentieth day being Sunday, early in the morning we went thence to Frankfort. And the next morning after his Excellence went to Hannaw to visit Sir James Ramsey, a Scotch Gentleman and Governour of the town. The foure and twentieth day foure of the Burgers of the Citie came and presented their service to his Excellence with twenty flaggons of wine and then dined with him." A stay of three days was made at Frankfort. The journey by river was begun on Saturday, November 26. Mentz was reached on the 27th where "divers poore people were found lying on dunghils almost starved, being scarce able to crawl for to receive his Excellencies almes, and presently returning to our boate to dinner wee afterwards releeved many poor hungry soules with the fragments. Thence to Rudeshem . . . and there cast anchor and lay on the boards likewise.

Very earely the next morning we weyed anchor . . . and came to Bacharach where some of our Company did but goe ashore (and presently hastened after in a little boat) were pursued by five Musketiers almost to his Excellencies boat, who discharged very often at them, yet

by good fortune mist them . . . then going on to a large island an English mile from Coblentz we there cast anchor and lay all that night . . . which night we lay in much danger . . . for as some of our company did but goe a little way from our boat, they were layd hold on, and one that fled had a Musket shot at him and hee that was taken they carried before their Commander. The next morning his Excellence sent againe to the Governor for passage, who like a base fellow made us stay that night also and the next day until three of the clock in the afternoon and would not let us passe for all that his Excellence had sent him the Emperor's Passe and Letter wherein hee was commanded not onely to give passage but to assist him in anything hee required; yet for all this he kept us still and would not give way that our Trumpeter might go to the French in the Castle; but they perceiving how unworthily he did deale with his Excellence discharged four or five cannons at his house and shot quite through it." Bonn was reached on November 30 and the boats anchored there for the night but the party durst not land, the plague "being very sore in it." Cologne was reached the next day. Coryat[1] says "There is a very strange custome observed amongst the Germanes as they pass in the boates betwixt Mentz and Colen, and so likewise betwixt Colen and the lower parts of the Netherlands. Everyman whatsoever he be poore or rich, shall labour harde when it commeth to his turne, except he doth either by friendship or some small summe of money redeeme his labour. For their custome is that the passengers must exercise themselves with oares and rowing *alternis vicibus*, a couple together. So that the master of the boate (who methinks in honestie ought either to doe it himselfe or to procure some others to do it for him) never roweth but when his turne commeth. This exercise both for recreation and health sake I confesse is very convenient for man. But to be tied unto it by way of a strict necessity when one payeth well for his passage was a thing that did not a little distaste my humour."

The party stayed three days at Cologne "until we had exchanged our boats for bigger and every day his Excellence had presented unto him 24 Flaggons of wine sent from the Magistrates who once dined with him.

And on Sunday the 4th day of December about foure of the clock at night tooke shipping and the next morning at three a clock set sayle." Toll was paid at Zonz and so to Düsseldorf "where as soone as we

[1] "Crudities," ii, p. 299.

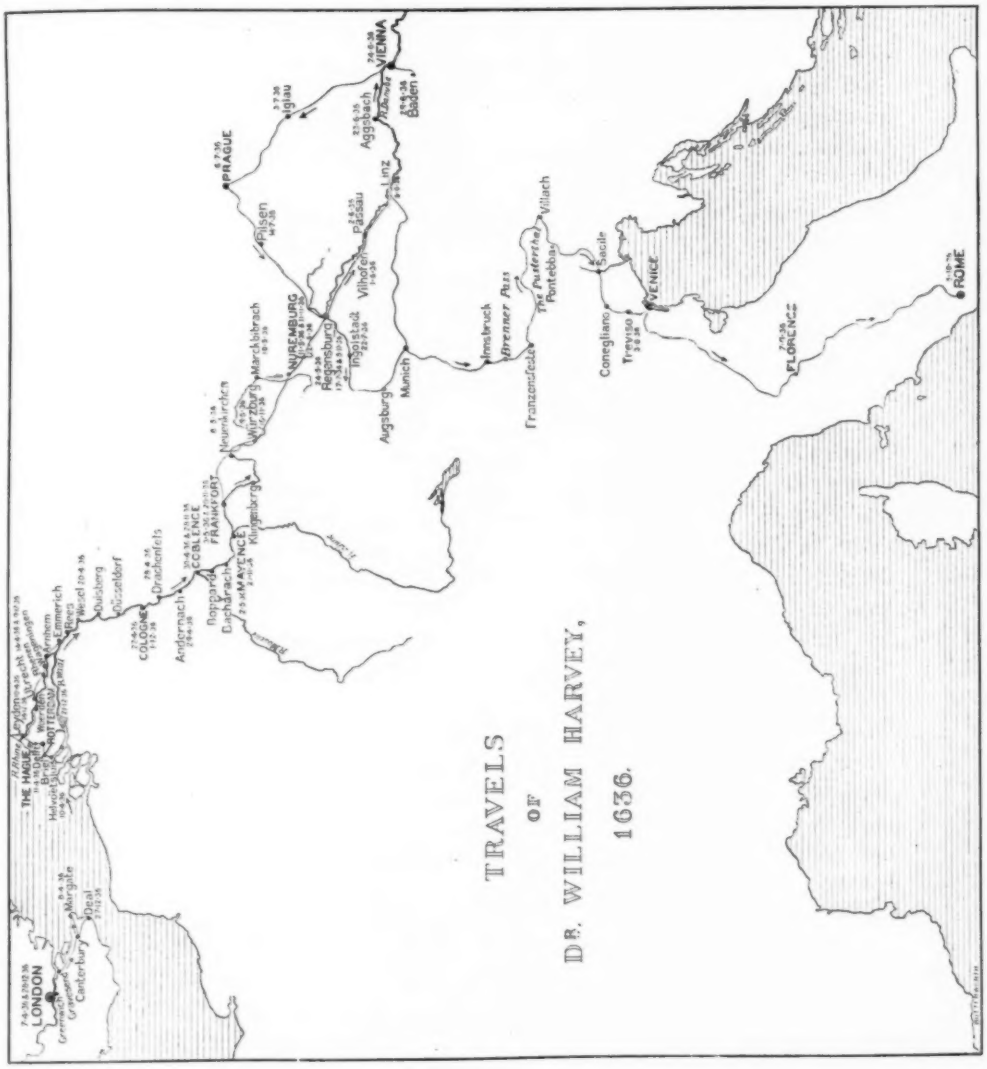

TRAVELS
OF
DR. WILLIAM HARVEY,
1636.
LONDON
Canterbury
Margate
Deal
THE HAGUE
Leyden
Utrecht
Arnhem
Emmerich
Wesel
Duisburg
Dusseldorf
COLOGNE
Drachenfels
Andernach
Coblentz
FRANKFORT
Hoppard
Dashdrach
MAYENCE
Klingenburg
Neumarkt
Neumarkt
Amberg
Weiden
Marchbirach
NUREMBURG
Regenoburg
Ingolstadt
Augsburg
Munich
Innsbruck
Brenner Pass
Franzenfeste
Pontebba
Villach
Sacile
Conegliano
Treviso
VENICE
FLORENCE
ROME
PRAGUE
Iglau
Pilsen
Aggbach
VIENNA
Baden
Linz
Passau
Vilhofen

FIG. 2.

came but neere the shore out came the Noble Duke of Neuburgh and clambered over other ships to come into ours to visite his Excellence being much joyed at his safe returne and had made provision at his house to entertaine his Excellence, but perceiving he would not stay sent for a wilde Bore, wine, and five pictures and presented them to his Excellence and then tooke his leave . . . he staying by the shore and walking along as farre as the water would give him leave and stayed untill we were out of sight. . . . This night wee lay in much danger for there did lye on each side of us parties which robbeth and pillageth all Passengers; for wee saw above fifty in a company going all along by the shore, but a little before wee cast anchor and at 10 of the clocke in the night, being very darke, was a false alarum given by the watch of a partie cumming, which made us all flye to our weapons, at last perceiving it was but one boate and they that were in it crying out 'Friends from the Duke of Neuburg' else wee had shot them, who came for to have passage into England."

Utrecht was reached on December 13, the previous day being spent in very bad travelling on account of the weather. At Leyden the next day His Excellency viewed the chief things of note in the town, as the Universities and the Anatomy school. From thence after dinner to the Hague. "Here we stayed until December 21st and then left for Rotterdam. On Saturday being the 24th December (and Christmas Eve by our style) at 11 of the clocke in the night tooke boates and went to our ship called the Garland and about 3 in the afternoone set sayle and sayled over the barre, having a Pilot sayling before us with a lanthorne on the top of his mast sounding for the depth all the way. And the next day at twelue of the clocke cast anchor in the Downs and there rode and could not land for the roughnesse of the sea until Tuesday morning the 27th December and then landed at Deale and from thence by poast to Canterbury and so to Sittingbourne to bed.

"The next day in the morning earely to Gravesend and there tooke water to London, where on the way my Right Honourable Lady met his Excellence, who exchanged barges and there she entertained him with a banquet and so earely the next morning, went to Hampton Court to His Majesty."

An allowance of £19,262 was made for the expenses of the Mission and of this amount £7,262 remained unspent.

Two or three additional facts about Harvey's accomplishments may, I think, be gathered from this account of his travels. In the first

place he must have possessed some technical knowledge of art. It is clear that he had much in common with the Thomas Howard, the second Earl of Arundel, and with Basil, Lord Feilding, who were both great collectors. The Arundel marbles given to the University of Oxford in 1667 are still famous. Feilding brought over many pictures from Italy which found their way into the Royal collection. Harvey's interest in art must have been known to and appreciated by King Charles I, himself no mean judge, or he would not have been commissioned to leave the Embassy and proceed to Italy to collect pictures. It is possible too, that Harvey shared some of the business capacity which had made his brothers successful merchants in London and if this were the case there is no doubt that the King hoped to have found not only a good judge of art but a shrewd bargainer as well.

The fact that Harvey at the age of 58 undertook so arduous a journey through a country desolated by war shows that he was of an enterprising spirit and loved travel, whilst the little side light in his letter from Linz that "we drank hard" tends to show that perhaps his gout was acquired and not inherited, for it must have needed some previous training to have enabled him to hold his own at a seventeenth century Kneipe.

(*November* 15, 1916.)

The Medical History of the Exiled Stuarts.

By James Rae, M.D.

During the last fourteen years there have been published a large number of the Stuart Papers at Windsor Castle, and the latest volume to be issued contains an account of the death of James II's consort from the pen of her confessor, Father Gaillard. Interested by this the present writer made a search in the five preceding volumes to find other allusions to illnesses in the family of that king, and by drawing on other published sources he has procured additional details. It is the purpose of this paper to give an account of the illnesses and deaths of James II's wife, of their two children who reached adolescence, and of their grandchildren.

In the calendar of Stuart Papers, amongst countless references

to schemes of intrigue, applications for relief, poverty, requests for places in convents and religious fanaticism, we get occasional glimpses of more personal matters, and a few of these show us the figure of Mary of Modena struggling for her son's restoration under a burden of ill-health and allow a more pleasing idea of her than that of the vindictive harpy depicted by Macaulay.

Mary Beatrice of Modena was married to James, the second son of Charles I, in September, 1673. To him she bore seven children, five of whom died young. The surviving daughter, Princess Louisa Mary, died at the age of 19, leaving only one brother alive, James Francis Edward, born June, 1688, who was taken, six months later, to France by Queen Mary.

To Mary's health the first reference is in 1690, in the spring of which year she had "a great cold." No other mention of her health is found until October, 1699, when "it is completely re-established," showing that she had again been ill. Though she had a serious illness of some sort in 1702, the letters are again silent on the matter for thirteen years, save for "a trifling indisposition" in January, 1707, but in October, 1712, she is described as being well. On the 24th of the month following, the Duke of Berwick, the illegitimate son of James II by Lady Arabella Churchill, found her "without feavour but still a great cold." In February next year she was again in perfect health, but five months later is said to be "extreamly fallen away." The blame for this is laid on the air of Chaillot, and her physician, Dr. Garvan, hopes that she will remove from the cold cloisters. Not until October 17 is there any news of her removal, and then it is only a hope that she will return to St. Germains, "The Queen is weak but without feaver." A week later she was "still out of order" but had removed at the end of the month, and her good health is again reported in the middle of December.

But a more serious illness was to occur the year after. On February 4, 1714, Berwick again writes, "The Queen has had a little shivering this afternoon; her pulse is quick, but Dr. Garvan hopes that a breathing sweat will carry it off." Two days later she is much better, and on the 13th "in a good way of recovery though not quite out of all danger." The Duke writes once more on the 15th to say that she "is in a very good way; the pulse was pretty well, though not free from fever, which the doctors do not expect will leave her yet some days. Bar accident we may reckon her almost out of all danger." After two days the bulletin is "in a mending way, though it will

come on slowly. The physicians find her pulse almost quite right, she looks much better and her voice is considerably stronger." On the 19th, "she is out of all danger and in as good a condition, even in a better, than one could have hoped for in so little a time." The next bulletin, that she is pretty well, is followed on February 24 by the news that "the Queen is in a cheering way. I hope that within eight days she may begin to sit up. She sleeps and eats well." This seems to show that the illness was more serious than her attendants wished the Pretender to realize. Convalescence was slow, but on March 4, she "continues daily better and better" but not until April 4, do we find a letter written by the Queen herself from St. Germains. But evidently her attachment to Chaillot was too strong, for on June 19, she writes from there once more, "I cannot say I am sick, but I cannot say neither that I am quite well." Three weeks later she writes that "the French doctors are of opinion that I should go to Plombières." Whether she yielded the letters do not indicate, but it was from that place that the Pretender issued his protest in August, at the accession of George I. In the closing days of 1714 the Queen was "very well."

Four months later the Duke of Mar wrote to James that the doctor was sending "an account of the Queen's sickness. I hope that the quinquina will carry it off." On May 1 she is "quite rid of her fever, she has a little spice of the gout which will do her good," and on May 21, she is said to be "perfectly well recovered." Only five days later the Queen herself wrote, "I thank God I have no other ailement then the gout. I had a great deal of peine last night, but after I was a bed I grew better and I slept well. I am pretthy well today, but not able to sett my foot to the ground." Next day she wrote again (from Chaillot) "though the peine in my foot is very muech abated, yet it is very red and very muech swelled, so that I don't think I can putt it to the ground, nor will I try if I can, till tomorrow." On the 31st, the pain compelled her to "lye in bed this whol day," but two days afterwards the foot was "almost quitt well." In July she was well, but on October 1 she wrote, "My physick did well with me yesterday notwithstanding all the visits whicch left me no quiett, but the Doctor won't lett me wris (rise) till this evening tho' I am well enough."

The next mention of her health is on February 13, 1716, when she again had "some touches of the gout." In July she was perfectly well, but two months later the "great cold" returned for a few days. In the middle of November the Queen had "a great cold and in bed all day," and three weeks later was again "in bed all day for a small indisposition" which lasted three days.

No further reference occurs until September 5, 1717, when we learn "She hath been much out of order of late by colics and a looseness that hath mightily weakened her; she is now much better but hath not yet been in a condition to return the visit the Regent made her three or four months ago." Four days later she writes "I was far from well all day yesterday, but I have had a good night by the help of treacle and lodonom drops, and am well enough today." Apart from quinquina this is the sole indication of the treatment administered. On October 9, the Duke of Mar "never saw her looking better," but seven weeks afterwards she had "a great cold." On December 13, the report is that "she has kept her bed these four days of a great cold. She is better today but not yet well." More concern is shown in this passage from a letter of December 20, "Queen Mary herself has been and still is indisposed, though without any danger. She had a great pain in her hand before it swelled, but now she is much more easy. Her physician is not sorry that her ailments are turned to the gout." This illness left her enfeebled, for though well on January 10, 1718, she was observed as "far from able to drudge as she used." Two references in April describe her as well and in good health, but her confessor speaks differently in the letter written after her death:—

"For about a month the Queen had been attacked with a rheum which sometimes disappeared and occasionally returned, not being completely cured. The Queen was very well on Sunday, 1st May (1718). . . . She went to the parish church at five o'clock after dinner and knelt for half an hour; for the next half hour she sat back on her legs, not choosing to sit in an arm-chair which had been placed for her. Later she went on the terrace where she felt cold, and after half an hour, she went in, not feeling ill. Her appetite that evening was better than it had been for a long time. At her usual hour she retired, but had a bad night until five o'clock on Monday morning when she was attacked with a feverish shivering which lasted an hour and was followed by a heat. This persisted until she died; and it appeared worse from the accessions which came on—a mild one in the morning, the other at four o'clock, which was accompanied by a violent cough which continued until her death. Her Majesty asked for M. Dodart, now chief physician to the King (of France) and M. Boudin. They came each day from Tuesday to Thursday. At first they detected nothing dangerous in this fever; they ordered a second bleeding, the first having been done on Monday by order of Mr. Garven. The fits of fever and the cough worried them but they did not seem greatly alarmed. Wednesday and Thursday passed in the same fashion, and even the night Thursday-Friday was so good that hope re-awakened and a draught was ordered for the following day—Saturday. . . . On Thursday the Queen wished to confess. The physicians asked me to dissuade her lest the preparations should disturb the improvement which

had appeared, and I asked the Queen to postpone the confession. On Friday she spoke uneasily of dying without it. What decided me on agreeing to it was a violent cough and shivering which attacked her about five o'clock on Friday just after the physicians had left in the expectation of finding her better the next day, saying, however, that they found the fever to be very marked. . . . About an hour after the confession the Queen was becoming weaker and her disease stronger. On the advice of Mr. Garven I informed her that she should receive the last sacrament, to which words she listened with no more emotion than if one had spoken to her of going to mass when she was well. This was concluded at half-past eleven on Friday, and from then until half-past seven on Saturday, the Queen had no thoughts save of God. At two o'clock she asked for the prayers for the dying. Half an hour before she died she asked for absolution. . . . All this time her body was being destroyed by the oppression on her chest where all the disease had settled, and by the inflammation this caused. She could speak until a quarter of an hour before the end came, and even in that time she gave me signs that she heard my words. Her spirit left her at half past seven on Saturday morning, 7th May, 1718. . . . I had forgotten that at four o'clock in the morning she requested that mass should be said in her room in which she joined. . . . They will have informed you of the opening of her body, by which it appeared that she could not have lived much longer from the decay of the lungs and the great abscess in her side left by her last illness, which she often felt."

A summary of these different references shows that Mary had four attacks of gout, that five times she had a great cold, more emphasis being given to this by the fact that twice she appears thankful that gout is her only disease. These colds occurred especially in the winter months. In one year (1713) the only two references found deal with her weakness, and early in the next she had an illness with fever lasting over two months, followed by a slow convalescence, and leaving effects which were discovered after death. Later she suffered from abdominal trouble for three months, and was afterwards noted as being weak. Finally, she had an illness with fever, violent cough and oppression on her chest, leading to death on the sixth day. The lungs were found to be decayed, and an abscess present in the side.

It seems fair to sum up her case as that of a middle-aged woman who had at various times respiratory trouble dating back twenty years, most marked in the winter months, which left her debilitated. She had a serious illness four years before death, and three years afterwards intestinal trouble that persisted for months. Shortly before she died she was unable to give her usual amount of attention to her affairs, and was finally carried off by an acute respiratory disease in six days. After death her lungs were found to be decayed and the pleural cavity to contain an

abscess. A reasonable diagnosis is that of chronic bronchitis ended by an acute pneumonia. The earlier prolonged illness may have been acute pneumonia with empyema. On the other hand, the repeated attacks of severe cold *may* have been due to fibroid phthisis, but the history is too prolonged to make this even as probable as the other conjecture. If the intestinal trouble had been tuberculous, probably she would not have survived; the causative organism is more likely to have been Friedländer's bacillus.

The daughter mentioned above, the Princess Louisa Mary, was born on June 28, 1692, and grew into a remarkably winsome and beautiful girl. In 1708 she had measles. In the early spring of 1712 James took small-pox. On April 10 of that year, while at her toilet, she noticed a few spots on her chest, and on Monday, April 18, at 9 in the morning, she died of that disease which had already been fatal to her father's brother Henry, Duke of Gloucester, in 1660; to his sister Mary, wife of the Prince of Orange, and to her step-sister, Mary II of England. Had Lachesis but decreed otherwise, and taken the life of James instead, Princess Louisa Mary would have had a great likelihood of succeeding to the throne of England at the death of Anne. Her body was embalmed and lay unburied for eighty years beside that of her father.

The Calendar has no reference to the childish disorders of James Francis Edward, and the pompous and precise "order" for his training at the age of 8, issued by James II, contains only a brief prohibition of food being given by his associates. This appears to have been advisable from the trials he had had to undergo in his infancy in the way of diet. He was a weakly child, and his life was often despaired of. In 1704 he had an acute respiratory disease, accompanied by spitting of blood, which was regarded as consumption; but early in 1705 he made a sudden recovery. In 1708 he caught measles from his sister, and was again seriously ill. Later in that year, while in Flanders, he had a violent fever which clung to him for months, and was only partly shaken off; a similar attack followed in 1709. In March, 1712, he had his attack of small-pox, this being the first of his illnesses referred to in the Calendar. In the following year he was greatly troubled by colds, to one of which he alludes in a letter of March 10, 1713. On June 18, 1715, James himself writes: "I find myself much the better for the physick I have taken, and my bleeding yesterday. I am to take another doze tomorrow, which I hope will be the last." This was probably an attack of ague.

Fifteen months further on we get the first hint of a trouble which

was to cause great anxiety to his adherents. In the campaign in Flanders, which he joined for experience, and during which he caused much embarrassment to Queen Mary and her faithful treasurer, Dicconson by his expenditure, he had suffered from piles, and on September 25, 1716, he began to be uneasy with the same trouble. Some remedies which he used gave no relief, and he was unable to do any writing. On October 6 is the first mention of sending a surgeon to Avignon to treat him, and there was the usual abundance of suggestions. The choice fell on Guérin, who started on October 13, knowing only that he was to attend a person of rank. On October 21 the Duke of Mar wrote to the Queen: "His Majesty was this morning cut of a *fistula in ano*. He endured the operation with all the resolution in the world. I was by and was grieved with the thing but charmed with the spirit he showed. The operation lasted about five minutes and was very well performed by Guérin, a chirurgeon we had sent for from Paris. He had ten cuts of the knife and scissors but the wound is not deep. He is as calm and tranquil as possibly can be expected without the least appearance of any fever."

By November we find several references to all danger being over, and recovery being quick, but on the 11th "It will be a long time before he is in a travelling condition. He is still in bed on a thin diet and having pain." A fortnight later he had perfectly recovered of his wound, but was still weak. He was able to leave his room on the 26th, and on December 7 was dressed in his ordinary clothes for the first time since the operation. Another week passed before he wrote any letters with his own hand. In the beginning of January, 1717, "his health comes on apace, but he can scarce be able to travel at least for a month," and six weeks later he was "keeping his health very well."

On March 1, however, a new note comes into his letters. Writing to the Duke of Mar on that date, he says: "I keep my health well enough, but am now but too sure of having the illness that you used to laugh at me for thinking I had, so that I must lose no time in taking proper remedies, if I have time for it, though by my looks nobody would suspect me and I am not sick." A fortnight later he prophesies that his illness will be very troublesome, and last a long time. On the same day Mar replied to his former letter in a way which throws a good deal of light on the nature of the illness. "I cannot believe anything of that illness you fancy you have, and I know all that makes you think so so well by myself and my ails that I cannot but be of that opinion. All proceeds from a weakness in your stomach and fault of

digestion; that effects (*sic*) the blood and that the nerves, and so reciprocally. Remedies to sweeten the blood ought to be taken, but at the same time those ought to regard the stomach which is the cause of all, and if set further wrong turns dangerous. I would not pretend to so much of the physician if two very able ones I used had not always given me this advice, and though they were great users of mercurial medicines, yet they were sparing of them to me on account of my stomach. . . . Forgive my saying so much, but my ills are so like yours that I thought myself compelled to tell you so." The illness worried James, and a couple of days afterwards he wrote again about being unable to stir for a long time while he underwent treatment. He concluded his letter with "There is no danger in the illness but 'tis very disagreeable and not to trouble the Queen to no purpose I would not tell her just how the matter stands." Three weeks later James wrote: "I am I own a little afraid of the remedies I am going to take. . . You would do more good to the hypo- than all the antimony that Ditty is preparing for me."

Here, then, we have the case of a young man who is attacked by a disease which has been the subject of chaff between him and an older man of questionable character. The illness is to be kept secret, especially from the patient's mother; it will require prolonged treatment (as we shall see it prevented James from travelling for over six weeks), and it was to be treated by antimony; it was some time before he felt sure he had it. The facts point with some decision to venereal disease. The word "hypo-" may stand for hypogastrium, but there is nothing in these letters to suggest that James was acquainted with any of the more technical medical terms, unless, as may be, he had learned this one from his apothecary, Ditty.

He began his remedies the same day, and one of his companions wrote later that they were easier than had been anticipated, and could be abandoned at any time, but it would be a week after that before James could travel. Something had been told the Queen, for on April 8 she wrote: "The King writt a few lines to me from Pesaro of the 20 wher he was arrived that night and well enough tho' he was going soon to beggin the remedys he intended, of whicch I own I am afraid as well as you are, and I am glad you have given a touche of it to him, but I fear it will com to late;" and again on April 20: "He has begun his remedys . . . I own to you that I am very uneasy when I think of these remedys for I cannot think them proper for him." The uneasiness shown in various other letters about James undergoing treatment

is concerned with the intrigues for a Swedish invasion of England which the Jacobites were trying to instigate at this time. For the next month the letters contain little that is new, but from that of May 15 we find James was so far recovered on that date that he intended to start on a journey, and did in fact leave for Rome on May 22. On May 7 he had taken a vomit, which left him weak for two days. On July 22 he wrote to Mar from Urbino: "I have begun to ride again, for my little ailment is quite over, and I never found my health and stomach better than since my remedies." He does not himself mention that a fortnight previously "he had been taken ill with a *débordement de bile*, but he took physic which eased him," and he was able to continue his journey towards Urbino.

For the remainder of that year he was in good health, and the fact is commented on thankfully by his adherents. On February 17, 1718, Mar wrote: "The King himself is a little out of order today in his stomach, but I hope it will be nothing." This indisposition had passed off three days later, and Mar explained that he himself and others had been upset in the same way, he thought from the new wine they had been compelled to drink. There was general relief to hear that the ship bearing the King's wine had put in at Leghorn. On April 17 James again complained of stomach trouble, and on May 2 took medicine because he was "often troubled with bile." In April, too, he began to be out of order from tertian fever; on May 19 he had a marked recurrence. Five days afterwards a fuller account was sent, attributing the ague to the sudden change from excessive heat to very cold, which had affected some of his suite as well, and hoping a few doses of the bark would carry it off. The fever left him on June 12, but the great heat hindered his recovery, and at the end of the month he was still very weak.

This reference to James's health is the last calendared, and his later history has to be obtained from other sources. Before turning to these we may note that the Calendar contains an important letter written but a few days after the one just quoted. This is a letter of instruction to the envoy appointed to negotiate a marriage with the Princess Clementina Maria, youngest daughter of Prince James Sobieski, and the petty duplicity his representative is ordered to practise shows that James was true to the Stuart stock. The Princess was born in Italy in 1702, though at this time the family were in Poland. Owing to diplomatic interference the unfortunate marriage did not take place until September 1, 1719, and on December 31, 1720, Prince Charles

Edward Philip Louis Casimir was born; the other child, Prince Henry, being born in March, 1725. Eight months later, Princess Clementina left her husband on account of his conduct and spent two years in a convent. The reconciliation was but nominal, and her unhappiness was ended by her death in 1735, of ill-health brought on by her asceticism; she is vaguely stated to have had "lung trouble."

In the medical history of the Pretender there is a gap from 1718 to 1742, when he had an illness which was probably ague, a disease which frequently troubled him. In 1752 his health was wretched, chiefly from indigestion. In 1754 he was confined to his room for some weeks by a pain in his leg, thought to be sciatica, but later found to be due to hernia. In the same year he had a slight fit of apoplexy and was troubled by dropsy. By 1756 his digestion had failed so completely that he became emaciated, and so dyspeptic that the Pope granted him a dispensation to take broth or chocolate instead of communicating fasting. In August, 1758, he was seized with convulsions and severe pains in the stomach, accompanied by indigestion, which were held to be senile consumption. On March 29, 1760, came another attack of convulsions, with a relapse on April 16. All hope was given up, and preparations were made for his funeral. About the middle of May he began to improve, and at the end of the month was able to get up and have his bed made for the first time in forty-seven days. It was not until June that the funeral preparations were abandoned. Just before Christmas he had another fit, and from that time took no interest in political or household affairs. Throughout the next year (1761) he seemed to be just alive, but Sir Horace Mann, the English Ambassador, reported wrongly that he was imbecile. In December, 1761, he again had convulsions, with a fresh attack on October 13, 1762, after which his mouth was distorted and his speech hardly intelligible; in this attack he remained long unconscious. In November, 1763, his condition grew worse; in September, 1764, came another stroke, and during the next twelve months his attendants marvelled that he should still live. In November, 1765, he had an attack of quartan ague, which subsided on December 10. A relapse followed two days later, in which he was bled, and treated with bark. The paroxysms of pain in the stomach returned. On Wednesday, January 1, 1766, he was able to have a good dinner, but afterwards complained of oppression in the chest, and died shortly after nine o'clock that night. "So ended the troubles of poor Mr. James Misfortunate."

His son, Charles Edward, appeared to change his character after

quitting Scotland in 1746. It is generally agreed that his fondness for alcohol dates from the months of his concealment by the Glenmorriston men. His drunken habits were notorious even in Scotland in 1747, and the weak protests of his father were fruitless; the two never met after Charles set out for Scotland.

After his expulsion from France in 1748, in accordance with the treaty of Aix-la-Chapelle, he led an obscure life, accompanied by his mistress, Clementina Walkinshaw, daughter of the man who helped Charles Edward's mother to escape from Innspruck. By her he had a daughter in October, 1753. They lived together intermittently, and he so ill-treated her that at last she abandoned him for a convent at Meaux, on July 22, 1760.

His sottishness increased, and in 1770 he is described as being bloated and red-faced; he went to the baths at Pisa for a few weeks on medical advice. He had begun to be dropsical and to lose his appetite. In 1772 he married, and for two years his habits improved, but he returned to his drunken violence after that period. In 1776 Mann reported that: "His legs are much swelled, and one is commonly open, the discharge from which is supposed to be necessary for his existence. Whenever that ceases he is ill indeed." He suffered from colic and constant sickness, so that he often had to leave his box at the opera and go into the public passage to relieve himself, but there is no word of hæmatemesis. In 1779 Mann was told that he frequently had epileptic fits which his physicians said would one day carry him off. Later on dropsy and a severe cough made their appearance. In March, 1783, he had a fever, with shortness of breath and dropsy, and was thought to be dying, as mortification set in in his legs. There was a sudden change for the better, and in the summer of that year he travelled through Tuscany. A few months later he began to drink heavily once more. On January 24, 1784, Mann wrote: "He is very ill—it is not apoplexy but an inflammation of the brain; he has been speechless and insensible for two days." Consciousness then returned, but speech not until next day, and then for only a few hours. On the 30th he was again thought to be expiring, but on the 31st speech and senses returned, and he was out of danger on February 14. The swelling of his legs was so great that he had to be carried from room to room. On December 18 Mann's note is: "He decays every day visibly. The disorder in his legs increases. The faculties of his mind are as weak as his body." In 1785 his physicians were unwilling that he should travel. During the next spring the shortness of breath, dropsy and sickness returned, and in

March (1786) he had a fit so prolonged that it seemed apoplectic. In 1787 he was almost helpless, and lay for hours half-dazed on a sofa, reviving only when he was reminded of Scotland. On January 7, 1788, he had an apoplectic seizure, followed by a right hemiplegia, and remained semi-comatose; the fits were repeated, and on January 30 another caused his death between nine and ten in the morning.

His daughter was summoned by him in autumn, 1784, legitimated, and given the title of Duchess of Albany. She survived him less than two years. It is stated that she fell from her horse in 1789, was operated on in Bologna for an abscess due to this, and died in November of that year. Another story is that she died of inflammation caused by operation for an internal tumour.

On April 17, 1772, Charles Edward married Princess Mary Louisa of Stolberg-Gadern, his bride being thirty-two years his junior, and just a year older than his illegitimate daughter. His drunkenness, and an attempt to strangle her made her leave him on November 30, 1780. She seems to have had no serious illness until 1823, though dropsy had been coming on for a few years. In 1824 she had shortness of breath and intense thirst, but would not allow that she was ill. No other symptoms are mentioned; she died in the morning of January 29, 1824.

The other child of the Pretender was Henry, who became Cardinal at the age of 25. He led an easy placid life until 1794, when he lost two rich livings, and in 1798 his generosity made him almost destitute. He was granted a pension by George III, had a fever in 1803, and died of malaria in 1807.

The direct line of the Stuarts is represented by the descendants of Anna Maria, second daughter of Henrietta Anne, daughter of Charles I, who in 1684 was married to Victor Amadeus II, Duke of Savoy, by whom she had two daughters.

BIBLIOGRAPHY.

"Calendar of Stuart Papers," vols. I-VI, Historical MSS. Commission, 1902-16.
DORAN. "Mann and Manners at Florence," 1856.
EWALD. "Charles Edward Stuart," 1875.
HENDERSON. "Personal History of the Stuarts," 1908.
SHIELDS AND LANG. "King over the Water," 1907.
STRICKLAND. "Stuart Princesses," 1872.
VITELLESCHI. "Court in Exile," 1903.

Further information might be obtained from the following had they been available.

CAMPANA DE CAVELLI. "Les Derniers Stuarts,"
"Diary of Henry, Cardinal York," 1875. (This is said to give a detailed account of the last days of the Pretender, but 75 copies only were printed.—Chiswick Press.)
STANHOPE. "Decline of the last Stuarts." Roxburghe Club.

Section of the History of Medicine.

President—Dr. Raymond Crawfurd.

(*May* 24, 1916.)

The Figures of the Bristol Guy de Chauliac MS. (*circa* 1430).

By Charles Singer, M.D.

The Guy de Chauliac MS., of which the miniatures and diagrams are here reproduced, lies at the Bristol City Reference Library (founded 1614). The volume has been sufficiently described as regards its general features by Mathews and Nixon. It was the work of Nixon that drew the attention of the present writer to the MS.

The Bristol MS. is written on paper bearing the earliest Bordeaux water-mark. It measures 28·5 cm. high by 21 cm. across, and contains 288 folios in double columns of forty-five lines each. It is made up in quires of sixteen leaves, of which folios 6-15 are blank except for a few MS. notes on folio 8 recto and on folio 15 recto and verso.[1] The contents are as follows:—

(1) Practica astrolabii (folio 1 verso).

(2) In dei nomine Incipit Inventarium (folio 16 recto), followed by the Greater Surgery of Guy de Chauliac containing the miniatures and diagrams here reproduced.

(3) A treatise on astrology, perhaps also the work of Guy de Chauliac. Incipit: "Circa istam materiam est intelligendam pro natura dierum criticorum" (folio 272).

[1] Cp. Norris Mathews, "Early Printed Books and Manuscripts in the City Reference Library, Bristol," Bristol, 1899; and J. A. Nixon, "Guy de Chauliac, a new MS. including the 'Practica Astrolabii,'" Janus XII, 1907, and "A new Guy de Chauliac MS.," in the Seventeenth International Congress of Medicine, Section XXIII, History of Medicine, London, 1914, p. 419.

(4) The Centiloquium of Ptolemy. "Incipit. via seu sentencia tholomei in suo centilogio" (folio 280 recto).

(5) A Latin English Glossary (folio 282 recto).

Items (2) and (3) are in the same handwriting and continuous. It is hoped to publish (3) shortly. We are here concerned only with (2).

This surgical MS. has the following incipit: In dei nomine Incipit Inuentarium seu collectarium in parte cirurgicali medicine compilatum et completum. Anno Domini Millesimo Tricentesimo sexagesimo tercio. Per Guidonem de Caulhiaco cirurgicum magistrum in medicina in preclaro studio montispessulani. Quodquid inuentarium seu collectarium fecit scribi et taliter ordinari, venerabilis vir Magister Johannes Tourtier Magister in Cirurgia. Ad requestam altissimi excellentissimique et potentis principis Domini Johanni ducis de bedford Regentis regnum francie et protectoris Regni anglie.

The explicit is as follows: Explicit liber per optimus de cirurgiae ditus a Guidone de Caulhiaco magistro in medicina et cirurgia.

The MS. was therefore written by Jean Tourtier, master in surgery, for presentation to John, Duke of Bedford. It can be dated between 1420, when the Duke of Bedford became regent of France and 1435 when he died. The writing is loose and somewhat untidy. The minuscules tend to pass into a semicurrent script, and the general result is unpleasing. There are a number of badly illuminated capitals. The work is nevertheless a complete and fairly accurate copy of Guy de Chauliac's composition.

Eight miniatures, all of which we here reproduce, are scattered through the volume. These miniatures are pen drawings, brownish-black in colour and artistically somewhat inferior. Bleeding spots or areas have been coloured red. In addition to the miniatures there are a few rude drawings of instruments, the nature of which we discuss below.

(I) The Miniatures.

The first miniature (folio 16 recto, fig. 1). The left half is occupied by a figure in hood and gown presenting a scroll to a crowned figure. Behind are three other figures as well as a dog and a castle. It is probable that the crowned figure represents the Duke of Bedford. It should be noted that one of the figures behind wears the same head-dress as that of the surgeon in the remaining miniatures.

In the right half of the picture a robed figure is seated on a chair and occupied in study. The robed figure which thus appears in both

scenes is presumably intended to represent Guy de Chauliac himself or Jean Tourtier the scribe. Guy de Chauliac, it will be remembered, was a cleric.

The second miniature (folio 25 recto, fig. 2) is prefixed to the first treatise, which is concerned with anatomy. It shows us an anatomical demonstration. The professor is seated and indicates to his pupil by means of a pointer the important features in the skeleton placed across the field. The head-dress of the surgeon recurs in the



FIG. 1.

Headpiece of the Bristol MS.

other miniatures. The anatomical section of Guy's work was regarded as of great importance, and several MSS. are known which contain this treatise without the remaining part of the "Surgery." Some of these are in English. A number of the Guy de Chauliac MSS. are similarly illustrated with dissection scenes.

The third miniature (folio 41 verso, fig. 3) is prefixed to the second treatise, which deals with apostumes, issues and pustules. Here the surgeon, with the same head-dress as before, is kneeling down to open

with his knife an apostume in the right groin of a patient. The sufferer sits on a bench, with his clothing partially removed, and displaying also an axillary abscess.

In this and the following miniatures appears the surgeon's box, a chest divided into many compartments which formed part of the usual field equipment of the surgeon. Such a chest is figured, for instance, by William Clowes as late as 1583, and was doubtless carried by him on to the battlefield. It contained the materials for emergency surgery.



FIG. 2.

An anatomical demonstration.

The fourth miniature (folio 83 recto, fig. 4) is prefixed to the third treatise, which is devoted to the treatment of wounds. The patient is here nude except for a pair of drawers. He is suffering from a very large number of small wounds which the surgeon is stitching with a formidable needle. The wounds are shown red in the original. Behind the surgeon a number of instruments are spread out on a stool; in front of him stands a vessel containing a large bandage.



FIG. 3.
Opening an inguinal bubo.



FIG. 4.
Garsing.

It is possible that this miniature is capable of another interpretation and that the surgeon is not dressing but inflicting the wounds. He is perhaps engaged in "garsing." This process of inflicting numerous shallow wounds was held to have valuable remedial effects.

The fifth miniature (folio 123 verso, fig. 5) is prefixed to the fourth treatise, dealing with the nature and treatment of ulcers. This miniature is very nearly a reproduction of the third (fig. 3). A patient suffering from numerous wounds is being dressed by the surgeon.



Fig. 5.

Operating on thigh.

Some of the wounds have apparently been sutured and the surgeon is engaged in extending an incision in the groin. A swelling is shown in the right axilla.

The sixth miniature (folio 149 recto, fig. 6) is prefixed to the fifth treatise, devoted to fractures and dislocations. Here the surgeon is shown bandaging the fractured leg of a patient seated and supported by a crutch. The injured limb rests on a stool and exhibits a splint made up of separate slips of wood which have been glued on to a piece

of cloth. This type of splint, similar to the modern "Gooch," was in common use by mediaeval surgeons. The operator is fixing the splint in place by means of a bandage. By the surgeon's right elbow may be seen a pestle and mortar. The instrument placed on a stool in the region of his left shoulder we are unable to identify.

The seventh miniature (folio 161 recto, fig. 7) is prefixed to the sixth treatise. The surgeon is washing the crippled limb of a patient, who sits with his crutch under his arm. The knee- and ankle-joints are



FIG. 6.

Putting a fractured limb into splints.

enlarged. Above hangs a flagon. The object grasped by the patient on his left hand is the wooden pillow that was in use as a head-rest in the Middle Ages. The patient is perhaps suffering from rheumatoid arthritis.

The eighth miniature (folio 230 recto, fig. 8) is prefixed to the seventh and last treatise, the antidotary. This treatise opens with a chapter on phlebotomy, and we are here shown a blood-letting scene. The patient sits grasping the barber's staff. A venesection has



FIG. 7.

Treating a case of osteoarthritis.



FIG. 8.

Phlebotomy.

been made at the bend of the elbow and the blood spurts into the basin held in the patient's left hand. It was usual to make the patient hold the bowl himself as he thus provided a useful danger signal. When the bowl dropped, the blood-letter knew that his patient had fainted and that it was time to stop. In this scene the phlebotomist, intent upon his work, is compressing the veins with his hands both above and below the incision. In the lower left-hand corner of the picture stands another bowl, and a couple of lancets lie upon the stool.

(II) The Diagrams of Surgical Instruments.

Our knowledge of mediaeval surgical instruments is still in a very partial and fragmentary state, despite the learned labours of Channing, Leclerc, Gurlt, Power and others. Of the instruments themselves very few have come down to us, and they are much rarer than surgical appliances dating from Greek and Roman times. We are thus thrown back almost entirely on contemporary figures for our knowledge of the instruments used in mediaeval surgical procedure. Now, in the study of these figures certain special difficulties are encountered:—

(1) With the exception of a few Byzantine MSS. no early figures of surgical instruments have survived. All are later than the Arabian revival. No Western MS. containing diagrams of surgical instruments has, we believe, yet come to light of a date anterior to the thirteenth century.

(2) The earliest figures of surgical instruments are derived or copied from Arabian originals. The great source of the mediaeval surgical armamentarium was the Arabic work of Albucasis, translated into Latin in the twelfth century by Gerard of Cremona (died 1187). Figures are rare in Arabic MSS., and when they occur are almost always of exceedingly bad workmanship. The illustrated MSS. of Albucasis are no exception to this rule. The Moslem artist, forbidden to represent the human or animal form and ignorant of the rules of perspective, produced the most wretched and frequently incomprehensible diagrams.

(3) Nor are the figures of the Latin translations of Albucasis nor those of the earlier original Latin surgical MSS. a great improvement on their Arabic predecessors. The illustration of these works was not for the most part undertaken by the best artists. Moreover, the figures were often copied from MS. to MS. by scribes quite ignorant of the material with which they were dealing. Further, even the simple rules

of perspective were not generally known until the second half of the fifteenth century. Lastly, the character of the instruments figured in these MSS. is often obscured by meaningless ornamentation.

In identifying early surgical instruments of the mediaeval period we have to rely largely on the interpretation placed on them by the later Renaissance writers, chiefly of the sixteenth century, when the original Arabian tradition had become developed and modified. To obtain a more exact idea of the conditions under which surgery was practised in the Middle Ages proper, it is therefore worth while to examine closely the diagrams of instruments in the earlier MSS.

Of the large number of MSS. of Guy de Chauliac, a considerable proportion are illustrated by miniatures, but of these only three besides the Bristol MS. contain figures of instruments.[1] Of these three only one is as old as our MS., which was unknown to Nicaise when he prepared his monograph on Guy de Chauliac.[2]

In the Bristol MS. the drawings of instruments consist only of pen diagrams showing the outline without any attempt at shading or perspective. In interpreting them we have used, besides the text of the MS. itself, E. Nicaise's important French work "La grande chirurgie de Guy de Chauliac" (Paris, 1890), and Laurent Joubert's "Chirurgia magna Guidonis de Gauliaco" (Lyons, 1583). Our text varies somewhat from the Latin of Joubert. We have also consulted the following MSS. and printed books with especial reference to the diagrams of instruments that they contain:—

Albucasis (died 1106).—(*a*) Arabic MSS: Bodleian, Huntingdon 156, Marsh 54, Bodley 491. (*b*) Latin MSS: British Museum, Additional 36617 (thirteenth century); Bodleian, E Museo 19, (fourteenth century, before 1344). (*c*) Printed editions: Venice, 1497, Latin (reprinted 1500, Locatellus); Strassburg, 1532, Latin (Schott); Basle, 1541, Latin (Petrus); Oxford, 1778, Arabic and Latin (John Channing); Paris, 1861, French (Lucien Leclerc).

[1] The other three MSS. are at the Library of the Faculty of Medicine of Montpellier: MS. français 184, fourteenth century, Bibliothèque Nationale MS. latin 17846, written in 1472, and MS. latin 6910, written in the second half of the fifteenth century.

[2] I have to thank Miss Jane Willcocks for drawing my attention to the existence in Paris at the Bibliothèque Nationale, of an early printed French version of Guy de Chauliac with figures of instruments. This was published at Lyons in 1478 by Barthélemy Buyer. Reproductions of the figures are given by M. A. Claudin "Histoire de l'Imprimerie en France," iii, p. 11-18, Paris 1904. Many of these figures clearly resemble those of our MS., but in other cases there is considerable divergency.

William of Saliceto (1201-77).—MS. 76, St. John's College, Oxford. A fourteenth century MS. containing some very fine drawings of instruments. The incipit ascribes it to Guillelmus Parmensis but the text is identical with the printed versions of William of Saliceto.

Lanfranchi of Milan (died 1315).—"Cirurgia" in the collection of Gregorius de Gregoriis, printed Venice, 1513. (We have not seen the editio princeps of Lanfranc, Venice, 1490.)

Jan Yperman (1295-1351).—"De Cyrurgie van Meester Jan Yperman naar de Hanschriften van Brussel, Cambridge, Gent en Londen," by E. C. van Leersum, Leyden, 1912.

John Arderne (1306-90).—"Treatises on Fistula in Ano," edited by D'Arcy Power. Early English Text Society, London, 1910.

Hieronymus Brunschwig (*circa* 1450-1533).—"Das Buch der Wund-Artzney," Strassburg, 1497.

Hans von Gersdorff.—"Feldtbuch der Wundtartzney," Strassburg, 1517.

Ambroise Paré (1510-94).—"Oeuvres," Paris, 1575. "Oeuvres complètes," edited by J. F. Malgaigne, Paris, 1840.

Guido Guidi (died 1569).—"De cirurgia" in Vol. III of the collected Ars medicinalis, Venice, 1611.

E. Gurlt (1825-99).—"Geschichte der Chirurgie und ihrer Ausübung," Berlin, 1898, 3 vols. In the first volume is a masterly summary of the state of knowledge of mediaeval surgical instruments.

The diagrams in the Bristol MS. are scattered in five groups through the pages of the volume:—

The first group of diagrams (folios 113 verso to 114 recto; figs. 9, *a* to *j*,) is concerned with trepanning (Treatise III, Doctrine 2, Chapter 1). The description of the instruments is as follows:—

"Concerning the instruments necessary for the operation. There are six principal instruments and they should each be of three sizes, large, small and medium.

"*Firstly*, there are the *trepans*, which are for making holes so that the bone may be elevated. These are of divers forms. Galen used them in the form of *terebrae* (borers or augers)[1] with a little ridge surrounding the sharp extremity to prevent injury to the *dura mater* in the act of perforation (fig. 9, *a*). The surgeons of Paris in order to avoid a number of instruments adapted to the thickness of the bone in that part, use trepans perforated above the sharp point and provided

[1] Galen, "Methodus medendi," vi, p. 6.

with a peg (cavilla, cheville) so that by changing its socket they adapt the instrument to the thickness of the bone (fig. 9, *b*). The surgeons of Bologna use an instrument in the form of a lance, so shaped that when the point enters, the width of the remainder gives the surgeon the necessary control, so that it does not slip (fig. 9, *c*).

"*Secondly*, there are *separatoria* for the purpose of opening up the bone between the holes. There are two forms of separatoria, the straight or French form (fig. 9, *d*) and the curved or Bolognese form (fig. 9, *e*). The handle of the latter can be used as an elevator.

"*Thirdly*, there are *elevators* used for raising and separating (fig. 9, *f*).

"*Fourthly*, there are rugines to widen the fissures and they are like the *ruginæ sustanorum* as may here be seen (fig. 9, *g*).

"*Fifthly*, there are the *lenticularia*, and these are instruments much praised by Galen for safely smoothing and finishing the roughnesses by means of the ridge at their termination. The form is that of a penknife (scindipendium, trancheplume) with the *lenticula* at the end thus (fig. 9, *h*).

"*Sixthly*, there is the hammer for striking upon the lenticularia. It should be of lead, for this substance weighs heavy and sounds dull. This is its form (fig. 9, *j*)."

It will be noted that Guy de Chauliac performs the operation of trephining by means of separate borers or terebræ. The holes thus made were ultimately joined by the use of chisel and hammer or by other means. He does not use the trephine and in this respect follows the tradition of Albucasis. This avoidance of the circular trephine is the more remarkable since the instrument was well known to the ancients, including Hippocrates and Galen, and is still used by Arab surgeons[1] as well as by numerous savage races.

The nature of the terebra or borer used by Guy can hardly be made out from the figures of our MS. (fig. 9, *a* and *b*), which are however explained by the "tympanum rectum ad dilatandum os" of Joubert and of Gersdorff (fig. 10). These crude figures of the Bristol MS. represent a type of trepan that was screwed into the bone like a gimlet (fig. 10). The little circles at the end of the instrument are an attempt to represent the spiral worm of the screw. In the type used by the Parisian surgeons (fig. 9, *b*) there were a series of small holes represented

[1] M. W. Hilton-Simpson, "Some Arab and Shawia Remedies, and Notes on the Trepanning of the Skull in Algeria," *Journ. Anthrop. Inst.*, iii, Lond., 1893.

by dots in the figure, through one of which a transverse peg was thrust to prevent the instrument penetrating too deeply.

The remaining figures used for trephining are not all easy to identify. The hammer (fig. 9, *j*) is obvious, as are also the elevators (figs. 9, *e* and *f*) but the remaining two (figs. 9, *g* and *h*) present a

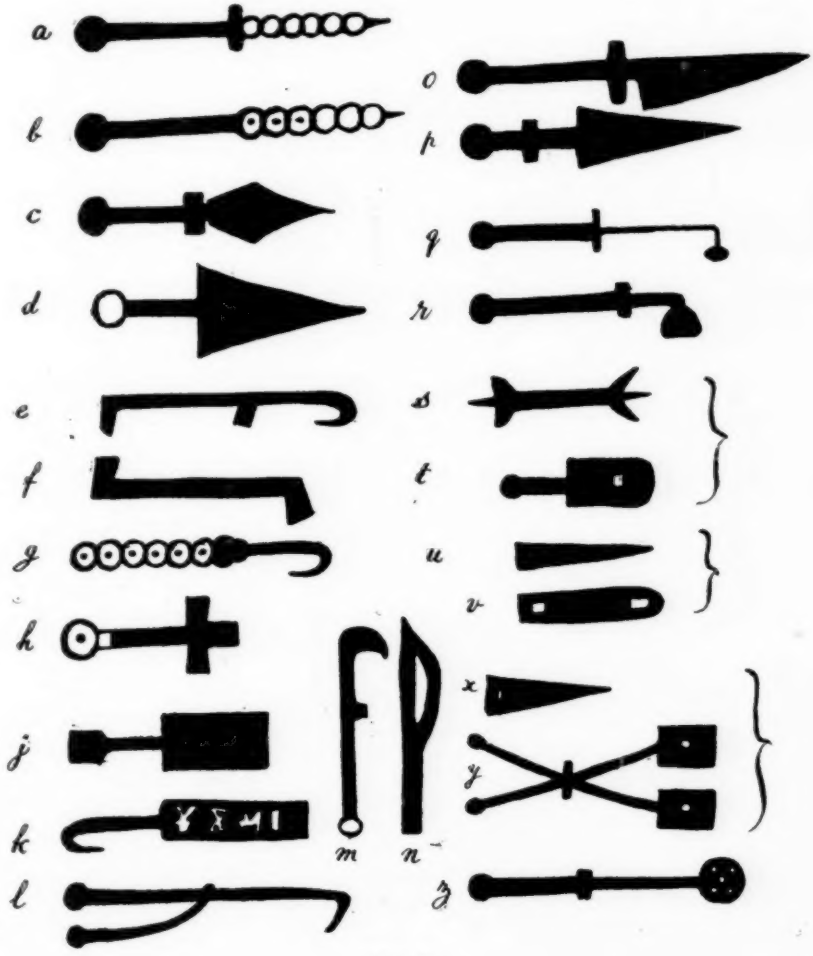


FIG. 9

Diagram of surgical instruments from the Bristol Guy de Chauliac MS. The explanations are given in the text.

difficulty. The description of Galen to which Guy refers helps us very little, nor do the figures of the accessory trephining instruments presented by Joubert (fig. 11) aid us much further.

The second group (folio 121 verso, fig. 9, *k*) contains only a single drawing which occurs in a chapter on abdominal wounds (Tractate III, Doctrine 2, Chapter 6). The instrument there figured is for the purpose of replacing the intestines in cases of penetrating wounds of the abdomen. The text states that this

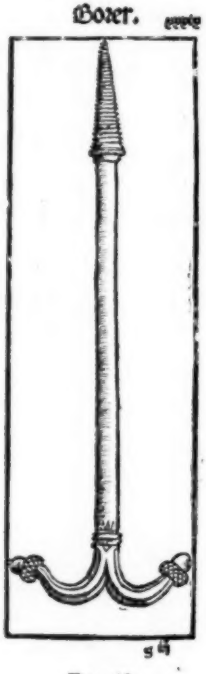


Fig. 10.

Trepan or borer, from Hans von Gersdorff.

weapon was used by Albucasis, but no such instrument is discoverable in the MSS. or printed texts of that author examined by us. These all give, under the heading *De vulneribus ventris et exitu intestinum*, an instrument in the form of a sharp hook, said to be used also in the treatment of hæmorrhoids (fig. 12, *a*). An instrument figured in a Flemish MS. of Yperman's Surgery, however (fig. 12, *b*)

suggests that we have here to do rather with a blunt hook carried in a transversely grooved handle.

The third group (fig. 9, *l*, folio 139 recto) also contains only a single figure and occurs in the course of a chapter on ulcers and polyps of the nose (Tractate IV, Doctrine 2, Chapter 2). The diagram is described as that of a nasal speculum, but no such instrument of this form is to be found among the books and MSS. that we have examined.

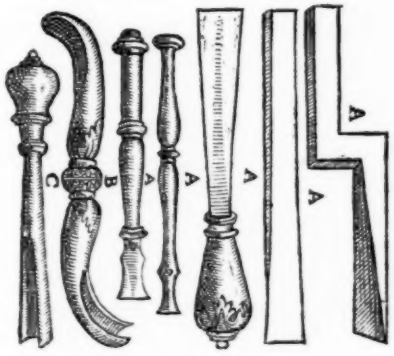


FIG. 11.

Cranial elevators, after Joubert.

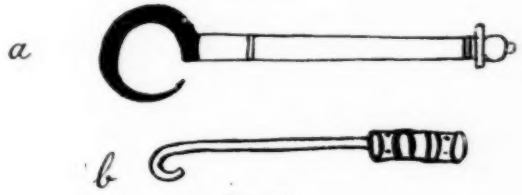


FIG. 12.

Forms of hook used by mediaeval surgeons. *a*, Form used in cases of abdominal wounds, from the "Cyrurgia Albucasis," printed Venice, 1500; *b*, from Yperman's Surgery, after van Leersum.

The fourth group (fig. 9, *m* and *n*, folio 14 verso) contains a couple of instruments used for operation on fistula in ano (Tractate IV, Doctrine 2, Chapter 7). The text describes a method of incising the fistula slightly variant from that of the editions of Nicaise and Joubert: "The mode of incision with the hook, which is an instrument of this

form (fig. 9, *m*) according to Albucasis. 'This is inserted with its cord and the intestine is compressed as much as possible with the cord that hangs out of it. After this the instrument called *bene scindens* is inserted, and all that is contained with the cord is incised thereby. Or according to my teacher there is passed along the track of the cord an instrument of this kind (fig. 9, *n*) curved and prolonged at one end and at the other with a heated cutting surface. All that is contained within the cord is thereby incised so that both cord and instrument fall away."

The description and instruments for fistula are not easy to identify with those of Albucasis. That writer (Chirurgia, II) describes the following method of incising a fistula: "Take a probe (tenta) with a perforated end of this form (fig. 13, *a*) and thread it with a five-fold cord. Insert it into the fistula with the cord wound round it so that it follows it. If now it emerges in or near the margin of the anus insert the finger therein and extract the cord. Then tie the two ends of the cord and leave for one or two days, and when the cord cuts into the flesh, tighten it well so that it cuts through the flesh and falls out and the wound heals up and solidifies and [the patient] is cured. But if the orifice of the fistula is blind then make a counter opening unless it is a deep one for then you must not do so for fear of injuring the muscle. Then cut it through as I have described [elsewhere]. And this is the form of the beaked spatula which is used for cleaving the fistula (fig. 13, *b*). Its concavity is very sharp but not the other parts so that they do not cut that which should not be cut."

The hook figured in the Bristol MS. is perhaps the same as the gorget or tendiculum that was adopted by John Arderne (fig. 13, *c*, *e*, *f*). This instrument, as Power has shown, was thrust into the fistula that had first been threaded with a cord. The cord, one end of which hung out of the rectum and the other end out of the fistula, was then tightened up by a screw or wrayste fitted into the gorget (fig. 13) and the compressed tissue cut through. 'Arderne adds to these instruments a curved director, the so-called "snowted needle" (fig. 13 *d*), which is passed along the gorget to engage with a metal guard placed in the rectum. A sharp knife, the *bene scindens* of our MS., is then passed along the "snowted" needle and performs the section. The Bristol MS. recommends that the *bene scindens* be heated so as to control hæmorrhage.

The fifth group (fig. 9, *o* to *z*, folio 245 verso and 246 recto) consists of a number of diagrams of cauteries embedded in a chapter on

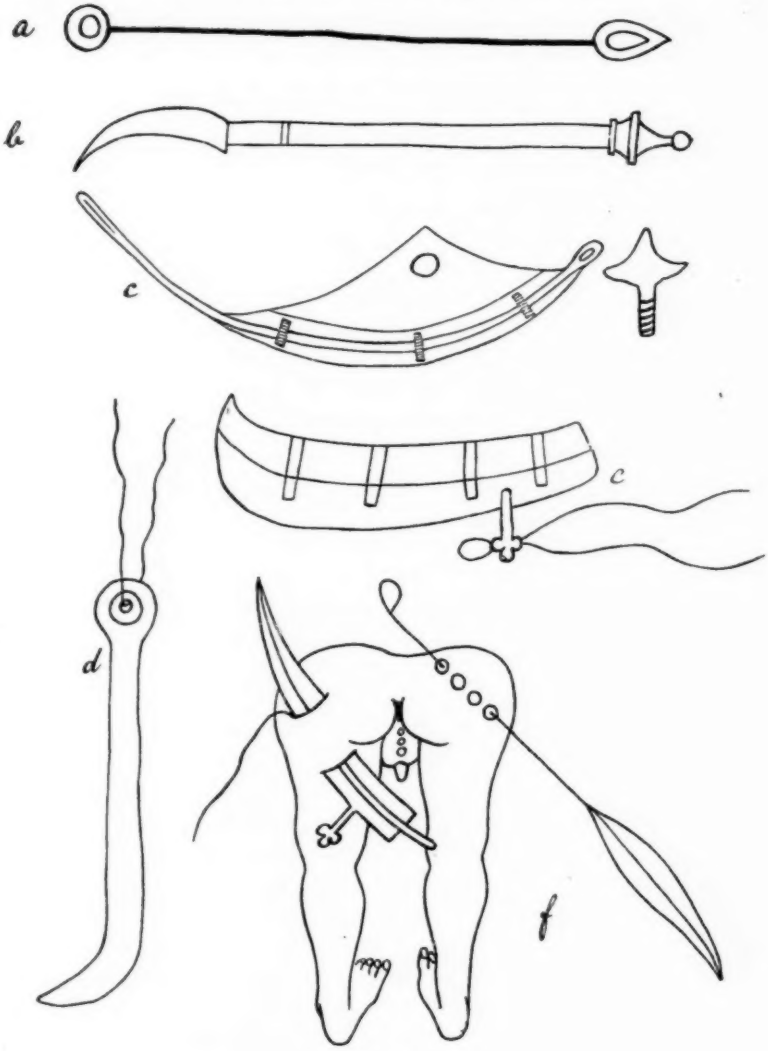


FIG. 13.

Mediaeval surgical instruments for operating upon fistula. *a*, A probe or bodkin with eye; *b*, a bistoury; *c*, the tendiculum or gorget, with the screw peg or wrayste by its side; *d*, the snowted needle; *e*, another form of tendiculum with the wrayste in place; *f*, the left buttock shows the tendiculum in place, with its wrayste; the right buttock shows the snowted needle about to be drawn through a series of fistulas. *a* and *b* are from the 1532 Strassburg edition of Albucasis; *c* and *d* are from MS., Sloane 2002 (15th century), after Power; *e* and *f* are from Sloane MS. 6 (15th century), after Power.

cauterization (Tractate VII, Doctrine I, Chapter 3). Cauterization was the Arabian operation *par excellence* and Guy, having informed us that a great variety of actual cauteries was used by the ancients, proceeds to assure us that the number had been greatly reduced by the moderns. Thus William of Saliceto used only six or eight, Lanfrank ten, Henri de Mondeville seven, and our author himself only six.

"The first form is knife shaped and used for cutting. There is also a two edged type. And with these, superfluous flesh is cut away and apostumes are opened and ulcers rectified. And their sword-like form is thus (fig. 9, *o* and *p*).

"The second instrument is the *olivary*. It is so-called not after the olive leaf as William [of Saliceto] and Lanfrank and Henry [de Mondeville] have supposed, but after the olivary bones [i.e., of the cranial vault] as Haly Abbas says in sermo IX pars 2 *De coquendis capitibus*, because that operation is performed on the upper part of the head as those masters themselves teach. The operation is performed over the sutures and nerves on account of pain there. . . . And this is its form (fig. 9, *q*).

"The third instrument is the *dactillary* and it is made like the dactillary bones [i.e., the phalanges]. . . It is oblong and coarser than the olivary and is specially useful for ulcers and corruption of boncs. And its form is thus (fig. 9, *r*).

"The fourth instrument is the *punctuale*. It has a slender rounded projection to cauterize the skin only, and it is double, being provided with a shield and a stop so that it does not penetrate the skin during cauterization (cp. fig. 9, *s* and *t*). And with this, cauterization is performed in the cubital and popliteal spaces. Another form is straight and rod-like, being provided with a canula so that it does no lateral damage (cp. fig. 9, *u* and *v*). With it deeper organs are cauterized, such as in *fistula lachrimalis* or polypus of the nose, or connected with the teeth. . .

"The fifth instrument is the *subtile* cautery used for the insertion of setons, and it is provided with broad perforated tenacula for performing the puncture. . . The form of the needle is thus (fig. 9 *x*), and its tenaculum thus (fig. 9, *y*).

"The sixth instrument is circular with five projections for the purpose of making five burnings. It has a plate with five perforations and it is used over the thigh in pain of that part, and over the shoulder for tenderness and swelling there, and its form is thus" (fig. 9, *z*).

The nature of the cauteries outlined in figs. 9, *o* to *t*, is sufficiently

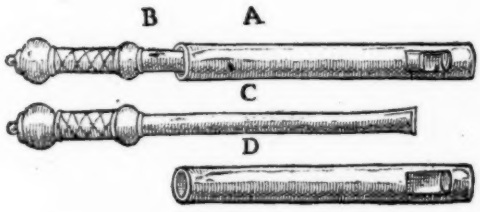


FIG. 14.

Canula type of cautery, from Joubert.

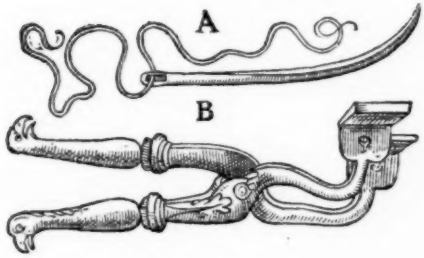


FIG. 15.

Seton and seton-holder, from Joubert.

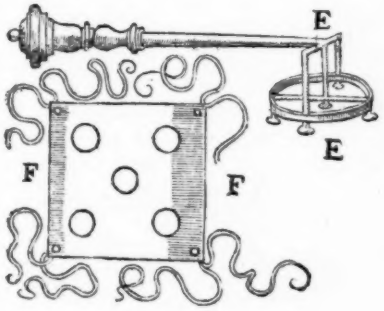


FIG. 16.

Round cautery and plate, from Joubert.

obvious from the description. The remainder require a few words of explanation and are elucidated by Joubert's figures.

Fig. 9, *u* and *v*, illustrates the canula type of cautery. This instrument was hollow with an eye at one end. Into its cavity fitted a rod (fig. 14). This rod was heated and inserted into the canula and thus cauterized the skin only in the region of the eye-hole. Albucasis describes a similar instrument.

Fig. 9, *y*, represents a pair of forceps with flat blades (cp. fig. 15). These grasped the heated needle (*x*) which was then forced through the subcutaneous tissue and left to cool. It was afterwards threaded and the thread drawn through as a seton.

Fig. 9, *z*, the sixth type of cautery, is explained by referring to another of Joubert's drawings (fig. 16). The plate FF in that drawing was bound to the affected limb by means of the accompanying ligatures. Then the cautery EE, which the scribe has roughly sketched in our MS., was applied so that its knobs passed through the perforations in the plate. Albucasis describes a similar instrument.

(*January* 17, 1917.)

A Case of Cupping Instruments.

By Vincent Dickinson, M.D.

The few remarks I am about to offer in showing a case of cupping instruments are not intended to be a dissertation on the art of cupping but merely to arouse some interest in a long discontinued practice. This refers, of course, only to wet cupping, whereby the skin was incised and blood drawn. Dry cupping is still practised by many, and was a therapeutic agent much used by the late Sir Richard Quain in cases of bronchitis and pneumonia. The manual dexterity required of him who aspired to be an expert cupper was such that in former times the operation was usually relegated to a professional cupper, and a small treatise on the Art of Cupping "in which the history of that operation is traced; the various diseases in which it is useful and indicated and the most approved method of performing it is described," was published in 1813 by Thomas Mapleson, Cupper to his Royal Highness the

Prince Regent, to the Westminster Hospital and the St. Pancras Parochial Infirmary. A quotation from Celsus, Lib. ii, Cap. xi, is prefixed to the title page, "Idque auxilium (sc. cucurbitulae) ut minus vehemens, ita majus tutum, neque unquam periculosum. And this remedy, cupping, as it is less violent so it is more safe, nor is it ever attended with danger."

The use of the word "cucurbitula," or gourd, is interesting, for gourds were used in former times for the purpose of eliciting blood after the operation of scarifying, and as such are described by Hippocrates. A small gourd was chosen furnished with two orifices, one of a sufficient size to include the scarifications, the other small; by applying the mouth to the latter, the air was sucked out and a partial vacuum formed; it was then closed by means of a bit of wax, until a proper quantity of blood was obtained. Celsus describes two kinds of cups used in his time: One was of horn, with an aperture adapted to the mouth by which the suction was effected; the other was a brass cup, which was exhausted by a piece of burning flax being put into it at the moment of application. This latter variety of cup may be taken as the connecting link between the gourd exhausted of air by mouth-suction and the cupping glass of later times exhausted by means of heat.

The cupping apparatus which I show to this Section to-day consists of (1) the scarificator, (2) the cups, and (3) the torch or exhausting lamp.

(1) *The scarificator* is a metal box, usually octagonal, being the shape handled with the most facility. It is fitted with two rows of lancets, either acting in the same direction, or, as in Fuller's Improved Scarificator, in contrary directions, performing their half-rotatory motion on the axis of the pinions from the centre to the circumference. These pinions can be removed from the box for the purpose of cleaning and sharpening the lancets. A screw in the centre of the box, to which a milled knob is affixed at the bottom, allows the plate at the top through which the lancets pass to be raised or lowered, so as to give the lancets any length which is thought proper. The success of the operation depended on setting the lancets to a suitable depth. If they were too deep, they penetrated the skin down to the adipose tissue, leaving incisions which allowed the cellular tissue and fat to protrude as soon as the glass was applied, by which the capillaries were compressed and the bleeding prevented. On the other hand, if the incisions were too superficial, the outer layers of the skin only were cut into, and unless they

extended to the cutis vera, no bleeding ensued. For general purposes the scarificator was set so that the lancets projected $\frac{1}{4}$ in. from the surface of the box. Exceptions to this rule were made in the case of different sites to be cupped, behind the ears for example the depth should be $\frac{1}{7}$ in., for the temple $\frac{1}{8}$ in., and for the scalp $\frac{1}{6}$ in. The lancets are sprung through the skin by a trigger-like mechanism, which causes them to be released on pressing the bottom at the side of the box.

(2) *The Cupping Glasses.*—These need no lengthy description; they should be light so as to prevent their falling off easily, and the rim should be flat, not rounded, measuring about $\frac{1}{8}$ in. wide. A thin edge, if the part was irritable or tender, occasioned a good deal of pain; besides this, it caused such a degree of indentation of the skin around the incisions as to cut off all communication between the vessels beyond the area of the cup and those within it, and if a glass with a thin rounded edge were used on a part where there was little tissue between the skin and the bone, as the temple for example, there was small chance of obtaining any blood.

(3) *The Torch or Lamp.*—Usually this consisted of a cylindrical tube of brass or silver about $3\frac{3}{4}$ in. in length and $\frac{1}{2}$ in. in diameter. This was stuffed with cotton and dipped in spirit before lighting. In the cupping set shown here to-day, a teapot-shaped lamp takes the place of the torch.

Briefly the operation of wet cupping—details of which I have gleaned from "A Treatise on Practical Cupping," by Samuel Bayfield, published in London in 1823—was performed as follows: The site having been chosen, and the cups selected with a view to securing complete adaptation to the part, they were immersed in hot water till they became warm. The part itself was fomented with hot water. The operator having poured 2 dr. or 3 dr. of spirit into a glass, took the torch or lamp into his right hand and a cupping glass in the left and placed the lower edge of the glass in contact with the skin, in the exact spot where it was to be fixed. The wick of the torch was now dipped in the spirit and lighted (or the wick of the lamp containing spirit) and carried under the glass to its centre, where it was allowed to remain for about two seconds, and it was then withdrawn quickly. If this had been properly performed the operator would feel the glass sink from his fingers and fix itself to the part; the skin rising slowly into the glass till it occupied nearly one-third of the space within it. The glass having remained affixed for about a minute, during which

time the scarificator should be warmed on the palm of the hand, the operator holding the scarificator in his right hand, took hold of the glass with his left, and insinuating one of the finger-nails of his right hand beneath the glass, the air rushed into it. He instantly removed the glass, and before the swelling had subsided, sprung the lancets through the skin. The glass was then immediately exhausted and applied as before, when the blood would be seen to flow copiously. Much dexterity was required in discharging the scarificator; this had to be done before the distended part had subsided, requiring great quickness of hand without jerking and without force. Each glass, when about three-quarters full, was removed, emptied, and reapplied.

Most of us have had occasion to observe the series of parallel fine rectilineal scars left by previous cuppings, but when the operation was unskilfully performed or followed by suppuration, these resulted in unsightly gapings in a congested skin. À propos of this I remember that the late Mr. Lawson Tait, on the occasion of the opening of the Examination Hall, built by the Conjoint Board of the Colleges of Physicians and Surgeons near the Adelphi Terrace, and now occupied, I think, by the Institute of Electrical Engineers, told the architect that, on surveying its architectural features from the Embankment, it reminded him more than anything else of a badly done cupping scar. I do not suppose the architect felt complimented, or ever forgave Mr. Lawson Tait.

(*January* 17, 1917.)

A Mediaeval Panacea.

By Robert Steele.

Though the word panacea is Greek, no such idea ever entered into Greek medicine or into the schools, Roman, Arabian, or Byzantine, founded on it. A panacea is, in fact, incompatible with any of the theories of disease current in these schools. The idea first entered into medicine from alchemy. Egyptian alchemy sought the making of gold, and to this, or cognate studies, the Greek alchemical tracts are limited. But somewhere about the eighth or ninth century, this science came in contact with the Chinese school, which sought the elixir of life. From

this conjunction a new theory of metals and a new theory of diseases grew—that all metals were diseased brethren of the only perfect one, and that all men might be brought to a perfect state of health by an appropriate medicine. The alchemist went on to assume that the same elixir would answer both purposes, and the world giving credit to him, the idea of a panacea was born in or about the eleventh century. As Bacon defines it, it is a medicine by which life is protracted to its utmost natural limits, since it expels all infirmity, reduces evil complexions to a good condition, and cures all diseases.

The panacea, of which I am to give you an account to-day, is contained in a well-known mediaeval treatise called the "Secretum Secretorum." It is supposed to be written by Aristotle for Alexander, and to contain his secret doctrine. A preface states that it was translated from Greek into Syriac, and from Syriac into Arabic, in the time of the Caliph Al-Mamun, and it seems probable that it really took origin in the seventh century in Syria from Greek originals at about the same period as the Alexander legends, afterwards so famous in Western Europe. We have, however, no Syriac form of the Secretum, though there is some reason to believe that it has existed. The Arabic text, as we know it, is found in two forms, a Western and an Eastern. Both of these have been much added to, but in the Western form the additions come at the end, in the Eastern they are incorporated in the middle of the text. We are thus able to arrive at the original text and to distinguish the various additions to it. The most important of these are three distinct treatises on popular medicine, one on physiognomy, and one on alchemy which contains the earliest form known of the Emerald Table of Hermes.

The Secretum is almost as important in the history of popular medicine as the Regimen Sanitatis of Salerno, not by reason of the panacea of which I am to speak shortly, but because of a short treatise on diet and the preservation of health which was translated into Latin by John of Seville early in the twelfth century, passed quickly into every European language of the time, and was current in chap-books till the eighteenth century. It was founded partly on Greek rules of health, and was followed by a tract on the four parts of the body, their diseases and remedies, taken from Diocles Caristes, "ad valetudinem tuendam" (B.C. 320), and reproduced by Antonius Musa, "ad Maecentatem." The relationship between these tracts I hope to deal with on another opportunity. Another tract founded on Galenic principles precedes these, and a number of chapters on diet, drink,

wine, the bath, &c., follow. Last of all comes the "Mysterious Treasure of the Sages," which is the more immediate subject of our paper to-day. All of these tracts are additions to the original work, whose only reference to medicine is never to have one physician, as he may poison you, but to have ten at least and only take a prescription on which they are unanimous. The additions show what people really wanted to know.

The Secretum is popular still in the East, though the Arabic text has never been printed, but in the West it spread at once like wildfire. The longer Eastern form of it was translated into Latin in the first half of the thirteenth century by Master Philip of Tripoli, and immediately attracted attention. Unfortunately, it contained some magic, and as magic was making a good deal of headway just then it fell into the hands of the Church, and was thoroughly edited, so that its arrangement does not now correspond with that of its original. Moreover, though Philip was a very competent translator, he was not above incorporating the earlier translation of John of Seville (which was made from the Western text) and thus has a number of passages twice over. Philip fails most notably in the names of drugs, but when we consider that we are comparing an Arabic text, with five centuries of possible variations, and a Latin translation of which the earliest known MS. must be three removes from the original, its accuracy is wonderful.

Our best text of it, and almost the earliest, is due to the care of Roger Bacon. As you know, Roger Bacon began his career by writing a medical treatise, "De accidentibus senectutis," which he dedicated to Innocent IV (1243-54). Even then he knew the *Secretum*, but somewhere about 1265 he wrote an introduction to the work, and glosses and notes to the text. In his Introduction he speaks of this medicine as the "Inestimable Glory," and says that it excels everything written in the books of the physicians for the conservation of health and the preservation of strength alike of mind and body. He hints that it is the medicine which prolonged the life of Artephius for over 1,025 years, and tells how he had met a man who bore with him papal testimony to his great age, produced in a similar way. In his note on the text he tells how a well-skilled physician cured the greatest prince in France after the King (that is, Charles of Anjou, most probably), who was covetous, cowardly, sad, melancholy, feeble, and oppressed by many other weaknesses of mind and body, and made him generous, bold, joyous, and free from all faults of body and mind, natural or moral.

Roger Bacon's notes do not point to any knowledge of Arabic. At any rate one of these prescriptions uses the Latin term "mirobalanus," and another the Arabic "amlaj"—in the forms "emleg" and "elileg" —without his recognizing their identity. Of course, we have to remember that the period (*circa* 1265) was still early as regards the circulation of Arabic-Latin vocabularies such as we meet with in later MSS. of Avicenna, &c., and that any translator was forced to rely solely on his own knowledge or that of his native assistant.

This tract is of special interest to those who, like myself, have been led to take an interest in early medical translations from the Arabic. It is comparatively rarely that we can pin down the extraordinary forms which an Arabic word takes in mediaeval Latin, and compare them with an undoubted original. In the case of these simples we can trace the names, not only in classical Arabic, but in most cases in the common bazaar terms for them in the East of to-day—as, for example, preserved in Dymock's "Pharmacographia Indica."

A word on its provenance and date—matters on which I rather seek than offer an opinion. To me it seems that the drugs suggest a North Syrian outlook—perhaps Persian. As for the date, the only help we have is in the comparative simplicity of the prescriptions as compared with those of Rhazes himself or any of his successors up to Avicenna, or with those in the thirteenth century "Syriac Book of Medicine," recently published by Dr. Budge. This would lead us to put it as eleventh century or thereabouts. On the other hand, the very detailed astrological instructions given in the final prescription, with its powdered jewels, gold jar, and exposure to the celestial influences, is late enough, though I believe it is a subsequent addition. As to the author, I have read in vain all the well known Arab or Persian physicians published in Latin or modern translation, and have failed to find anything sufficiently near to any of these prescriptions to suggest identity of authorship—though one or two Indian prescriptions have a certain resemblance.

The texts here before you are, then, the Latin from a late thirteenth century manuscript (I use only one MS. because it is better than any other of the hundred or so known to me in every difference of reading) and a translation from a provisional Arabic text founded on four seventeenth and eighteenth century manuscripts, translated by a native scholar under my supervision, and edited by Mr. A. S. Fulton, of the Oriental Department, British Museum.

The preliminary prescription—and here I must apologize for not

using terms of art—is for the preparation of a medium. It consists of the juice of sweet pomegranates, apples, sugar and grape juice, mixed with that of sour pomegranates, quinces or apples, and boiled down on a gentle fire till it thickens to the consistence of honey. This is added to each of the following medicines as a medium :—

In the first medicine, for strengthening the brain, heart, and stomach, a decoction of roses and violets is made, to this parsley water, sweet marjoram (which Philip translated green mint), and bugloss extract were added. A small amount of myrobalans and cloves was added, and the mixture after standing twenty-four hours was reduced by heat to one-third. The honey was then added, and boiled gently till it thickened, when musk, ambergris and aloes were added in small quantity.

The second medicine, meant to fortify the stomach and act as a gentle purge, uses Cabul myrobalans (which Philip turns into myrobalans and galanga cubebis), Cassia fistula (which he makes caroble de Babilonia), liquorice, myrtle seeds, mastich (terebinth) and tabashir (bamboo manna, which he makes rabarir, and Bacon, making a shot, interprets as rhubarb).

The third medicine, strengthening the principal viscera, heart, brain and liver, is compounded of two myrobalans, cinnamon, galangale and nutmeg. The form darsan cariole is a corruption of Kirfah ed darsini—the popular Arabic name. Elileg is a scribe's error for Emleg.

The fourth medicine assists the appetite and strengthens lungs and chest. It is, in the Latin form, almost insoluble. It seems to mean equal quantities of the juice of vine shoots and brambles bearing wild mulberries (the Arabic has juice of bramble), mixed with celery juice, hyssop juice (translated as field lettuce) and sebestens (radices bethales). To this the Latin adds white vinegar and "effeti," which probably means absinth (afsantin Ar.).

The fifth medicine tempers the stomach, expels melancholy and disperses phlegm and wind. Philip gets three quite impossible names "escines recens" which is French lavender, "berforches" (barberries and "ferat viridi" which must be "Fikah al adkar" in the Arabic. A little aniseed is added to the infusion.

The sixth medicine for the chest, stomach, teeth, and kidneys opens with another puzzle—"colatura seminum coctanorum" which the Arabic tells us are spogel seeds and quince seeds, gum arabic and gum tragacanth (which Philip translates "grana pini").

The seventh also comforts the stomach and expels wind. It consists

of spikenard (or valerian), cinnamon, cubebs and rhubarb ("corawed" is Philip's form for rawūd). We may note that Bacon loses his chance here of a note on rhubarb.

The eighth medicine corrects the liver and the heart, and strengthens all the viscera. It consists of China rhubarb (recenti is Philip's shot at Sina—China) and doronicum (clavini or clavregeh, Philip). Sandal wood and lac are only found in the Arabic, perhaps they were too much for him altogether.

For the final composition of all these into one, the student has to take an equal quantity of fresh tamarind pulp, and boil the whole mixture with rose water till it inspissates, adding as it cools a little oil of balsam, ambergris, and musk. Here, I think, the recipe originally closed, but it now goes on to a grand orgy of expense. Add half an oz. of powdered pearls, 2 dr. each of rubies, sapphires, and amethysts, 3 dr. of emerald dust, and 8 dr. of finely divided gold. Put the electuary thus prepared in a gold pot, incensed with aloes, and expose it to the influences of the heavens for eight days, carefully guarding it from exposure to the Moon in her malignant aspects. The dose is 1 dr. fasting and 1 dr. after meat, and the virtues to be expected—too long to recite—may be read at length.

Latin Text.

De arte conficiendi "gloriam inestimabilem," et primo de arte conficiendi mel, quo conficiuntur medicine posterius dicende, scilicet .8. que componunt "gloriam inestimabilem."

Cum Dei benediccione sume de succo maligranati dulcis .25. rotulas, de succo maligranati acris x. rotulas, et de succo racemi dulcis claro .1. rotulam, et de succo pomorum dulcium .10. rotulas, et de zucaro claro albo et mundo, .10. rotulas. Hec omnia ponantur in vase, ita ut sit ad medietatem, et cum discreccione decoquantur cum suavi igne et penitus sine fumo, et tollatur tota spuma, et tantum decoquatur quousque efficiatur sicut mel spissum. Hoc quidem est illud mel optimum quo conficiuntur medicine; debes ergo uti sicut predictum est.

Prima medicina que confortat principalia membra, scilicet, cerebrum, cor, et stomachum.

Sumatur cum Dei benediccione et ejus adjutorio de rosis rubeis .1. rotulam, et de violis quarta pars rotule, et omnia ponantur in .x. libris aque dulcis, postquam in eo appositum fuerit de aqua eleorongam media pars rotule, et de aqua mente viridis medietas rotule, et de aqua ling(u)e bovis .1. rotula.

Colligantur ita omnia ista et decoquatur cum .1. uncia de elegaman et gariophilo. Et hec omnia debent stare super ignem per unam diem et noctem donec egrediatur tota vis eorum. Deinde ponatur supra ignem suavem donec diminuatur tercia pars aque, deinde deponatur et permittatur clarescere. Et postea ponantur in eo de preparato et predicto melle .iij. rotule, et tantum decoquatur quousque spissum fiat. Deinde ponatur in eo una dragma et dimidia de ambra, et .iij. dragme de ligno aloes trito et humido. Et hec est prima medicina sive pocio cujus effectus est confortare cerebrum, cor, et stomachum.

Confeccio medicine .2. cujus est corroborare stomachum et constringere et purgare putridos humores stomachi sine violencia et abhominacione.

Sumatur cum Dei benediccione de mirabolanis et galanga cubebis rotula, ejecto cortice citrini, de medulla caroble de Babilonia quarta pars unius rotule, et de liquiricia sine cortice que est citrini coloris due uncie, et de granis maturis de virotis in suo tempore due uncie. Conterantur bene hec omnia et ponantur in .10. rotulis aque dulcis per unam diem et unam noctem. Deinde decoquatur suaviter donec deficiat medietas aque, deinde misceatur et coletur donec fiat clarum. Et tunc ponantur in eo de melle preparato due rotule et tantum decoquatur iterum done(c) inspissetur. Deinde apponatur de pulvere masticis uncia una, et de rabarir quarta pars uncie. Et hec est secunda medicina cujus proprietas est corroborare stomachum et confortare et constringere et purgare malos et putridos humores qui congregantur in stomacho sine abhominacione et violencia et sine aliqua lesione. Insuper confortat cathenam corporis et pectus et cerebrum.

Confeccio medicine .3. cujus est confortare omnia membra occulta et maxime principalia, scilicet, cor, cerebrum, et epar.

Sumatur cum Dei benediccione una rotula de emleg et dimidia, et de elileg Indico dimidia pars unius rotule, et de darsan cariole, et de galanga, et de nuce muscata una uncia. Teratur totum non nimis subtiliter et ponatur in aqua dulci .x. rotulis, et maneat in ea per unam diem et unam noctem. Deinde decoquatur cum suavi igne et suaviter donec minuatur medietas, deinde misceatur et coletur donec clarum fiat, et postmodum ponatur cum tribus rotulis mellis artificialis; deinde tantum bulliat donec densum fiat. Et hec est siquidem tercia medicina, cujus proprietas est corroborare omnia membra occulta et maxime principalia.

Confeccio medicine quarte que juvat appetitum et instrumentum pectoris et pulmonem.

Sumatur cum Dei benediccione de aquam palmitum et de rubis ferentibus mora silvestria libra .1., de aqua salsa colata bene .1. libra, de aqua apii colata media libra, de aqua lactuce agrestis libra .1., de aqua radicum bethales

libra .1.: deinde totum colligatur et congregetur in unum et ponatur in eo aceti albi quarta pars "sub," et effeti quarta pars libre. Colligatur totum et stet per unum diem et unam noctem; deinde coletur, et ponantur in eo de melle libre .iij.; deinde decoquatur cum igne levi et tenui donec inspissetur. Et est quarta medicina; et ejus est proprietas quod juvat appetitum et instrumentum pectoris, et pulmonem.

Confeccio medicine quinte que expellit melancoliam, subtiliat flegma, et tollit humiditates, stomachum temperat, aperit constipaciones et ventositates expellit.

Sumatur escines recens libre ½, et berforches libre ½, et de ferat viridi .iij. uncie, et de trifera viridis "sub" .i. et ponantur in .xij. "sub" aque dulcis, addantur cimini "sub" .iij., et stet per unum diem et unam noctem. Deinde decoquatur donec medietas aque diminuatur, et sumantur "sub" .4. de melle preparato, et iterum tantum decoquatur donec inspissetur. Hec est igitur quinta medicina cujus proprietas est expellere melancoliam, subtiliare flegma, et superfluas humiditates tollere, stomachum temperare, aperire constipaciones, et ventositates expellere.

Medicinâ sexta leniens pectus, conferens respiramento, reparans stomachum, repellens dolorem dencium et renum.

Sumatur cum Dei benediccione de colatura seminum contrariorum medium "sub" de qualibet, de gumi arabico uncie .iij. et de granis pini uncia .i. Dissolvantur omnes in aqua rosata et apponantur de melle preparato "sub" .iij., et inspissetur bona inspissacione super ignem. Hec est medicina sexta, cujus proprietas est linire pectus, et conferre respiramento, reparat stomachum, et repellit dolorem dencium et renum.

Medicina .7. reparans stomachum et fugans ventositatem.

Sumatur cum Dei benediccione de spica indica dragme .iij., et de aretim, et tantum de amomo, et de cubebis dragme .iij., equaliter de corawed longo et brevi dragme .iij., et ponantur tres "sub" aque dulcis, et iterum stet donec exeat virtus ejus. Deinde teratur et coletur, et ponantur de melle preparato et clarificato in quantitate "sub" .iij., et decoquatur ab igne levi donec inspissetur. Hec est ergo septima medicina, cujus proprium est reparare stomachum et fugare ventositatem.

Medicina octava emendans epar, reparans cor, et confortans omnia membra interiora.

Sumantur de reubarbaro recenti solido dragme .iij., et de clavini dragma .i., et constringatur totum, et desuper ponatur de aqua dulci, "sub" .x., et stet in ea donec virtus ejus exeat. Deinde teratur leniter et diu et coletur, et desuper apponantur de melle preparato "sub" .iij., et decoquantur levi igne

donec inspissetur et ingrossetur. Hec est medicina octava, cujus proprietas est emendare epar, et reparare cor, et confortare interiora membra universa.

Medicina nona et ultima que est finis omnium medicinarum, cujus laudes patent in littera.

Deinde sumantur et aggregentur omnes octo medicine precedentes, et sic erit nona et ultima medicina et finalis. Deinde ponantur ad quantitatem omnium dactili Indici recentes mundati a granis suis, quorum interiora sunt tenera ad modum cerebri. Et si non inveniatur tale, dissolvatur in aqua donec egrediatur virtus, sicut factum fuit in predictis. Deinde congregetur totum in uno vase et fundatur desuper aqua rosata odorifera .vi. "sub," et decoquatur quiete totum donec videatur condensari. Deinde tollatur ab igne et dimittatur donec tepefiat. Consequenter ponatur in ea de optima ambra dragme .iij., et de musco optimo dragme .iij., et ponantur in dicto electuario de margaritis bene tritis medium "sub," et de pulvere lapidum preciosorum rubei coloris et celestis et flavi ana dragma .i. aut duas dragme de quolibet, et de succo nardi dragme .iij., et de subtili pulvere auri dragme .ix. Deinde ponatur hoc electuarium totum sic confectum, ut dictum est, cum predictis rebus in vase aureo subfumigato cum ligno aloes. Deinde ponatur sub divo ad serenum, ut descendant virtutes spirituales in ipsum, per octo dies. Et non permittatur ibidem in illa nocte in qua Luna erit malignior et carens cursu superiori in radiis suis.

Quando igitur completa est et juxta hunc modum consummata, erit de thesauris philosophorum hujus mundi. Accipe ergo de ipso post cibum dragma .i. et similiter ante cibum dragma .i. quia est summa omnium medicinalium (vel medicinarum) et finis cujuslibet medicinalis intencionis: et ad fugandum melancoliam et coleram flammeam, et calorem renum, et flegma, et ad fugandum emoroydas, et ad repellendum inflacionem, et salsam coleram, et ad dirigendum cibum, et ad temperandum complecciones, et ad subtiliandum musculos, et ad quiescendum timpora (vel tempora), et ad tollendum dolorem capitis, et ad visum clarificandum, et ad senectutem retardandam, et ad subtiliandum vocem, et ad effugandas fumositates, et quiescendas omnes molestias et egritudines apparentes et occultas. Aperit opilaciones, expellit ventositates, et non permittit aliquem locum apostemari nec colleccionem humorum facere. Et impedit naturales infirmitates et humectare facit corpus, et facit bene urinare et tollit tussim et confortat spinam dorsi, juvat respiracionem; et generat gaudium et letificat, et multa alia bona facit que longum esset enunciare. Set habet specialem proprietatem ad generandum intellectum et ad nutriendum cerebrum. O Alexander, ideo non oportet te aliquo tempore aliam medicinam sumere, set hac utaris.

Translation of the Arabic Text.

Description of the Honey in which the said Medicines are prepared.

Take, with the help of God, 25 ratls of the juice of sweet pomegranate and 10 ratls of the juice of sour pomegranates, 10 ratls of the juice of sour apples, and 1 kust[1] of pure rubb (syrup) of sweet grapes[2] and 10 ratls of the sugared conserves of roses. Put all the above in a clean stone-kettle, and burn under it a gentle steady fire without smoke, and keep removing the froth from time to time. Boil it thus until the mixture turns into the form of honey. It is the medicated honey which shall be used in the medicines I shall mention below (if God wills).

The First Medicine.

Take 1 ratl of dry red roses and $\frac{3}{4}$ ratl of violets. Soak them in 10 ratls of fresh water, after mixing with it $\frac{1}{2}$ ratl of the water of parsley (or bahn-gentle), $\frac{1}{4}$ ratl of sweet marjoram, and 1 ratl of the water of ox-tongue. Having mixed all these, add to them 2 oz. of myrobalan, and 1 oz. of cloves. Leave the whole mixture for one day and night, so that its power comes out. Then boil it on a gentle fire until one-third of its water evaporates. Then take it down from the fire, and press the solid portion of it with the hand. Then drain the mixture and add to it 3 ratls of the above-mentioned honey. Then boil the whole mixture again (until it becomes thick like honey), and add to it $1\frac{1}{2}$ dr. of musk, 1 dr. of ambergris, and 3 dr. of powdered fresh aloes. This is the first medicine, one of the eight medicines which shall be mentioned. Its property is to strengthen the stomach, the heart, and the brain (God willing).

The Second Medicine.

Take 1 ratl of Kabul myrobalans, peeled from its stone, $\frac{1}{4}$ ratl of the kernel of Khiyar Shanbar (cassia fistula), 2 oz. of the root of liquorice peeled, and of moderate thickness, and 2 oz. of myrtle seeds, in a ripe state. Break and pound the hard substances, and soak the mixture in 10 ratls of fresh water. Leave it for a day and night. Then boil it gently until half of its water evaporates. Then rub with the hand, drain, and add to it 2 ratls of the medicated honey. Boil the mixture again until it becomes thick like honey. Then add to it 1 oz. of powdered mastich and $\frac{1}{4}$ oz. of tabashir, and bottle up immediately. This is the second medicine. It strengthens the stomach, wrings, and cleanses it of all putrid matter without causing any pain, and it strengthens the nerves, the chest, and the brain.

[1] A ratl is a pound of 12 oz.; a kust is a little less than 10 ratls.

[2] Another MS. adds: "10 ratls of the juice of sweet apples."

The Third Medicine.

Take 1½ ratl of myrobalan and ½ ratl of the Indian myrobalan, and 1 oz. each of cinnamon, the herb galangale, and nutmeg. Break them all into pieces, and soak them in 10 ratls of fresh water. Leave them thus for a day and a night. Then boil the concoction on a gentle fire until half of its water is evaporated. Then rub the contents with the hand and drain. Then add to it 3 ratls of the above-mentioned honey, and boil again until the compound becomes like "rubb" or honey. Then bottle it up. It is the third medicine, and its property is to strengthen the inner organs, especially the vital ones.

The Fourth Medicine.

Take 2 ratls of the juice of the fresh and cleaned ansaj (bramble), ¼ ratl of the juice of karafs (celery), and ½ ratl of fresh zūfā (hyssop) and mukhayyat (sebestens) each. Add them together, and leave them for one day and night. Then drain, and add 2 or 3 ratls of the medicated honey. Then boil on gentle fire. This is the fourth medicine, and its property is to open up obstacles, and to benefit the organs of the chest and the lungs.

The Fifth Medicine.

Take ½ ratl of fresh ustukhudus (French lavender), and ½ ratl of bubaris (barberries), and 3 oz. of fikāh al adkhar. Soak them in 12 ratls of fresh water, and add to them 3 oz. of anisum (aniseed). Leave them thus for a day and night. Then boil until one half of the water is evaporated. Add 4 ratls of the medicated honey, and boil into rubb, and bottle up. This is the fifth medicine. Its property is to drive out gently spleen and phlegm, to cure melancholia, restore the stomach to its proper order, to open up the internal obstacles, and to break up the winds.

The Sixth Medicine.

Take ½ ratl of the juice of bazr-katuna (spogel seeds), ½ ratl of the juice of the seeds of quince, 2 oz. of al katira (gum tragacanth), and 3 oz. of gum arabic. Dissolve the gums in rose water, and add 3 ratls of the medicated honey. Boil well on the fire and bottle up. This is the sixth medicine, and its property is to soften the chest, cure asthma, restore all the breathing instruments to their proper condition, put down the feverish heat, and to feed the bowels.

The Seventh Medicine.

Take 1 oz. of spikenard, 3 oz. each of cinnamon, kurfa (a kind of cinnamon) and cubebs, and ½ oz each of round and of long rhubarb. Soak them in 5 ratls of sweet water. Leave them until their strength comes out. Then rub them with the hand, and drain. Add to it 3 ratls of pure medicated honey, and boil on gentle fire into the form of rob. This is the seventh medicine. Its property is to heaten the stomach and to drive out the winds from it.

The Eighth Medicine.

Take 3 oz. of hard China rhubarb and 1 oz. of darwanj sini (doronicum of China), ½ oz. each of the three kinds of sandal wood, and 1½ oz. of purified lakk. Break them all into pieces, and pour over them 10 ratls of sweet water. Let them soak in it until their power comes out. Then press them through with the hand, drain, and add 3 ratls of pure medicated honey. Boil slowly on a gentle fire. This is the eighth medicine, and its property is to restore the liver and the heart and to strengthen the inner organs.

Finally, when all the above-mentioned eight medicines are prepared, add to them one-fourth of their collected weight of fresh tamarind, whose stones have been removed, and which has been previously dissolved in water as mentioned before. Then, add the eight medicines with the dissolved tamarind in a large stone kettle, and pour over them 6 ratls of sweet-smelling rose-water. Boil the compound gently until it becomes like "rubb." Then take it down from the fire and let it cool. Then take 1 oz. of the oil of balsam, 3 dr. of ambergris, 4 dr. of the most fragrant musk, and add them to the compound of the medicines. Then add ½ oz. of powdered pearls, 6 dr. of powdered rubies—red, blue, and yellow (2 dr. of each), 3 dr. of powdered emerald, 8 dr. of powdered gold. Then put the whole mixture in a porcelain pot, or better still, in a golden vessel, previously scented with the incence of aloes. Then place the vessel under the sky for a week so that the heavenly powers may affect it with their influence. But do not expose the compound to the heavens on the night when the moon is inauspicious, or free from its journey, or "under the rays."

When all these directions have been carried out thou shalt obtain one of the greatest treasures of the world, and the most precious possessions of kings.

Then take from it 1 mithkal every day after food, and 2 mithkals on an empty stomach, for it is the extreme of all extreme objects.

A few uses of this grand medicine are the removal of phlegm, bile and spleen, heating the kidneys, driving out piles and wind, digesting the food, moderating the temperament, clearing the complexion, softening the chyme, allaying the headache, clearing the eyesight, purifying the colour and the smell of the body, strengthening the teeth, stopping greyness of the hair, opening the pores and outlets of the body. It removes all pain and diseases—external or internal—drives out wind, prevents foulness of breath, exhilarates the motions of the bowels and the bladder, removes cough, strengthens the nerves and muscles, purifies the blood, heats the food and helps digestion.

It also removes palpitation of the heart from whatever cause it may be, stimulates and cheers the heart, and does many other benefits, too numerous to relate. One of its peculiar virtues is to create wisdom by increasing the brain matter, and to strengthen the intellectual faculties generally. I do not know of any medicine discovered by philosophers excelling this one in preserving health and strength, and benefiting bodies and minds. Therefore take to it.

COMPARATIVE TABLE OF LATIN AND ARABIC PRESCRIPTIONS.

First Medicine—

de rosis rubeis	1 lb.	dry red roses.
violis	¼ „	¾ violets.
aqua eleorongam	½ „	water of parsley.
„ mente viridis	½ „	¼ sweet marjoram (marzankūsh).
„ lingue bovis	1½ „	1 lb. water of bugloss.
elegaman	1 oz.	2 oz. myrobalans (amlaj).
gariophilo	1 „	cloves.
ambra	1½ dr.	1 dr. ambergris.
ligno aloes	3 „	fresh aloes.
	1½ „	musk.

Second—

de mirabolanis galanga cubebis	1 lb.	black myrobalans (halila kabli).
caroble de Babilonia	¼ „	kernel of Cassia fistula (khiyar shanbar).
liquiricia	2 oz.	liquorice root.
granis maturis de virotis	2 „	myrtle seeds.
pulvere masticis	1 „	powdered mastich.
rabarbir	¼ „	tabashir.

Third—

de emleg	1½ lb.	myrobalans (amlaj).
elileg Indico	½ „	emblic myrobalans (halila Hindi).
darsan cariole	1 oz.	cinnamon (kirfah ed darsini).
galanga	1 „	galangale.
nuce muscata	1 „	nutmeg.

Fourth—

de aqua palmitum et de rubis	1 lb.	2 lb.	juice of bramble (ansaj).
„ salsa cayrsen	1 „		
„ apii colata	½ „	½ „	juice of celery (karafs).
„ lactuce agrestis	1 „	½ „	„ hyssop (zūfā).
„ radicum bethales	1 „	½ „	sebestens (mukhayyat).
aceti albi	¼ „		
effeti (afsantin, absinth)	¼ „		

Fifth—

de escines recens	½ „	fresh lavender (ustukhudus).
berforches	½ „	barberries (barbaris).
ferat viridi	3 oz.	(kafar *or* Fikah aladkar) squinanch (flowering grass).
trifera viridis	1 lb.	
cimini	3 „	3 oz. aniseed (anisūn).

Sixth—

de colatura seminum coctanorum, *each*	½ lb.	— juice of spogel seeds (bazr kātūna).
	½ lb.	juice of quince seeds.
gumi arabico	3 oz.	3 oz. gum arabic.
granis pini	1 „	2 „ gum tragacanth (alkatira).

Seventh—

de spica indica	3 dr.	1 oz. spikenard.
aretim	3 „	3 „ cinnamon (darsin).
amomo	3 „	3 „ kurfa cinnamon.
cubebis	3 „	3 „ cubebs.
corawed longo	3 „	½ „ long rhubarb (rawūd).
corawed brevi	3 „	½ „ round rhubarb.

Eighth—

de reubarbaro solido	3 dr.	3 oz. hard China rhubarb (rawūd sini).
clavini (or clavregeh)	1 „	1 „ Doronicum (Darwanj sini) azlak.
		1½ „ each 3 kinds of sandal wood.
		1½ „ purified lac.
Mix all with addition of de dactili Indici		¼ weight of tamarind pulp.
		1 oz. oil of balsam.
ambra		3 dr. ambergris.
musco	3 dr.	4 „ musk.
margaritis tritis	½ lb.	½ oz. powdered pearls.
lap. prec. rubei		2 dr. „ rubies.
„ „ celestis		2 „ „ sapphires.
„ „ flavi		2 „ „ amethysts.
succo nardi		3 „ „ emerald.
pulvere auri	9 dr.	8 „ gold dust.

Section of the History of Medicine.

President—Dr. Raymond Crawfurd.

A Review of the Medical Literature of the Dark Ages, with a New Text of about 1110.[1]

By Charles Singer, M.D.

Contents.

Chapter		Page
I.	Medical Science in the Dark Ages	107
II.	The Scientific Encyclopædia contained in MS. 17, St. John's College, Oxford (*circa* 1110-12)	117
III.	Text of the Medical Compendium, being a Section of the Encyclopædia	128
IV.	A Partial Translation of the Medical Compendium, with Commentary...	149

(I) Medical Science in the Dark Ages.

In the West of Europe medicine, like other sciences, may be said to date its second birth from the advent of translations of the so-called *Arabian* writers. The works of these authors began to be well known in the second half of the twelfth century. Latin translations from Arabic versions and from Arabic commentaries on Aristotle, Galen, Ptolemy, and other Greek writers, formed for at least the next three centuries the basis of all scientific knowledge. For the purposes of medical history we therefore place the termination of the Dark Age and the beginning of the true mediaeval period somewhere about the year 1150. The beginning of the Dark Age of Medicine we are inclined to place at the death of Bede in 753. In our discussion of the medical

[1] A preliminary account of this work has appeared in the *Bulletin of the Society of Medical History of Chicago.*

literature of this earlier barren period we shall refer only to works available to Western readers, leaving the consideration of the copious Byzantine material for a subsequent occasion.

Even before the twelfth century, a certain amount of Arabian learning had filtered into Europe. Constantine the African, who ended his days in 1087 as a monk at Monte Cassino, had already interpreted into Latin the Arabic writings of Isaac Judaeus (died 941), and his work set deep its stamp on the already ancient medical school of Salerno.[1] A generation or so later certain of the same, or at least very similar, works were circulating in Europe, bearing the name neither of Constantine nor of Isaac but of Haly Abbas (died 994) as their author, and of Stephen of Antioch (1127) as their "translator."[2] Soon, also, the labours of Herman the Dalmatian (flourished 1143) were doing for geographical and astronomical science what Constantine and Stephen had done for medicine.[3] Gradually these works spread over the Continent; but it was not until the end of the twelfth or the beginning of the thirteenth century that the deluge of translations by Gerard of Cremona (1114-87), Gerard of Sabbionetta (thirteenth century)[4] and others effected that thorough Arabicization of science

[1] The collected works of Constantine were printed in two volumes by Henricus Petrus at Bâle, 1536 and 1539. There are several other editions of single works and collections of the works of Constantine. For the influence of Constantine see Moritz Steinschneider in *Virchow's Archiv*, Berl., 1866, xxxvii, p. 351; F. Wüstenfeld, "Die Uebersetzungen arabischer Werke in das Lateinische seit dem XI Jahrhundert," *Abhandl. der Kgl. Ges. der Wissensch.*, Göttingen, 1877, xxii, p. 10; K. Sudhoff, "Die medizinischen Schriften, welche Bischof Bruno von Hildesheim in seiner Bibliothek besass, und die Bedeutung des Konstantin von Afrika im 12 Jahrhundert," in the *Arch. f. Gesch. d. Med.*, Leipz., 1916, ix, p. 348; Charles Singer, "A Legend of Salerno," in the *Johns Hopkins Hosp. Bull.*, Balt., January, 1917.

[2] The so-called "Liber Regius" of Haly Abbas, a work which bears an extremely close resemblance to the "Pantegni" of Isaac Judaeus as rendered by Constantine, has twice been printed; viz., at Venice in 1492, and at Lyons in 1523. Both editions bear the statement that the translation was made by *Stephanus philosophi discipulus* in 1127.

[3] Herman the Dalmatian, a translator of the twelfth century, appears to have completed his rendering of the "Planisphere" of Ptolèmy at Toulouse in 1143. He was probably also the translator from the Arabic of a work known as the "Experimentarius" of Bernard Sylvestris. This work is as yet unpublished, but exists in two excellent MSS. at the Bodleian Library at Oxford.

[4] Much has been written on these two industrious translators from the Arabic. A MS. at the Vatican, which throws a flood of light on them and their work, is described and printed by Prince Baldassare Boncompagni, "Della vita e delle opere di Gherardo Cremonense, Traduttore del secolo duodecimo, e di Gherardo di Sabbionetta, Astronomo del secolo decimoterzo," Rome, 1851. Boncompagni's MS. has been recently re-examined by K. Sudhoff, "Die kurze 'Vita' und das Verzeichnis der Arbeiten Gerhards von Cremona, von seinen Schülern und Studiengenossen kurz nach dem Tode des Meisters (1187) zu Toledo verabfasst," in the *Arch. f. Gesch. d. Med.*, Leipz., 1915, viii, p. 73.

that was so striking a feature of the succeeding age. Thus the great expounders of mediaeval science, men such as Alexander of Neckam (1157-1217), Vincent de Beauvais (1190-1264), Albertus Magnus (1193-1280), Roger Bacon (1214-94), St. Thomas Aquinas (1225-74), and Bartholomew de Glanvil (flourished 1260), all drew on Arabian sources. Indeed that characteristic product of the Middle Ages known as "Scholasticism" is largely the result of the Arabic revival.

In this respect the medical compendium, the text of which we here print, is pre-Arabian and pre-mediaeval. It is a product of Western learning untinged by any Oriental influence, and represents the state of knowledge in Europe at the very end of the pre-scholastic period. It thus belongs to what we call the *Dark Age* of medicine.

Medical MSS. of mediaeval origin are numerous in the great libraries of Europe, but the overwhelming majority date from the Scholastic or Arabian period. Remnants of the pre-scholastic or Dark Age medicine are much more scarce, and are mainly of works that originate from Salerno—that one lamp of ancient science that continued burning in Christian Europe throughout the Dark Ages. There survives, however, a small residue of medical writings, at once pre-Arabian and non-Salernitan, though often deeply influenced by the writings of Salerno. How small is this residue we will now proceed to show.

St. Isidore of Seville (570-636) and the Venerable Bede (675-735) both made contributions to medical literature which, though slight in themselves, were of importance as influencing their successors. The four centuries which follow the death of Bede are extremely barren of medical works as regards the West of Europe, and we shall therefore be able to review briefly most of this scanty literature. The works that we have to consider fall naturally under three heads:—

(*a*) Vernacular literature.
(*b*) Latin extra-Salernitan literature.
(*c*) Salernitan literature.

(*a*) *Vernacular Literature.*—There is a small number of these early works in the Teutonic languages.[1] The most considerable item among them are the two books of the English "Leech Book of Bald," and the Leech Book included in the same MS. as Book III of the

[1] A fairly complete list of the published Teutonic vernacular medical texts antedating the sixteenth century, is given by K. Sudhoff in his "Die gedruckten mittelalterlichen medizinischen Texte in germanischen Sprachen," *Arch. f. Gesch. d. Med.*, Leipz., 1910, iii, p. 273. He gives eighty-nine texts in all. Of these not more than thirteen are earlier than 1100, and of the thirteen all save three or four and all the longer texts are English.

"Leech Book of Bald,"[1] probably all written in the first half of the tenth century.

An MS., partly Latin and partly Anglo-Saxon, attributed to Byrhtferð, monk of Ramsey, and probably dating from the early years of the eleventh century, has a section on the relation between the four seasons of the year, the four qualities (heat, cold, dryness, and moisture) and the four Humours.[2] One of the figures of our St. John's MS. appears to supply a copy of a lost diagram (*see* fig. 3) from this unique MS. of the "Handboc" of Byrhtferð. The work is a miscellany, consisting largely of calculations for the Calendar. We will return to the discussion of the section here noted when we examine the figures of the "Physical and Physiological Fours," from the St. John's MS.

Another Anglo-Saxon MS. has a section on unlucky Egyptian days when blood-letting must be avoided. This is taken from the "De Minutione Sanguinis siue de Phlebotomia," of Bede, whose name, however, is not mentioned.[3]

Another Anglo-Saxon work of some bulk is a translation, prepared in the first half of the eleventh century, of the Herbarium of Apuleius, a Latin work which itself dates from six cycles earlier. This, like the Leech Books, contains only a list of herbal remedies with the conditions for which they are to be used. Then there is an Anglo-Saxon document bearing the Greek title "περὶ διδαξέων,"—"of the Schools of Medicine," but containing only one short paragraph devoted to that subject, and for the rest consisting merely of a list of symptoms with appropriate herbal remedies. This work dates from the twelfth century and draws largely on the Salernitan author Petrocellus.[4] The scant remaining portion of Anglo-Saxon medical literature consists only of scraps of recipes, charms and glossaries.

[1] British Museum, MS. Royal 12, D, XVII.

[2] Bodleian MS., Ashmole 328. The Anglo-Saxon version of the section on the "Fours," is on pp. 10, 11. The MS. has been described by H. Wanley, "Librorum Vett. Septentrionalium qui in Angliae Bibliothecis extant," Oxford, 1705, and by several modern writers including K. M. Classen, "Uber das Leben und die Schriften Byrhtferðs," Dresden, 1896, and Frank Clifton Smith, "Die Sprache der Handboc Byrhtferðs und des Brieffragments eines unbekannten Verfassers," Leipz., 1905. The main Anglo-Saxon portion of the MS. has been printed by F. Kluge in "Anglia," Halle, 1885, viii, pp. 298-337. The whole MS. is being edited for the Early English Text Society by Professor G. Hempl, of Stanford University, California.

[3] British Museum MS. Cotton Vitellius c. viii, fol. 22 recto. We may perhaps mention here also a copy of Bede's table of lucky and unlucky days of the month for phlebotomy, in British Museum MS., Arundel 60, fol. 1 recto, dating from the eleventh century. Though the legends of the table are in Latin, the Anglo-Saxon script is used.

[4] The work of Petrocellus is printed in S. de Renzi, "Collectio Salernitana," Naples, 1852-56 (5 vols.), iv, p. 185.

The general characteristic of this Anglo-Saxon literature is the wide knowledge of herbs which it displays, together with the extreme simplicity of its pharmaceutical processes. There are no anatomical descriptions, there is no attempt to separate diseases from symptoms. The pathological condition is named and the remedy follows (as in the *περὶ διδαξέων* or the Leech Books), or the herb is named and the symptom follows (as in the first part of the Herbarium of Apuleius).[1]

The other languages of the Teutonic group are even poorer than Anglo-Saxon in the number and character of their early medical texts. Of Old High German, anterior to the twelfth century, we have a couple of recipes of the eighth century, while a Latin recipe of the ninth century provides us with a few High German glosses. A third work is the *Glossae Latino-barbaricae de partibus humani corporis* of Hrabanus Maurus, abbot of Fulda (died 847).[2] This contains a list of the Latin names of the parts of the body, and gives in some cases their supposed derivation and in others their German equivalents. A fourth text is a recipe which yields some German plant names interlining the Latin of a later but doubtful date.

The remaining Teutonic dialects afford yet scantier relics, and no scrap of medical lore ante-dating the twelfth century appears to have reached us in any of the Middle German, Low German, or Scandinavian dialects. The Gallic group of languages is in the same case, while the known Celtic medical MSS. are all later than the eleventh century. We may thus say that for practical purposes vernacular medicine in Europe previous to the twelfth century is confined to the Anglo-Saxon texts.

(*b*) *Latin Extra-Salernitan Literature.*—When we turn to the Latin writings that have come down to us from the Dark Age we find ourselves better off, but the material is still very scanty. A number of recipes and fragments have doubtless survived and may lie scattered

[1] The early Anglo-Saxon literature has been in the main printed by O. Cockayne, "Leechdoms, Wortcunning, and Starcraft of Early England," 3 vols., Lond., 1864-66. Much interesting material on early English medicine is contained in a number of other works; among which the more important are: J. F. Payne, "English Medicine in Anglo-Saxon Times," Oxford, 1904; Max Löweneck, "Περὶ διδαξέων, eine Sammlung von Rezepten in englischer Sprache," in the "Erlanger Beiträge zur englischen Philologie IX," Erlangen, 1896, and Dietrich in Haupt's *Zeitschr. f. deutsches Altertum*, Berl., 1867, new series, i, p. 202 *et seq.* The Leech Books from MS. Royal 12 DXVII have been printed again by G. Leonhardi in the "Bibliothek der Angel-sächsischen Prosa begründet, von W. M. Grein, fortgesetzt von R. P. Wülken," Hamburg, 1905, vi.

[2] Printed by Melchior Haiminsfeld in his "Alamannicarum Rerum Scriptores aliquot veteres," Frankfort a/M., 1606, ii, 89, and in Migne "Patrologia Latina," cxii, col. 1575.

through the literature. Passing by these, however, we will consider only the texts proper.

First, though hardly falling within our period, is the "Commentarium medicinale" of Benedictus Crispus, Archbishop of Milan (died 735).[1] This consists of 241 verses entirely on the uses of herbs; it is based partly on Pliny and Dioscorides, but is believed to contain also some genuine folk medicine.

The work of Hrabanus Maurus, "De homine et partibus ejus" (*circa* 844),[2] can hardly be called medical, but a Latin medical work of similar date is the "Hortulus" of "Squinting Walafrid" (Walafridus Strabus, 807-849).[3] This writer had studied at that ancient seat of learning St. Gall, and later with Hrabanus Maurus at Fulda. Subsequently he became Abbot of Reichenau, near Constance. Walafrid's "Hortulus" consists of 444 lines of rhyme, arranged in twenty-three stanzas, each of which deals with a single herb. In some cases the description of the plant itself is good, but the medical element of the poem consists only of an enumeration of the symptoms for which each plant is beneficial.

Another work in Latin that can be referred to the ninth century, or earlier, and that has come down to our time, is a translation of the work "Περὶ *οὔρων*" of Galen. A very early MS. of this translation still exists at St. Gall, and others are to be found at Monte Cassino, Rome, and London.[4]

After the ninth century ensues a long and barren interval, but we must here again mention the section on the "Physical and Physiological Fours" in Byrhtferð's "Handboc."[5] Towards the end of this darkest period we encounter several Latin medical works dating from the first half of the twelfth century. Of these probably the earliest, except the Compendium before us, is the *Lapidarius* of Marbod of Anjou, Bishop of Rennes (died 1123).[6] This very popular

[1] The work of Benedetto Crispi has been several times printed in modern times. The most accessible edition is that in Migne "Patrologia Latina," lxxxix.

[2] Printed in Migne "Patrologia Latina," cxi, col. 137, as Book VI, cap. i, of the "De Universo," of Hrabanus.

[3] The work of Walafridus Strabus was printed twice in the sixteenth century. Among the more valuable modern editions are those of L. Choulant, Leipz., 1832, and F. A. Reuss, Würzburg, 1834. The "Hortulus" is reproduced also in Migne, cxiv, col. 1119.

[4] *See* K. Sudhoff, *Arch. f. Gesch. d. Med.*, Leipz., 1916, ix, p. 352.

[5] MS. Ashmole 328. The Latin version of this section is on pp. 9-11.

[6] Marbod's work is printed in Migne, clxxi, together with an interesting old French version.

work describes the supposed action of precious stones, and is only partly medical. It is the earliest medical work that has come down to us that exhibits the glamour and fascination with which the Arabians were already regarded, though it has few or none of the characteristics of Arabian medicine.

The next most ancient work of medical importance that the twelfth century produced is the *De bestiis et aliis rebus* of the mystical writer Hugo de Sancto Victore (1095-1141). This collection contains two interesting sections, *De hominis membris ac partibus* and *De natura hominis*.[1] Both chapters are valuable as giving us our first glimpse of anatomical knowledge in the West after the profound intellectual depression of the tenth and eleventh centuries. Hugo's knowledge is partly traditional, but is partly derived from a Latin translation of the Timaeus of Plato, which had become available in his day. Salernitan sources were also probably available to him. In many places in this work, and in its fellow, the *De Arca Noe mystica*, there is a similarity between the phraseology of Hugo and that of our Compendium, but there is no reason to suggest any direct contact between the two.

A contemporary writer, who resembled Hugo in certain respects, was Bernard Sylvester of Tours. Bernard's work, *De Mundi Universitate sive Megacosmus et Microcosmus* appeared between 1145 and 1153.[2] He is deeply under the influence of the Timaeus, and gives a description of the parts of man's body on the basis of the doctrine that man himself displays in his various organs a model of the greater world.

A somewhat similar writer of the same mystical group as Bernard Sylvester of Tours and Hugh of St. Victor, is Hildegard of Bingen (1098-1180). Her work, *Liber divinorum operum simplicis hominis a vivente luce revelatorum* has been discussed elsewhere by the present writer.[3] It may have been written with reference to the works of Bernard and Hugh. It was composed, like them, with the object of showing the similarity between the body of man and the structure of the universe, but it belongs to a considerably later period, and was written between 1163 and 1170.

We may here briefly refer to the work of Odo of Meune, Abbot of Beuprai (died 1161). His composition long passed under the name of Macer Floridus, who was thought to be identical with Aemilius

[1] The works of Hugo de Sancto Victore are printed in Migne, clxxv-clxxvii.

[2] Bernard's "De Mundi Universitate," has been printed by C. S. Barack and J. Wrobel, Innsbruck, 1876.

[3] Charles Singer, "Studies in the History of Science," Oxford, 1917.

Macer, the contemporary of Ovid.[1] It contains over 2,000 hexameters, and is concerned entirely with the action of herbs. Borrowing largely from the Regimen Sanitatis Salerni (*circa* 1100) it is interesting as perhaps the first Western medical work written outside Italy which definitely betrays the influence of the Arabian medical system.

Last among the extra-Salernitan Latin medical works which became available in the first half of the twelfth century was the famous "letter from Aristotle to Alexander the Great," *De sanitatis tuenda*. This spurious letter was translated by Ibn Daud, also called Johannes Hispalensis and John of Toledo (floruit 1135-53). He took it out of the shorter or Western version of the *Secretum Secretorum*, a work that attained the widest circulation. Innumerable MSS. of this epistle are known to exist, and versions and translations of it in every European language were largely influential in the early popularization of Arabian medicine.[2]

We have reached the confines of the period to be considered here. Arabian works were now beginning to penetrate more frequently to the West, and were soon to descend on Europe in a flood which would overwhelm the original system of medicine, the surviving traces of which we have discussed. Before we leave the non-Salernitan writings of the Dark Age, however, we must again revert to Constantine the African, who came to Italy from the East about 1075, and died in 1087. We have dealt with his works in some greater detail elsewhere, and it would be difficult to over-estimate his influence on European medicine in the twelfth century. He is especially important for us here as an interpreter of Arabian medicine who long preceded the great Arabian invasion. His dominating influence on the medicine of the twelfth century may be gathered, for instance, from a library catalogue of 1160, at Hildesheim, that has recently been examined by Sudhoff.[3] From it we learn that the medical works in that collection were almost exclusively the translations of Constantine. Moreover,

[1] There are a large number of editions of Macer Floridus. I have used that of L. Choulant, Leipz., 1832.

[2] A valuable document which, while not itself medical, yet throws some light on medical knowledge during the Dark Ages, is contained in the Bamberg Codex LIII, parts of which are printed by K. Sudhoff in "Eine Verteidigung der Heilkunde aus den zeiten der 'Mönchsmedizin,'" *Arch. f. Gesch. d. Med.*, Leipz., 1914, vii, p. 224. Another glimpse is afforded by the descriptions that have survived of the Hospital at St. Gall in the ninth and tenth centuries. These are summarized and the references to the literature given in E. T. Withington's "Medical History," Lond., 1894, and are reviewed by J. Pagel, "Handbuch der Geschichte der Medizin," Jena, 1902, i, p. 624.

[3] K. Sudhoff, *Arch. f. Gesch. d. Med.*, Leipz,, 1916, ix, p. 348.

he deeply influenced the school of Salerno. Indeed Salernitan documents can be dated from internal evidence according as they do or do not betray knowledge of the writings of Constantine.

(*c*) *Salernitan Literature.*—We now turn to the School of Salerno itself. Its great monument, the Breslau Codex, a twelfth century document containing no less than thirty-five works, was discovered in 1835, and has formed the basis of the most valuable contribution yet made to the history of mediaeval medicine.[1] These works and some others, including the great Salernitan cycle of verses, known as *Flos Medicinae Scholae Salerni*, dedicated to Robert of Normandy, and apocryphally ascribed to one John of Milan, all come within our period. In spite of their importance, we must here pass over them hurriedly, for even a cursory examination of the Breslau Codex, and still more of the Salernitan literature as a whole, would extend beyond our limits. The Salernitan writings, however, have certain common general characteristics, which may be tabulated in comparison or contrast to Anglo-Saxon medicine. But it must always be remembered that not only did Salernitan medicine deeply influence some of the Anglo-Saxon medical literature, but that the two have a common ancestor in Greek and Roman medicine. The divergence arises firstly from the differing native traditions on which the classical material was grafted, and secondly from the relatively greater culture of Southern Italy, where some memory of the ancient learning lingered throughout the Dark Ages.

The Salernitan literature then shows us:—

(1) Great stress on herbal remedies, implying a very extensive acquaintance with plants. The herbal knowledge of Salerno diffused itself over Europe, and most of the modern "herbalism" or folk knowledge of herbs could most probably be traced to a Salernitan source. Some of this herbal lore is embodied in the later Anglo-Saxon medicine.

[1] The documents of Salerno have been printed by Salvatore de Renzi, assisted by S. E. T. Henschel and C. Daremberg, in the "Collectio Salernitana," 5 vols., Naples, 1852-59. Some further gleanings are made by Piero Giacosa in his "Magistri Salernitani nondum editi," 1 vol., with atlas, Turin, 1901. Valuable contributions regarding the relationship of Salernitan to Anglo-Saxon medicine have been made by Max Löweneck (*see* note above); J. L. G. Mowat, "Alphita" in "Anecdota Oxoniensia," Oxford, 1887, i, pt. 2; G. Stephens, "Extracts in Prose and Verse from an old English Medical MS. in the Royal Library at Stockholm," *Proc. Soc. of Antiquaries*, 1844, p. 349; and T. J. Pettigrew, "Observations upon the Extracts from an Ancient English Medical MS.," *Proc. Soc. of Antiquaries*, 1844, p. 419.

(2) A complicated system of weights and measures and a highly developed method of making up prescriptions. In this respect the practice of the Salernitan medicine is very different from the straightforward English method. The Salernitan "Electuaries" may be sharply contrasted with the Anglo-Saxon "Simples."

(3) An interest in what we may call "theoretical" medicine. This displays itself, for example, in a couple of accounts that have come down to us of the anatomy of the pig—an animal selected as being most near in its structure to ourselves. True Salernitan works abound also in physiological speculation, and their authors love to descant upon the relationship of the humours, elements and qualities to the various processes and parts of man's body. Pure Anglo-Saxon medicine concerns itself almost exclusively with remedies.

(4) An emphasis on diet. This is notoriously exhibited by the great Regimen Sanitatis, that long poem to which many of our domestic sayings on diet and regimen may be directly traced. Anglo-Saxon medicine lays more stress on drugs, and pays little attention to diet. Later English medicine, however, borrows freely in this respect from Salerno.

(5) A preference for verse to prose as a medium. This characteristic the Salernitan medicine shares with some of the surviving Latin fragments that have come down to us from the Dark Ages of the art. Anglo-Saxon medicine, however, is mainly in prose, though some quite late specimens are in verse.

(6) An absence of the superstitious element of charms and spells characterizes the Salernitan literature, which in this matter follows the saner Greek tradition. Anglo-Saxon medicine, on the other hand, abounds in these follies, some of which are derived from Apuleius and Marcellus Empiricus, though others are perhaps of genuine Teutonic origin.

(7) A relative neglect of astrology is apparent in Salernitan writings. The belief in the influence of the stars, and especially of the moon and the signs of the zodiac, must be sharply distinguished from the beliefs we have classed under heading (6). However ridiculous astrology may appear to us, by the men of the Dark and Middle Ages it was regarded as a naturalistic hypothesis, as a means of explaining the workings of the universe without the intervention of supernatural powers. The Salernitan literature does indeed follow the universal contemporary practice in accepting the influence of the stars. The early writers of the school of Salerno, however, give far less attention to astrology than

do the majority of mediaeval medical works. Anglo-Saxon medicine, on the other hand, abounds in astrological lore.

(8) The earlier Salernitan works are marked by the absence of Arabic medical terms. After Constantine, however, Arabian anatomical and other terms, some of which have survived to our own time, become progressively commoner. True Anglo-Saxon medicine never uses Arabian terms.

(9) Salernitan medicine stands out peculiarly in the literature of the Dark Period from the fact that it still exhibits a faint and elementary knowledge of the Greek language and the Greek tradition. In this respect it bears some resemblance to Anglo-Saxon medicine, wherein from time to time a Greek word or phrase is unexpectedly encountered. In most of these points the twelfth century Compendium, which we here print, shows its affinities to the early Salernitan literature, from which indeed it contains definite extracts. It exhibits also, however, certain relationships to the Anglo-Saxon literature.

(II) The Scientific Encyclopædia of the Twelfth Century contained in MS. 17, St. John's College, Oxford.

This MS. is of folio size, the pages measuring 13 in. by 10½ in. It consists of 177 parchment leaves, containing a large number of diagrams in colours and a few pen-and-ink drawings. There are no miniatures or illuminations in the proper sense of the word. The text of the whole work, where it is not from Bede or other known writer, is in Latin of a barbarous type. The entire volume is in the same hand-writing (*see* fig. 7), and can be dated by certain of its contents. Thus on folio 3 verso we read :—

> Ab adam usque ad diluuium sunt anni ii.ccxlii.
> A diluuio usque ad abraham. dccccxlii.
> Ab abraham usque ad natiuitatem Xri. ii.xv.
> A natiuitate Xri usque ad presens tempus. i.cx.
>
> From Adam to the Flood are 2,242 years.
> From the Flood to Abraham 942.
> From Abraham to the birth of Christ 2,015.
> From the birth of Christ to the present time 1,110.[1]

[1] This Table is a reminiscence of the "aetates seculi" of Bede in his "De temporum ratione."

Further evidence of date is yielded by another part of the MS. (folio 29 recto), where mention is made of the Council of Gloucester in 1085, of the ordination of one Gunterius as Bishop of Lincoln in 1086 and of the death of King William Rufus in 1100. There is also a story (folio 29 verso) of how in the year 1111 two friars of Winchester conveyed the relics of St. Athelwold[1] consisting as we are told of his backbone—"*spinedorsi*"—to Thorney Abbey. All these are told as current events; and there can be no doubt that the MS. was written in England in the early years of the twelfth century, probably between the years 1110 and 1112.

The volume contains about sixty items, of which we need here only discuss a few of the more important. The various items are designed to include all knowledge other than theology accessible at the time. It is essentially a scientific encyclopædia, and is perhaps the earliest modern work of its kind that has come to light. It is of value as giving us a glimpse of the range of physical knowledge in England in the first decade of the twelfth century.

The Medical portion begins on the verso of the first folio, and covers also the second and the last three folios. These all show signs of having been bound in after the rest of the volume had been made up. The folios in question are, however, in the same hand-writing as the rest of the volume, and do not form a mere fly-leaf pharmacopœia such as is not infrequently encountered in old MSS.

Among the more interesting items in the volume are the following, which give a general idea of its contents:—

On lucky and unlucky days according to the influence of the moon (folio 3 verso).
A runic alphabet (folio 5 verso).
A map of the world (folio 6 recto).
A table of consanguinity (folio 6 verso).
A classification of knowledge (folio 7 recto).
A diagram *De Concordia Mensium atque Elementorum* from Byrhtferðus, monk of Ramsey (folio 7 verso).
A number of astronomical tables and calendars scattered through the volume.

[1] Perhaps Athelwold the younger, who was Bishop of Winchester, 1006-15. In the twelfth century the monks of Abingdon professed to have some of his bones. He is said to have written a treatise on the circle, a MS. of which is now in the Bodleian Library MS., Digby 83, folio 24.

Proemium Brihtferthi Ramesiensis Cenobii Monachi Super Bedam De Temporibus. Spiraculo ineffabili dum forent (folio 12 verso).[1]

Argumentum antiquorum de luna & de mensibus of Bede (folio 36 recto).

On the positions of the seven planets (folio 37 verso).

On the art of arithmetic from Boethius (folio 50 recto).

De natura rerum of Bede (folio 62 verso). This is a very good text but wants the first fifteen chapters which occupied two folios that have been cut out.

Jerome on the fifteen signs of the fifteen days preceding the day of judgment (folio 159 verso).

A list of etymologies (folio 159 verso).

Rules of grammar (folio 168 verso).

Rules of prosody (169 recto).

A brief discussion of the knowledge involved in some of these various items will enable us the better to estimate and place the medical compendium itself.

There can be no doubt that the writer was Saxon, and not Norman. Not only does he show great interest in the national hero, Bede, but he occasionally quotes Saxon words, using the Early English script. Thus, in the course of a calendar we read concerning the month of May (folio 18 recto) "anglica uero lingue iste mensis drymilce uocatur."[2]

A more unexpected feature of the MS. is that it betrays a vestigial acquaintance with Greek. There are several Greek alphabets scattered through the volume, and in addition the writer shows the same fantastic interest in derivation from the Greek as was exhibited by Isidore of Seville. Thus he tells us that the name of the month April is derived from Aphrodite. "Aprilis per uenerem dominatur quasi afrodis grece uenus dicitur."

A large number of the names of herbs, being derived from the Salernitan pharmacopœia, are of Greek origin, and these, like many Early English plant names, can be traced back to Dioscorides. In a few passages, however, medical phrases are used which suggest a more intimate degree of contact with Greek sources than is usual at the date of our MS. Thus, in the section on Bloodletting, we read:

[1] K. M. Classen, loc. cit., believes that this work is not by Byrhtferd of Ramsey. Classen is, however, under the impression that it was first attributed to him only about the year 1406 by John Boston, the monk of Bury, who examined the Anglo-Saxon writings in the libraries of the monasteries in this country. Cf. Tanner, "Bibliotheca Britannico-Hibernica," London, 1748, Preface p. xxx (a reprint of Boston's Catalogue) and p. 125. The *incipit* quoted by Tanner, however, is: "Spiraculo vitae humanum genus," and therefore does not quite correspond with that of our MS.

[2] Cf. Bede, "De temporum ratione," cap. xv, "De mensibus Anglorum."

"Inciditur autem de flebotomo optimo rectam percussuram *catatixin*, habet hec est in iussum primere flebotomum rectum et sursum leuare. Quod si male incisa fuerit collectionem in altum facit et uulnera insaniosa facit. insaniaque plurima et spissa nutrit uulnera. et deducit ad omnem perniciem." The passage recalls one in the Galenic writings :—

"*τὸ δὲ κατ᾽ ἴξιν ὅτι κατ᾽ευθὺ λέγει, πᾶσιν ὡμολόγηται σαφῶς αὐτοῦ κεχρημένου πολλάκις ἐπὶ τοῦδε τοῦ σημαινομένου τῇ κατ᾽ ἴξιν φωνῇ. τὰ δ᾽ ἀνάπαλιν αἱμορῥαγοῦντα μηδὲν ὠφελεῖν ἢ καὶ βλάπτειν ἐνίοτε τῷ καταλῦσαι τὴν δύναμιν ἄνευ τοῦ τὸ πάθος κουφίσαι.*"[1]

Our author's *catatixin* is clearly the *κατ᾽ ἴξιν* of Galen, that *recta percussura*, the neglect of which may lead to such grave results.

Other terms reminiscent of Greek medicine are *anastomo, plagiotimo, anacarsis, malanma, dissentericus, emotoicus.* A few words of this type are scattered through the Salernitan literature from which our author certainly borrowed. The most used of these Greek terms is the word *cephalica*, descriptive of the vein in the arm from which blood is to be let for diseases of the head. This term is one of the most persistent in the medical vocabulary, and comes to us from Galen, having been used in every subsequent age.

Occasionally our author himself explains a Greek derivation, "Eliotropium id est intuba a grecis siue solsequia uel sponsa solis." A few phrases in our MS. are actually in the Greek script, as for instance, the legends in the circle of Petosiris (fig. 1). This well known and widely spread diagram is an absurd device that was used to decide a sick man's chances of life or death.[2] It probably came into Europe with that bastard offspring of Greek and Oriental thought known as the "hermetic writings"; but at any rate it was very popular in the Dark Ages, and a similar use of a few Greek words is to be found in several contemporary Anglo-Saxon MSS.[3]

In a MS. of 1110, when England had already been under Norman influence for more than a generation, a certain amount of French

[1] Galen, "De curandi ratione per venae sectionem," cap. xv; Kuhn's edition, xi, 295, 296.

[2] The application of this device is explained by Hugo Magnus "Der Aberglauben in der Medizin," Breslau, 1903. Translated as "Superstition in Medicine" by J. L. Salinger, New York, 1905.

[3] The circle of Petosiris with the legends written in Greek is encountered, for example, in two Anglo-Saxon eleventh-century MSS. in the Cotton collection at the British Museum, viz., Tiberius A iii, folio 34 verso to folio 35 recto, and Caligula A xv, folio 121 verso to folio 122 recto.

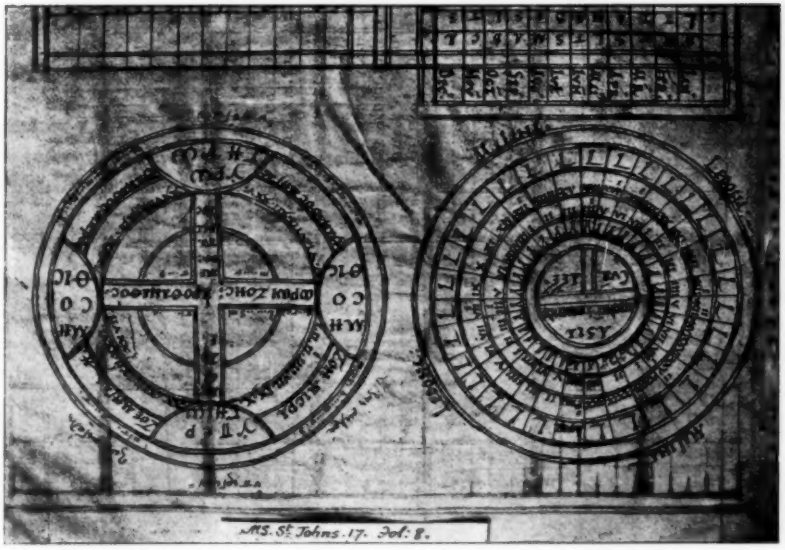


FIG. 1.

Astrological Circle and Circle of Petosiris from MS. St. John's 17, folio 8 recto.

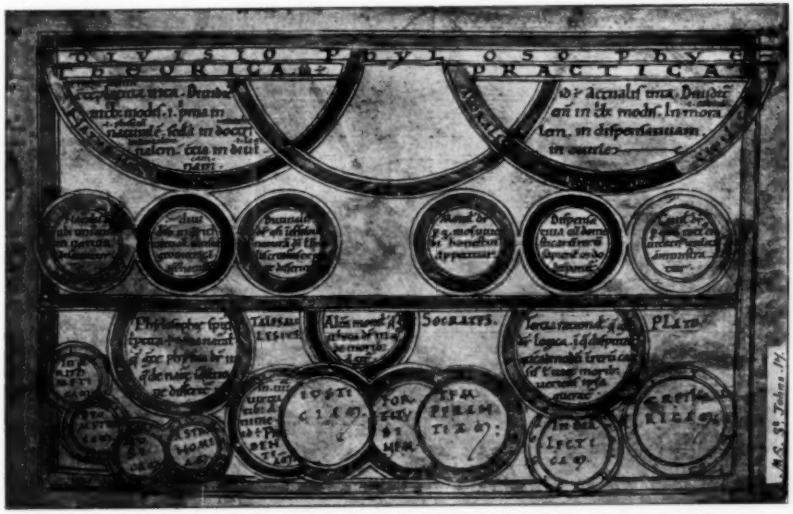
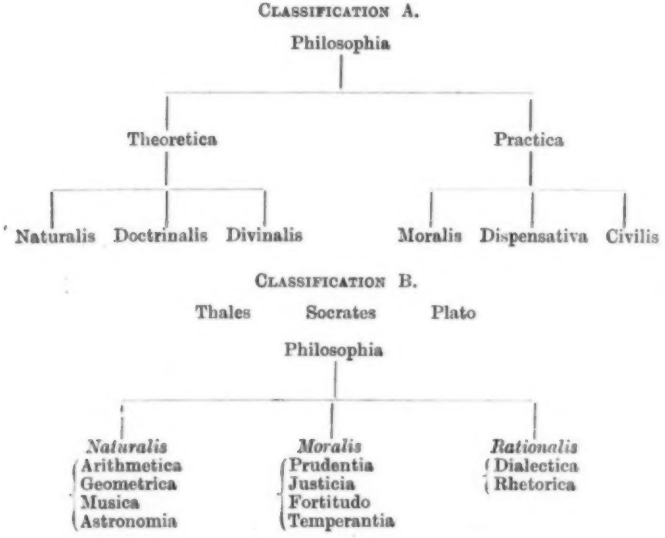


FIG. 2.

The double Classification of Knowledge from MS. St. John's 17, folio 6 verso.

influence might be expected; nevertheless this linguistic element is almost entirely absent, and can only be traced in one or two plant names such as pouncel [O. Fr. poncel = poppy], and the use of the word falicile (modern French falaise) for a rock or cliff.

Leaving the linguistic aspect, and turning now to the subject matter of our encyclopædia, we find that early in the volume is a classification of knowledge, or "philosophia" as our author calls it. Attempts of this sort are always instructive as betraying their authors' range of ideas (*see* fig. 2). In this case two methods of classification are given :—



It is with the group of "natural philosophy," together with some of the elements of "rational philosophy," that our encyclopædia mainly concerns itself. For the purpose in hand, however, we shall only deal with the purely physical science of the work.

The method of mediaeval *philosophia naturalis* presents a striking contrast to what we nowadays regard as science. The essential difference appears to us to lie in the use of *analogy* as an intellectual weapon. A modern scientist habitually uses analogy as a means of attaining truth and a guide to experiment, but he never adduces an analogy as a proof of his conclusions. In setting forth his results, indeed, he usually emphasizes his inductive proofs, and thus buries deep

among the débris of his abandoned working hypotheses the memory of the analogical processes that he has used. This was far from the case with the mediaeval natural philosopher. He started out with the idea that the universe was built on a systematic plan, of the broad meaning of which he believed he had the key. And he held also that he had something more than the key, for he believed that a part of the plan itself had been specially revealed to his forebears. Therefore the investigation of the remainder, on an analogical basis, should yield results similar to that of the already known part. If this proved not to be the case, the fault might lie with his application of the principle, but not with the principle itself.

The effect of this view in the strictly theological field is manifest, and is well known. But turning to "philosophia naturalis" proper we will illustrate the use of the method by one particular instance. The mediaeval scientist knew from the ancients that there were four elements and four humours. What more reasonable than to suppose that there were four principal organs or parts of the body to correspond? There were four seasons of the year—must there not then be four ages of man? Again, the twelve signs of the zodiac were known from of old to influence the parts of man's body, and the twelve signs arranged themselves obviously into four groups in relation to the four points of the compass.

Thus developed a whole mass of belief on the subject of what we will call *the physical and physiological fours*. The lore of the fours is a characteristic product of mediaeval scientific thought, and is but one instance of the tendency to an excessive use of analogy. Many examples of this method occur throughout our Encyclopædia and the subject is beautifully illustrated and summarized with its bearing on medicine in the accompanying diagram (fig. 3), which is described in the text thus: "Hanc figuram edidit byrhtferð monachus ramenensis cenobii de concordia mensium atque elementorum."[1] The reader can easily unravel for himself the concord of the months and of the elements, as worked out by Byrhtferth of Ramsey. The same relationship is expressed more simply in fig. 4. This latter diagram is encountered in MSS. of the ninth century and earlier, and was popularized by Isidore of Seville, and something very like it may be found in certain early MSS. of Macrobius.

[1] This figure most fortunately provides a copy of a page missing from Ashmole 32 B, a unique Bodleian Anglo-Saxon MS. with some Latin paragraphs, dating probably from the early years of the eleventh century, known as "The Handboc of Byrhdferð" (see C. and D. Singer, *Bodleian Quarterly*, 1917).

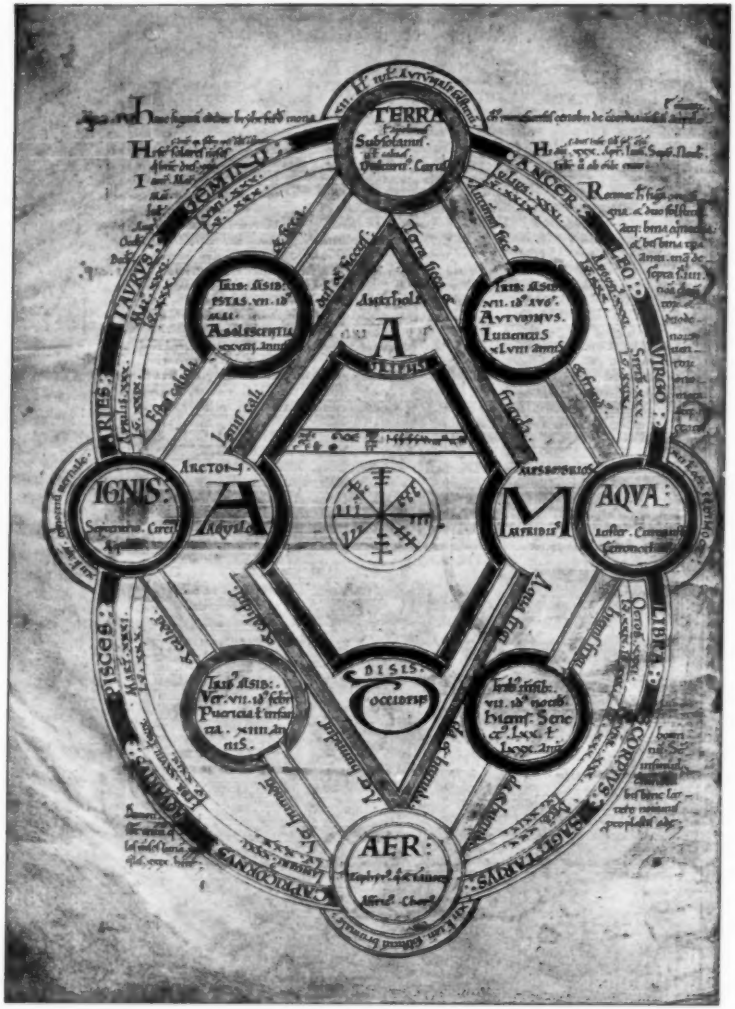


FIG. 3.

Byrhtferth of Ramsey's diagram of the Physical and Physiological Fours from MS. St. John's 17, folio 7 verso.

Turning now to the teaching of our MS. On the actual structure of the universe we find a geocentric system, the earth being surrounded by the seven spheres of the seven planets, each planet moving independently in its own sphere. The general conceptions both of astronomy and geography are here derived either from Bede or from Macrobius, from whom a considerable passage is quoted.[1] Macrobius is one of the very few pagan writers whose works were accessible in the Dark Age, and we find him extensively used as early as the ninth century by John Scotus Eriugena (810-75).

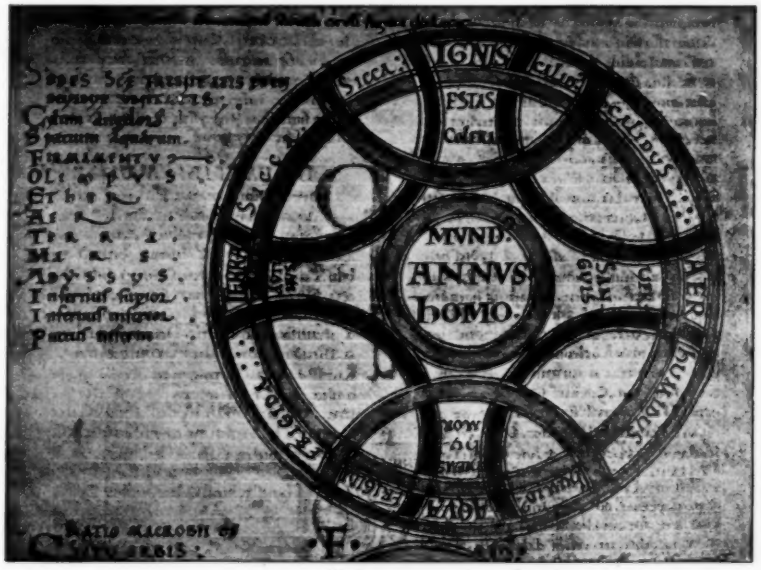


FIG. 4.

Diagram of the Physical and Physiological Fours from MS. St. John's 17, folio 39 verso.

The earth itself, following the majority of mediaeval writers, our author regards as a sphere. He knows also of the obliquity of the ecliptic. Of the "five circles of the earth" itself, he tells us that

[1] A quotation from Macrobius, which occurs on folio 39 verso is from the Commentarium in Somnium Scipionis, Lib. I, cap. xxii, 11 to 13 (Teubner's edition by F. Eyssenhardt, Leipz., 1868, p. 569).

"the Greek philosophers call these parallels, i.e. zones, and thus they divide the orb of the earth. . . . Of these, two, the *Septentrionalis* and *Australis* are uninhabitable because of cold. Two are tempered between heat and cold, and therefore are habitable, and these are the *Solstitialis* and the *Brumalis*. Now we are said to be in one of these and the antipodes in the other and [between them] there is the *Æquinoctial* zone, burnt up by great heat and therefore uninhabitable."[1] Our author endeavours to present these facts to the reader by means of a diagram (fig. 5) which is confusedly represented in something of the nature of perspective.



FIG. 5.

A diagram of the Zones or Climates from MS. St. John's 17, folio 40 recto.

Before we turn to the purely medical section of our MS. we may refer to the map of the habitable part of the earth's surface (fig. 6). This is represented as circular and thus is not in keeping with its description as a zone elsewhere. Such inconsistencies are not infrequent in the science, medical and other, of the Dark and early Middle Ages. With the advent of the true scholastic spirit, especially in the thirteenth century, all science gradually became levelled up and standardized to

[1] Folio 40 recto.

the supposed teaching of Aristotle. At the date of our MS., however, the amount of freedom was much greater.

The map presents many points of interest on which we may not dwell. Britannia, Hibernia, and Thule are represented beyond the rest of the habitable world far away to the left or north—the east being at the top of the map, as is usual in charts of this date. Jerusalem is

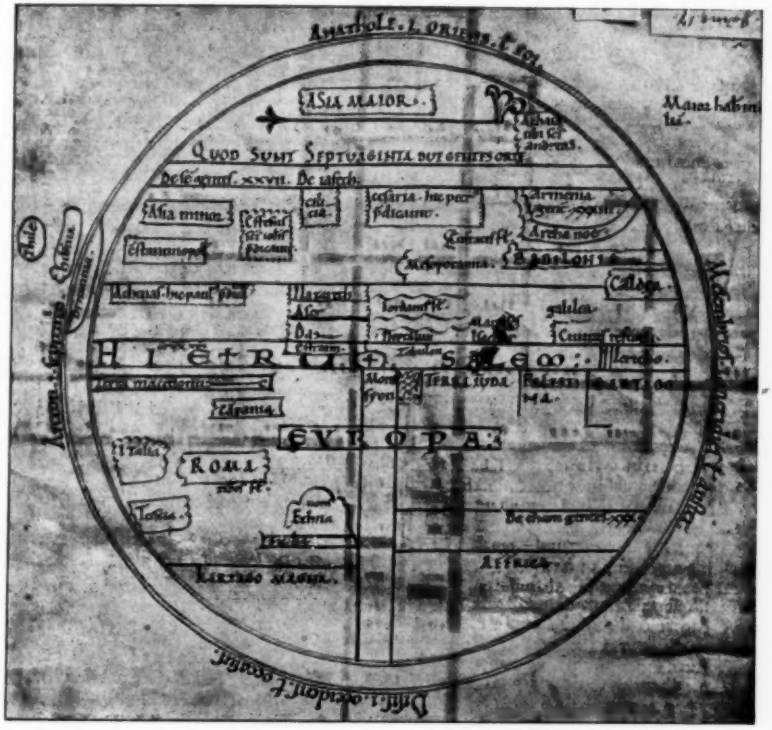


FIG. 6.

A map or diagram of the World of the so-called OT type, from MS. St. John's 17, folio 6 recto.

placed in the very centre of the habitable earth, and Noah's Ark still finds a resting place between Babylon and the thirty-three Gentes of Armenia, while Constantinople and Athens are but a cross-channel trip from England.

(III) Text of the Medical Compendium, being a Section of the Encyclopædia.

The medical compendium divides itself naturally according to subject into a number of chapters. These are indicated in the MS. by new paragraphs. There are no headings except in the case of the section on blood-letting. For the reader's convenience we have supplied headings to all the sections, and have broken up the sections into paragraphs. Folio 2 has been so bound that a number of the words on its inner margin are now illegible. In some instances we have been able to fill in the hiatus, and in a few cases we have rectified the text itself. All such corrections we indicate by enclosure within square brackets. We have not, however, attempted to amend the sometimes very surprising Latin. The identification of Greek plant names is the work of Mr. W. H. Stevenson, of St. John's College, Oxford. Mr. Stevenson has also kindly corrected some readings and made suggestions for others. The subject matter of the compendium may be divided into the following chapters:—

(1) On the constitution of the body from the four humours.
(2) On the regimen of the four temperaments.
(3) On blood-letting.
(4) On the prognostication of life or death.
(5) A glossary of herbs.
(6) A table of weights and measures.
(7) Recipes for electuaries.
(8) Recipes for plasters.
(9) An Anglo-Saxon charm to stop nose-bleeding.
(10) On the relation of fevers to the humours.
(11) On the principles of pathology.
(12) Panaceas.
(13) Alphabetical list of remedies, with the diseases for which they are appropriate.
(14) List of diseases, with their appropriate remedies.

(1) *On the Constitution of the Body from the Four Humours.*

[folio 1 verso, column *a*]

Hi quattuor humores dominantur in suis locis. Sanguis dominatur in dextro latere; in epate quod iecur uocamus. Aliter in corde. Colera rubea in eodem latere ubi uesica plena est felle hoc est in dextro;

[Colera] Nigra in sinistro quo sunt [s] plene. Fleuma autem in capite scilicet una pars. altera in uesica.

Sanguis enim feruens humidus & dulcis. Colera rubea; amara. uiridis. [Colera] Nigra; acida & frigida. Fleuma; frigida. salsa. & humida.

Haec omnia igitur essent suis temporibus. Sanguis uere. Colera rubea. aestate. Colera nigra; autum[n]o. Fleuma; hieme.

Hi uero humores habent respirationes per singulas partes corporis. Sanguis per nares. Colera rubea. per aures. Colera nigra; per oculos. Fleuma; per os.

Quattuor hi humores. mores tales faciunt. Sanguis facit hominem bonae uoluntatis. simplicem. moderatum. somno plenum & crassum. Colera rubea [facit hominem imperterritum. an iustum. macilentum. benemanducantem et cito digerentem. Colera nigra][1] facit iracundum. auarum. tumidum [? for cupidum]. tristem. inuidiosum. sepe habentem in pedibus cicatricem. Fleuma. facit compositum corpore. uigilantem. in se infra cogitantem. canes [probably for canities] cito producentem.

Corpus. iiii. habet in se qualitates. &. iiii. rebus consistit. ossibus. neruis. uenis. carne. Continet in se frigidum. calidum. humidum. & siccum. Et enim. iiii. habent diuersitates. Esurit. sitit. concupiscit. soporatur. &. iiii. sunt usus naturae. manducare. bibere. generare. dormire.

(2) *On the Regimen of the Four Temperaments.*

[folio 1 verso, column *a*]

Itaque iubemus eos quibus colera [rubea] dominatur uerno tempore flebotomare. & autumno corpus purgare. Nulla enim res melius est quam purgatorius bonus & optimus. Talibus purgari expedit. Diagridii silis. iii. turis masculi silis. ii. aloene petrosilis. iii. euforbii silis. ii. Hec omnia tritis & cum aqua calida dabis ieiuno bibere hora matutina propter humores noxios deponendos. Fit hec de albis rubeisque coloribus.

Nigra uero [colera] ex quibus melancolia nascuntur que uarietatem faciunt in corpore. uulnera intrinsecus nutriunt. & indigestiones faciunt. His nulla res melior est quam ut abstineant. & hec talia comedant suis temporibus. Carnes berbecinas. de sale. Iubemus non comedere caprinas. non leporinas. non porcinas. quia melancoliam nutriunt. insuper porcina caro uenenum melancolicis est. & stomachum graue facit. Bouinam carnem expedit sumere. Purgatur melancolicis. Dagridii silis. ix. agarici silis. ix. cassie & fistule silis. vi. reopontici silis. iiii. Hec omnia tritis & cum aqua calida ut supra.

[1] We have inserted this passage from a text attributed to Giovanni Monaco and printed by S. de Renzi, loc. cit., ii, p. 411. The insertion is necessary to make the context intelligible.

Sanguis est calidus. humidus. & dulcis. Fleuma ; salsa. acida. & sicca. Sicca maligna humoribus. Inter humores & sanguinem. xii. libræ sunt in corpore hominis. medietas sanguinis. & medietas humoris. Si sanguis superfluus humori fuerit facit elefantiosum. Porro quando hoc sentit inequali corpori debes succurere. At si humor supererit sanguini ; ydropicum facit. Oportet igitur ut adhibeat purgatorium corpori. aut flebotomum. ideo [folio 1 verso, column *b*] ut inequalitas corporis compositionem salubritatis [restitu]at. Quod si non fecerit languoribus grauissimis laborabit. . . .

Oportet medicum medicinam corpore adhibere. . . Nam idus caniculares dies. Quos [? for id est canicularis dies quo] non iubemus [ullam man]sionem. neque curam corpori adhibere. nisi infirmita[tes] aduenere. in initium febricitanti uel pletoricis [iu]bemus ut medicus omnem curam flebotomiae. uel . . . -nem quam explorauit a magistro suo faciat. ceteras. . . -res presumat. quia dies sunt caniculares.

Porro a pri[mo die Iulii]. ingrediuntur usque in idus augusti que sunt dies. xxx. [quaside] & subsolanus uentus. id est uulturnus dominatur. Addiu[ntur]. . . dies qui faciunt in unum dies. xlix. A quinquages[imo]. . . omnem curam corporis expedit medicum curare. . . medicorum fuere. qui non aspexere. xix. dies neque parti. . . neque per ingenium. morti haud potuere resistere. [Ab] autem septembri usque primo kalendas decembri medicus cautelam habere debet.

(3) *On Blood-letting.*

[folio 1 verso, column *b*]

[B][1] De Flebotomia. Si necessitas fuerit omni tempore adhibendus est fleobotomus. tamen precipue ab. viiii. kalendis aprilis usque in [primas] kalendas iulii. Tunc est utilitas detrahendi sanguinem. quia tunc sa[nguis] augmentum habet. Sed postea obseruationes sunt temporum & q[uali]tates. cursusque lunae obseruandae. Hoc est .v. Luna [x. xv. xx.] xxv. & xxx. In his non oportet flebotomum inp[onere] quod in his diebus quos medici quinnones uocant. haud fieri [flebotomum] oportet. Et quod multi medicorum affirmant quod [infinitus] numerus ob negligentiam non obseruantium qualita[tes temporum] & cursus lunae mortui sunt.

[B] Nam tamen super omnia obseruandum est ut a. xv. kalendis augusti usque. Nonas. septembris quos caniculares dies. pro stella que canicula uocatur dicti. neque potionem neque flebotomum debet aliquis habere . . -dium quia hi humores mixtis. Solum si ictus peruenerat [quod] non oportet nisi ut aperiantur uene & uastitas iniquitas [quidem] sanguinis. Euaporetur esset ut nullomodo hic facere oportet.

[1] The paragraphs marked [B] are almost identical with passages in the work "De Fleobotomia" attributed to the Venerable Bede.

[B] Potio uero autumno tempore ab. viii. kalendis octobris usque in. viii. kalendas decembris utilis est. & quod per hiemem & per estatem in fleuma & colera [corpus] contraxit. In autumno enim debemus adhibere purgatorium fleuma. & nigra colera pessima scilicet quod nos melancolic[am ap]pellamus. Eo tempore uetusta fleuma. & rubea. & nigra. eo [in] super hieme crescit. utatur quod oportet. uel de medicis . . . aut de obseruantia ciborum abstineat. de colera [nigra] curus esse potest usque in alium autumnum.

Quantas uires prostet flebotomia. edicamus. Sanitatem continet in se. sincerat mitem. prouidet memoriam. purgat uesicam. cerebrum exuocat. calcifacit medullam. abditum aperit. lacrimas stringit. fastidium tollit. stomachum propic | [folio 2 recto, column *a*] ciat. dignitatem inuitat leuem. nocem producit. sensum construit. uenerem cohercet. somnum fugat. anxietatem decrescit. primum sanguinem nutrit. extraneum abicit. longiorem uitam administrat. bonam perseuerantiam facit. omnes incautissimas collectiones remediat. acutas febres. ac reumaticas passiones. pestiferasque ualitudines pellit.

Uerum conuenit flebotomare illum qui laborat in acutis passionibus propter lesionem capitis uel infusiones oculorum de cefalica uena. Nam ipsa habet principatum in capite.

[B] Quid est flebotomia; Venae recta incisio. sanguinisque emissio. Quibus de locis flebotomari debet; De. xx. & tribus. Videlicet de arteriis. ii. in occipiti quas propter querelam capitis incidimus usque ad ossum mensura. iiii. digitorum ab aure; de angelogiis. ii. in ambobus timporibus propter effusionem oculorum. De sullingua. ii. propter reuma gengiuarum. uel uitia oris & dentium. Medio de fronte. i. propter dolorem capitis uel alienationem frenesis. De naribus. i. propter grauitatem capitis. De collo. ii. propter abundantiam humorum in capite. uel in oculis aut gengiuis. De manibus. iiii. duas secus digitum pollicem propter inflationem pulmonis. & alias. ii. secus digitum minimum. propter inflationem splenum. & dolorem dentium. Si dentes dextri doluerint in sinistra manu. & si dentes si nistri] doluerint in dextra manu in minimo digito in medio iuncturae trium. De lacrimis. ii. secus nares dextra leuaque propter accessiones humorum in oculis de sub talo pedum. propter algorem ipsius pedis uel nefreneticos. aut sciaticos. Sic multae cuius menstrua subtraxerit. uel non concipit. Super digitos pedum. ii. propter indignationem testium. In brachio incidimus uenas. iii. cefalicam. mesaon & epaticam. Cefalica & epatica. anacarsi utimur ad ambas id est a latere. in foris fleobotomamus. raptim meson catatixim. i. iusso primere flebotomum rectum & sursum leuare.

Quibus est locis ponenda. quando flebotomamus: Cefalica uena est que habet a capite principatum & ponitur super munusculum [for musculum] inciditur autem de flebotomo anastomo uel plagiotimo feritur anacarsis anastamus. a suso incidens flebotomamus in latus. aut incuruum hic habens acumen. Quod si vena ipsa male incidatur. aut

musculus qui sub uena est percutiatur subitaneum [facit] tumorem & per negligenciam ipse neruus soluitur uel frigidat. mortemque adducit. Mesa uena hec est mediana que a pulmonibus habet principatum. ponitur in medio brachio super altitudinem. Ponitur autem uacua inter

[folio 2 recto, column *b*]

| duos neruos. Inciditur autem de flebotomo optimo rectam percussuram catatixin. habet hoc est in iussum primere flebotomum rectum. & sursum leuare. Quod si male incisa fuerit. collectionem in altum facit. & uulnera insaniosa facit. insaniamque plurimam. & spissa nutrit uulnera. & deducit ad omnem perniciem. Epatica uena est defecatu que a [b e ⌈pate habet principatum. ponitque in extremam partem brachii. super neruum cui non conenit catatixin. scilicet plagiotimum uel anastamum. Quod si uena ipsa male incidatur. aut neruus ipse percutiatur humor exiens ut glanlosa [gladola, gloss in a later hand]. lutosa. carnium aut humarum [? for humanum or humarem] melitum facit & iam neruorum uel digitorum manus cunctractionum. Quibus pro causis flebot[omo] utimur cefalicam uenam incidimus; propter capitis causas & suffusiones oculorum. uel propter impetum atque magnum tumorem accidorum. Mesam uenam incidimus propter causas pulmonum. id [est] lismum & asmatiam apoferisis plurimum facientes. Epaticam uero incidimus propter eparis dolorem uel stomachi uel pleoriticos. Faciamus igitur rationem secundum causam quam proficiat scriptura nostrae medicine. uel quod sit mensura flebotomi uel quid prodest.

Dicimus secundum uires. secundum tempus. secundum emutationem colere. secundum lepotismiam imponderis [?] uel si pulsantur dolorem ponderis. Nam si pulsatur leue pondus aut dolor uel uomitum. aut ruptum ipse sanguis appendebit usque ad librem. ii. aut iii. secundum aetatem puerilem. uel senilem. Senibus minus demimus. iuuenibus paulo amplius secundum uires. Igitur si tardaueris in egritudinem ueniet. aut ex nimia uentris solutione desicamus [? for dessicatus] erit. aut cibum non accipiet. uel nimiam febrem capiet propter nimium calorem. uel frigus; oportet uerno tempore. flebot[omum] adhibere. quia equalis est aer tamen non conuenientibus in locis. Secundum emutationem colere. Si sanguis in principio exieret niger. usque ad rubicundum deme. Quod si spissum uel crassum. ad tenuitatem aquosum usque ad crassitudinem. Secundum uel epocissimam hoc est usque ad malefactionem & debilitationem stomachi. facit hoc esse propter cautelam quam haud scimus agere. Nam emissio sanguinis infrigidat. Inde mala surgunt. ex fleuma. uel colera conclusa in stomacho. non habent exitum suum. uel ex cruditate ciborum uel potuum. sit hec debilitatio stomachi. plurima emissio sanguinis. Quum si sanguis proprius tollitur. ledit. Alienus si tollatur; iuuat. Quomodo subuenimus in lepotismia [? for flebotomia]? Si hiemis tempore fuerit calida aqua faciem fouemus uel plantas. Si estate. frigida aqua faciem

& plantas fouemus. uel uomitum prouocamus. & naribus odora opponimus optimum. pulegium. mirtam. sisimbrium. aceto perfusum lateri circumdamus. Quomodo adhibebemus diligentiam flebotomie? Cephalica uena per incisionem si intumuerit. oleo & aqua fomentabimus. Si hieme; calida. Estate; frigida. & emplastrum diaquilon imponimus.

[folio 2 verso, column *a*]

Quod si mediana intumuerit. bete folia tritet. panemque infusum in rosatio cataplasmamus. Quod si epatica. fouebimus ex calida ubi caxerimus alteum.[1] & emplastrum rubeum. quod accipit mellilotum. senopiditem. colophonia. cera. & oleo myrtino. Sane flebotomiam cum incisionibus atque cautela operare. debemus quoniam si aliqua uena perincisa fuerit. non conglutinatur neque sarcinatur. Inde cum cautela fieri oportet.[2]

(4) *On the Prognostication of Life or Death.*

[folio 2 verso, column *a*]

Prognostica uera e libro galieni. Mortiferum signum est. cum in corpore humano frons ruit [? for rubet]. supercilia declinantur. oculus sinister minuitur. nasi summitas albescit. mentum cadit. pulsus autem currit. pedes frigescunt. uenter defugit.[3]

Iuuenem uigilantem. & senem dormientem. si uideris; hec sunt mortifera signa.

Prognostica ad omnem egritudinem. ut intelligas si uiuere habet homo aut mori. Si testiculi infrigidauerint. & contraxerit natura. uel testiculi ambo absconsi fuerint; signum est mortis.

Ad malum malannum [? malum anum or malagnum]. Accipe dolsam radicis da ei bibere in aqua [?] benedicta. Si ei anus indoluerit: morituri sunt. Si non. uiuere.

Ut scias si possit uiuere infirmus. Fermento manus eius illimas. postea da cani manducare. Si manducauerit; uiuet. Si non; morietur.

[1] = althea, ἀλθαία

[2] Marginal gloss to this paragraph in same hand as text: Ad eos qui venantur & mentis suas non habent. herba gentina cum aceto & semen rutae da bibere.

[3] Marginal gloss to this paragraph in same hand as text: Ad stomachi tumorem. absinthio man. pl. ii. ruta. man. i. eoque in vino & bibe.

(5) *A Glossary of Herbs.*

[folio 2 verso, column *a*]

Nomina herbarum.
Sarminum. id est cerfolium [*χαιρέφυλλον*].
Lupopectina. id est carduus magnus [carduus].
Blandona. id est moledonum [*μολύβδαινα*].
Ypiricum. id est triscalanum [*ὑπέρικον*].
Subtussella. id est petrosella.
Agaonem. id est cornudella [*ἀειζωον*].
Polipodiam [*πολυπόδιον*].
Caliadam [*? κολίανθρον*].
Opanicum. id est ius de panaco.
Litargiriam. id est spuma argenti.
Ratilia. id est tornella [tormentilla].
Mastice. id est album incensum.
Iusquianum. id est canicularis [*ὑοσκύαμος*].
Felterre.
Arnegalica. id est conferia [*ἀναγαλλίς* = comfrey].
Scolastica. id est reomacina.
Scitidata. id est ambrosia.
Basilica. id est troforata [*βασιλικόν*].
Radix ortalana. id est raphanum.
Papaver. id est pouncel [O.Fr. poncel = poppy].
Sappa. id est longus sauus.
Anagalon. id est consolida maior [*ἀναγαλλίς*].
Monoglonos [*μονόκλωνος*].
Irius. id est gladio uel ortalanus.
Tragantum. id est mater herbarum [*τραγάκανθα*].
Avellana. id est nux maior.
Panactum. uel regium. id est radix liuestici [*πάναξ*].
Cimion. uel codion. id est cicuta [*κώνειον*].
Ermendactila. id est alnus agrestis [hermodactylus—i.e. Ilium agreste]
Ylion. dolor intestinorum [ilium].
Glesides. id est piona [*γλυκυσίδη*].
Pergomon. id est ruta [*πήγανον*].
Puleius minor.
Serpillum.
Origanum. id est puleius maior [*ὀρίγανον*].
Pentafilon. id est quinquefolium [*πεντάφυλλον*].
Meliolata [*μελίλωτον*].
Calamine. uel calamonis. id est nepta [*καλαμίνθη*].
Basamita. id est sinsinbrium [*σισύμβριον*].

Magisca. id est musica minor.
Alosantum. id est boletus maior.
Scolopendria. id est splen uel cerui lingua [*σκολοπένδριον*].
Arcantilla. id est apiata.
Ebrelilfan. id est frisgon.
Glicanum. id est puleius ortensis [*γλήχων* = pulegium].
Artemisia. id est mater herbarum [*ἀρτεμίσια*].
Ditamen. id est salgemma.
Dunima. id est tanaceta.
Bariona. id est iouis barba.
Tuuus. id est toruis filcis (?)
Asarum. id est radix uulgagum [*ἄσαρον* = vulgaginis].
Presterion. id est berbina [*περιστέριον*].
Timbra. id est satureia [*θύμβρα*].
Gamitruis. id est gamandreia [*χαμαίδρυς* = germander].
Politritum. id est capilli ueneris [*πολύτριχος*].
Oximelle. id est acetum cum melle [*οξύμελι*].
Blandona.
Vapum.
Cepa.
Plantago.
Aglosa.

(6) *A Table of Weights and Measures.*

[folio 2 verso, column *b*]

Calculus est ciceris grana. v. & est. iiii. pars oboli.
Duo oboli. faciunt scripulum.
Tres scripuli. dragmam complent.
Dragma & scripulus & obolus. quadrantem faciunt.
Duo quadrantes. stateram faciunt.
Duae staterae unciam reddunt.
Unciae. xii. libram. efficiunt.
Librae. lii. talentum.

De Liquidis.

Coclear habet dimidiam dragmam. i.
scripulus & obolus cochlearia. iii. condam faciunt.
Conde. iiii. ciatum reddunt.
Cyati. ix. eminam faciunt. uel minae duae. & semis.[1] complent.
Cyatus. dragmas. x. sextas medicinales; Vncias. x. habent.
Siliqua sexta pars denarii.

[1] The MS. has "ss." As semis is usually represented by "s," this is probably a slip for "& s."

scripulus. hoc est denarius.
Obolus habet siliquas. iii.
Dragma habet denarios. iii.
Didragma sunt. ii. dragme.

Coclear pensat denarius. i. &. semis.
ciatus pensat Dragma. x.
Cotila habet ciatos. vi.
Emina [habet] libram. i.
Sestarius de uino aut de aqua pensat libras. ii.
Sestarius olei pensat libram. i. &. semis. & uncias. ii.
Sestarius mellis pensat libras. ii. &. semis.

Modius dictus eo. quod sit suo modo perfectus. Est autem libre. xl. iiii. id est sextariorum. xxii.[1]
Modus unus & semis urnam faciunt.
Urnae. ii. amphoram complent.
Urnae. iii. id est artabam.
Modii. xii. gomor reddunt.
Gomor. ii. chorum faciunt.
Siliqua habet. iii. grana ordei.
Obolus habet siliquas. iii.
Olcha habet obolum. i. & semis.
Scripulus. i. habet obolos. ii.
Dragma habet scripulos. iii.
Uncia habet dragmas. viij. & scripulos xxiiij.
Calculi. iiii. habent. siliquas. iiii. dum habent scripulos. ii. & semis.
Criseus id est aureus habet scripulos. iiij.
Libra habet uncias. xii.
Semis libre habet uncias. vi.
Clatus habet dragmas. x.
Cotila in siccis habet uncias. vi. & semis. In humidis uncias. v.
Sestarius in siccis. habet uncias. xv. in humidis uero uncias. xviii.

(7) *Recipes for Electuaries.*

[folio 2 verso, column *b*]

Electuarium dia calamitatis. quod iuuat ad omnem frigus. & ideo uocem raucam.

℞ hec calamantis maiorem & minorem. ysopo. puleio. liuestici. piper. cinnamomo. gg. omnia equali pondere mel quantum sufficit.

Electuarium pro nimia saliua. a capite descendente.

[1] This sentence is from Isidore "Etymologiarum," xvi, 26, 10.

Zinziberis. iuniperi. ana unciam. i. piperis uncias. iii. mel quantum sufficit. Da coclearia. ii. siue tria.

Electuarium ad tussem probatum.

℞ hec siler. himila. petrosinilo. origano. pineae. intindate yris. illirica. puleio. ȝȝ marubio. ysopo. omnia equali pondere. mel quantum sufficit.

Electuarium ad solutionem.

Gingibri dragmas.ii. cinamomi. dragmas.ii. costi. dragmas.ii. piperis. dragmam.i. simul misce cum melle. & in modum auellane da eunti dormitum.

Electuarium probatum.

Cumini assi uncias.ii. sinapis seminis uncias. ii. piperis uncias.ii. calamantis. cinnamoni. peretri ana unciam.i. mel quantum sufficit.

Electuarium tussientibus de frigore.

Piperis grana. xx. bacas lauri. v. pulegii pondus.

(8) *Recipes for Plasters.*

[folio 175 recto, column *b*]

Ad capitis tineam. Sume picem. & ceram. & piculam. simul coque. & raso capite induces in panno. & super tineam liges. & ante nouem dies non tollas. sic sanabitur.

Item. Farina de lupinis faucibus facies. & de mulsa temperabis & sic super ustulam pones. non multum uero spissa cataplasma esse debet. Cumque super pustulas cataplasmaueris. lacer uinum. & spissum temperabis. & cataplasma super limes assidue. Et cataplasma. Iacer mutabis. Nichil tamen presens est quam sanguisugas apponere. ut feruorem pustule sublato sanguine minuant.

Lini semen combure. & cum oleo misce. & sic caput perunge.

Ceruobuli radices in uino teris. ex hoc caput lava frequenter.

Ad uomitum retinendum. Pulegium in ouo manducet per .iii. dies. & uomitum retinet.

Eadem bachas. iiii. tria grana gramilis & granum nigri pruni. & si non potes inuenire eius grana. accipe interuscum. & felgorolam quercus insimul tere cum uino. Hec medicina per petra est.

Ad caliginem & ad albuginem & lepidas maculas. Hec passiones sanat.

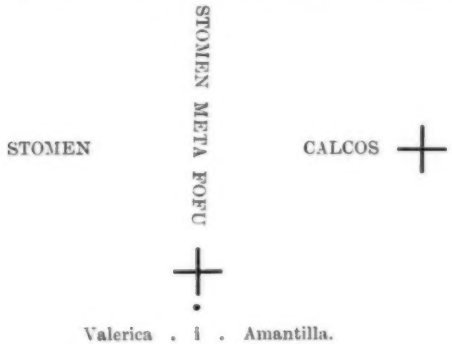
℞ hec. Celidonia. radices mundas tritas sucus expressus oua plena. iij. aceto oua plena. v. mel ouo pleno. piper solutum grana lx. aloe nigro denarios pensos. iiij. coquis acetum staupos. iii. usque ad unum ueniat. cum penna in oculis mitte.

(9) *An Anglo-Saxon Charm to stop Nose-bleeding.*

(A marginal gloss in the same hand as the text.)

[folio 175 recto]

Þid blod rine of nosum wriht on his forheafod on Xristis mel



Valerica . i . Amantilla.

(10) *On the Relation of Fevers to the Humours.*

(A marginal gloss in the same hand as the text.)

[folio 175 recto]

Cotidiana febris sit ex fleumate. Tertiana ex colera rubea. Quarta ex melancolico id est ex nigro colere. Simonacus est effemorus. ex sanguine cotidiana omnibus diebus sit res [?] accensa. Sinocus semper febrens est. Cotidiana sit ex frigido humore. quia fleuma frigida & humida est. Terciana ex calido. quia colera rubea calida & sicca est. Quartana ex melancolico. quia siccus & frigidus est. Simonacus ex sanguine quia calidus & humidus est. Cum igitur febris cotidiana fuerit. cibum dabis calidum ante accessionem. In accessionem autem nichil dabis ei. sed ante accessionem dabis ei pulcinum manducare habentem piper & cuminum [?] & pedes lactantis pecudis bene coctos & bene conditos. de aceti & melle & pulegio & buccellas de silicino habentes modicum oleum. & si satierit [?]. fac ei aquam coquam cum radice apii. feniculi. ac petrofolii & da ei bibere.

(11) *On the Principles of Pathology.*

[folio 175 verso, column *a*]

Corpus hominis diuiditur in. iiii. partes. caput. pectus. uentrem. atque uesicam; quia uer capiti nocet. aestas toraci. autumnus uentri. hiems uesicae. Natura precipue hominum. pecudum. alitumque; calido.

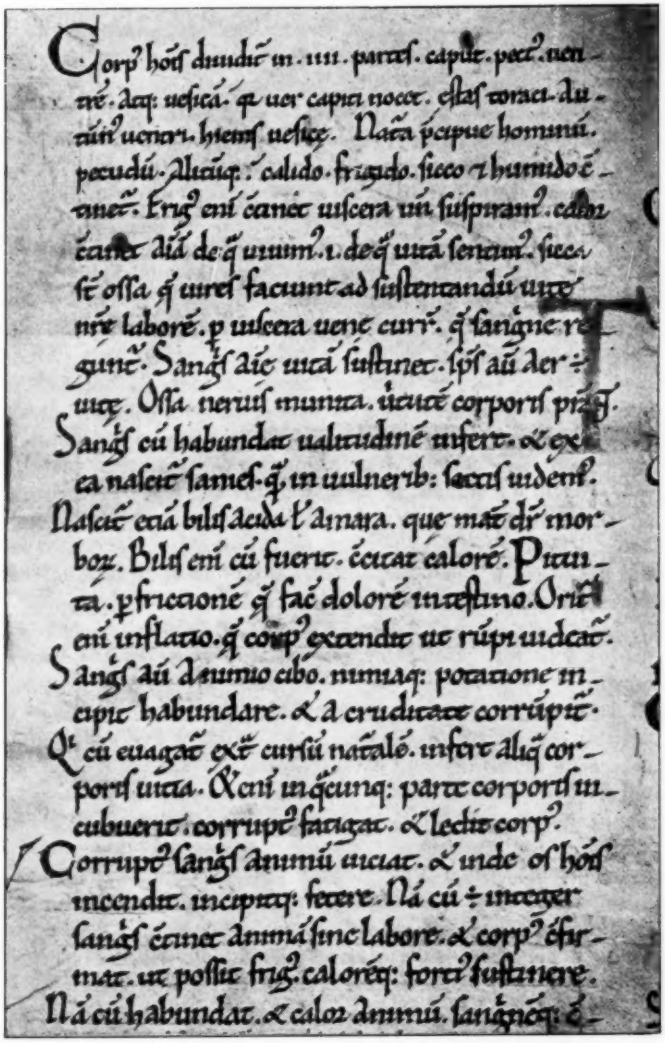


Fig. 7.

MS. St. John's 17, folio 175 verso, column *a*. A passage from the section on the Principles of Pathology to illustrate the script.

frigido. sicco & humido continetur. Frigus enim continet uiscera unde suspiramus. calor continet animam de qua uiuimus. id est. de qua uitam sentimus. sicca sunt ossa quae uires faciunt ad sustentandum uitae nostrae laborem. per uiscera uenae currunt [cum humido eorum] quae sanguine reguntur. Sanguis anime uitam sustinet. spiritus autem aer est uitae. Ossa neruis munita. uirtutem corporis parant.

Sanguis cum habundat ualitudinem infert. & ex ea nascitur sanies. quam in uulneribus sectis uidemus. Nascitur etiam bilis acida uel amara. quae materia dicitur morborum. Bilis enim cum fuerit. concitat calorem. Pituita. per frictionem quae facit dolorem intestino. Oritur enim inflatio. quae corpus extendit ut rumpi uideatur. Sanguis autem a nimio cibo. nimiaque potatione incipit habundare. & a cruditate corrumpitur. Qui cum euagatur extra cursum naturalem. infert aliqua corporis uitia. Et enim in quacunque parte corporis incubuerit. corruptus fatigat. & ledit corpus.

Corruptus sanguis animum uiciat. & inde os hominis incendit. incipitque fetere. Nam cum est integer sanguis continet animam sine labore. & corpus confirmat. ut possit frigus. caloremque fortius sustinere. Nam cum habundat. & calor animum. sanguinemque concitat. & sit sanies quam bilem dicimus. quae concitat calorem ut frigus. quae facit corporis languorem. animaeque malum generat odorem. Quo facilius bonis commodis ualitudinis fruamur. & incommoda deuitemus prognostica. id est precurrentia signis naturalibus cognoscamus. cognitaque euidemus mortifera.

Quattuor enim partes corporis originem ostendunt. a capite. & torace; uentre. & uesica. Itaque locorum motus cognoscimus. si homines sani erunt.

Urina mane alba. uel ante prandium rufa; post prandium candida. uel ante caenam rosea. Nec ego tamen prandendi necessitatem imposui. uel magis urinae tempora naturalia ostendi. Candida debet esse urina. ubi cruditas non est. Mane autem requieto corpore. urina limpida est. deinde motus ipse | [folio 175 verso column *b*] & deambulatio interanea miscet & excitat limum uesicae. ex qua urinam expellimus ideoque coloracior fit. Quod si mane urina mutauerit colorem. ostendit subesse uitium infirmitatis. quod tamen propriis indiciis intelligimus.

Cum enim a capite morbus oritur. solet capitis dolorem penetrare. Tunc & supercilia grauantur. tempora saliunt. aures sonant. oculi lacrimant. nares repletae odorem insentiunt. Cum ex his aliquid accidit. caput purgari oportet hac ratione; Ysopi. aut conule. bubulae. fasciculum feruere facias ad tercias. Sed exinde aquam ore continebis. & caput calide tractabis ut fluat pituita. Quod si quis negligit. caueat epiforas oculorum aut dentium dolorem. Interdum etiam strumae paratides nascuntur. & alia uicia quae circa fauces. ceruicesque oriri solent. Item distillacio & grauitudo narium erit. & interdum in capite ulcera nascuntur. aut etiam capilli defluunt. Frequenter uomendo stomachus corrumpitur.

Cum autem a torace morbus nascitur. incipit caput sudare. linguaque sit grossior. aut os amarum. aut tunsiles dolent. oscitatio sequitur frequens. sine somno & quiete. torpor membrorum animique dolor. prurigo corporis. brachia manusque assidue tremescunt. subitoque tussis arida & inquieta nascitur. Ex his ergo cum aliquid acciderit. in talibus uicium graue. si uomueris siue ieiunus seu pro caenam uel in balneo. Plus autem prodest si ieiunus bilem eieceris. eam dicimus matrem morborum. Sed qui uomere nolunt uel non possunt. quod uomitus corrumpit stomachum. decimo quoque die quoque ieiunando ab omni cibo se abstineat. Quo facto. uitium grande uitabit. Nam ut dixi frequenter uomendo solet stomachus corrumpi.

Quod si a uentre morbus oritur. haec erunt signa. Uentus uertitur atque turbatur. & sentiet crebros dolores. cibus & potus amari uidebuntur. succidunt genua. lumbi grauantur. interscapilium contrahitur. totumque corpus particulatim grauabitur. tardantur pedes. pigra fiunt crura. renes indolescunt. & pro haec febriculae incurrunt. His itaque cognitis. prima est abstinentiae utilitas. Tunc etiam medicamentis aluum purgari oportet. ut graue corpus leuamentis adiuuetur. Quod si morbus maior primere uidetur. adicies diem alterum abstinentiae si tamen uires patiuntur. Summius quam leuissime sumes cibum. tantum ouum sorbile. aut aliquid oui simile accipias. Hoc qui negligiunt | [folio 176 recto, column *a*] fiunt ciliacii. termum duosi. desinterici. Etiam ex eo nascuntur tercianae. & quartanae. Fiunt podagrici. Morbus etiam articularis hinc accidere solet. Quidam etiam sunt amantes. Quibusdam etiam sanguis erumpere naribus solet. Oportet ergo occurri tam grauibus morbis curatione predicta.

A uesica cum nascuntur morbi. haec dabunt signa. Pleni uidebuntur. & cito saturari. sequitur inflatio uentris. strepitus creber uidetur oscitare nec oscitant. sed tantum os deinducunt. sequitur totius corporis stupor. sonusque grauis. sit urina liuida. & uix erumpens. tumescere etiam uerenda. & inde calculosi fiunt. Haec uitia sic emendantur. Feniculi & apii radices uino austero madefacto. uel etiam earum herbarum radices ceteres. & ex uino ciatis duobus tantum de aqua calida accipiat assidue uel duaci semen & myrtum cum aqua calida sumat. uel cicer album non arietinum madefactum in uino. ut supra scripsi. & bibe. uel radices asparagi. uel herbam erraticam. uel serpillum decoquis. eius aqua uino mixto bibe. Quae qui negligunt. fiunt hydropici. Sequitur his iecinoris remum. & uesicae dolor. fiunt quoque cauculosi. Sequitur strangiuria. & uenter tumescit. Intueri autem oportet aegri uires. quomodo possit sustinere medicinam. ut ita febre & cruditate careat. Fiunt ergo cataplasma adibentata quoque apta sunt capiti. uentri. uesicae: & cruditati. frigori. calori. prout tempus exegerit; uti autem ad haec oportet rosa. aceto. uino. oleo. yrino. linguam quoque egri aspera melle perfricari. uel mente folia cum melta linguam illi fricet.

(12) *Panaceas.*

[folio 176 recto, column *a*]

Aduersus omnes impetus morborum duo sunt remedia certissima.

Primum est ut decimo quoque die sit abstinentia in cibo & potu: deinde postero die ut laueris. ciboque firmo utaris. Quare & obseruationem efficies. ne quod omnino uitium corpori tuo accidat. ut perpetua sanitate sis tutus.

Est et altera potio salubris. quae omne corporis tui uitium eliminat. & ualitudinem confirmat. Has autem potiones iam tecum habebiss. Compositionem myrrae denarios .iii; feniculi dragmas. .iii. piperis dragmam. j. aneti dragmam.; contusi ualidissime & mixti. adicies uini candidioris. tantum quantum sufficerit. ut coeat quasi fermentum. Quod mixtum. mittes in linteolo mundo. alligabisque diligentissime. Quod ligatum mittes in uino suauissimo & ceruigium. ut ibi sit |

[folio 176 recto, column *b*]

diebus. xx. & quantos dies feceris. tanto melius erit. Deinde tolles linteolum. ex eo uino cotidie bibito in die eminam mixtam unam potionem ante prandium. & unam pro cenam. Hac potione usus nunquam infirmabis corpore. Herbas crescentibus numeris lunam obseruas dum tollis. & componis curationem. Si ita non feceris & ad diminutionem lunae sustuleris. minus te ualere scire oportet. Cum autem minuitur luna. una quoque res diminutionem sentit. Quod si ita est in herbis quoque legendis componendisque medicamentis uim eius & potestatem ne dubites.

(13) *Alphabetical List of Remedies with the Diseases for which they are appropriate.*

[folio 176 recto, column *b*]

Aquilon. fascimentum. Eiusdam usus. camilonta; genera eius sunt. ij. alba & nigra. alba utilior. Sucus cuius cum rosatio capitis dolorem sanat. Bachae eius. xi. cum aqua trita liquorem qui bibat. calculum frangit.

Elenus id est linus. Elimos[1] id est lini semen.

Ecitdidinis [?] id est serpens. Eliotropium id est intuba a grecis siue solsequia. uel sponsa solis.

Egineos [?] id est caprificus.

Elelifagum id est saliua.

Berbina[2] gessis [? for grecis]. girobotanis.[3] prestantior nascitur locis occultis.

[1] ἔλυμος. [2] Vervicina. [3] ἱεροβότανη.

Costa in aqua ad tercias impetum oculorum sanat. puluis eius cum uino trita. uenenum extingit cum uino & pipere pota tercianas & quartanas discutit.

Celedonia irundinea. sucus foliorum in umbra siccatus collirium factum oculos sanat.

Cerfolium. cicimas [? for racemus] eius. iiii. teris mel coclearium. j. cum glicano. papauer uiride bulliat. quod si malanma stomacho inducis. dolorem tollit ; uiride cum uino bibe ictericos sanat.

Brionianossa.[1] cucurbita a grecis. nascitur iuxta sepes. calami eius longi. folia subalbida. aspera. radix alba. grossa ut rapa. sucus radicis eius pota cum mulso uentrem mouet. & uomitum prouocat.

Britannica similis plantagini, folia ut beta. nascitur locis aquosis sine calamo & flore. cum uino pota paraliticis prodest.

Centauria major. id est felterrae. sucus eius cum melle ac lacte caprino caliginem radit. In uino cocta bibita lumbricos & tineas uentris extingit. in aqua cocta ad tercias stomachi doloris sedat.

Centenodia sanguinaria. ileicon. proserpina. policonus nascitur locis cultis. Sucus eius cum uino optimo emotoicis[2] prodest. sucus eius cum caltenda [?] dissentericos curat.

Fisalidus.[3] fistolidia. serola [? for ferula]. nascitur locis ha-

[folio 176 verso, column *a*.]

renosis calamus longus. flos alba bene olida. milfolia prope similis. radix nucleata quae cum uino & pipere pota omnia uitia uesice purgat. cauculum [i.e., calculum] frangit.

Gladiosus ortensis irius nascitur locis ortensis. folia eius ut gladioli paludensis. calamus uastus. flos eius persus. radix crispa & lata. ut pollex. ex radice eius cum mulso bibitur. urinam mouet radix eius in mulsa cocta & pota siccam tussem curat.

Gamandrea[4] nascitur locis montuosis. folia uiridis coloris & scissa. flos purpureus. leges eas mense augusto cum uino pota sanguinem spargit.

Glicanus.[5] puleius medianus. masculus habet florem rubeum. femella album cocta. bibe menses mouet. in melle & aceto. cocta gargaricetur. caput purgat.

Eliotropia. id est solsequia. sucum uenenum extingit. folia eius tunsa luxum sanant in cibo sumpta stomacho prodest.

Ebolus. radix eius in aqua cocta ad tercias bibat. cauculum frangit sucus. radix eius coculi. quattuor uino emina bibita. ydropicis ualidissimum est.

Ysopus herba maritima. calefacit fleumam & deducit.

Maurella ficaria nascitur locis humidis. calamus altus in cacumine eius capitelli minuti. folio lata ut ortica.[6] cum anxugia imposita prodest ad ficum pota similiter.

[1] Bryonia. [2] αἱμοπτύικοι. [3] φυσαλλις. [4] χαμαιδρυι. [5] γλήχων.
[6] Urtica (ortica = Romance spelling).

Malua ortensis uirtus eius mollis. & purgat uentrem & uesicam cum oleo sumpta uentrem molliat.

Nimphea alga. nascitur locis aquosis & paludensi[bu]s. calamus & folia canapis similes in cacumine calami flos oblongus purpureus. radix aforis nigra. intus subrubea. dura ut lignum. radix munda concisa. in lacte caprino cocta sepius pota disenterriocos curat. semen eius facit similiter.

Flomus[1] ueruas[cum][2] uernamo[3] blandoneia. genera eius sunt. ii. alba & nigra. Nigra utilior. masculi folia densa & utiliora semine. albidiora folia in aqua cocta tumorem spargunt. radix cum uino & pipere pota pectus & uesicam purgat.

Nepta trita cum calida aqua ante horam accessionis bibe cotidianas & tercianas liberat. folia eius cum sale trita percussura & liuores sanguinolentos curat.

Narticos.[4] ros maior sucus eius dragmam. i. in uino bibe aluum mouet. uenenum excludit. excutit sucus eius cum melle caliginem extingit.

Sinapis sucus eius gar | [folio 176 verso, column *b*.] garnizetur. caput purgat. cum nasturtio tritus ad ospicias sanat.

Ostriago. nascitur circa sepes uel monumenta uel parietes folia eius camitrio[5] similia. calamo rotundo. radix mellina colligis eam mense iulio tunsa. & imposita omnia uitia corporis sanat.

Politricon.[6] paritaria[7] uetraginis[8] capilli ueneris. nascitur prope parietibus uel in puteis. siue in falisiis.[9] ramus eius quasi seta porcina. folia eius quasi filicicula[10] minuta. sed minora & angustiora folia eius trita & pota. capillos nutrit. & sepius pota gutta spargit. ipsa poeta [for pota] partum secundum eicit.

Polipodium adiantur[11] filcicula[10] quae in arbore nascitur. radix eius in mulso cocta cum pipere trita. uentrem deducit colera & fleuma purgat urinam & menses mouet. & ad omnes intrinsecas passiones curat.

Andragines[12] id est crassa gallina uoc[-atur][13] mastigata morsum scorpionis curat in aceto triti. ardorem stomachi sedat.

Papauer. semen eius tritum cum aceto emigranium curat. simul cum aqua fronti inductum somnum facit.

Puleius maior in aqua coctus emendat epaticos. tussientes fleuma deducit. sanguinem & colera[m] mitigat.

[1] φλόμος. [2] Verbascum. [3] Uernamo = vervain = verbena. [4] νάρθηξ.

[5] χαμαίδρυς. [6] πολύτριχος. [7] Rarietraria.

[8] Vitrago = vitrearia (pseudo Apuleius).

[9] Falisilis, a French form = modern French "falaise."

[10] Filicula. [11] ἀδίαντος. [12] ἀνδραχνη.

[13] This gloss "id est herba grassa gallina vocant," occurs in two MSS. of the tenth century, Cod. Bernenais 337, and Cod. Vaticanus 4417, printed in Goetz "Corpus glossariorum latinorum," Leipz., Teubner, iii, 615, 31, and 627, 34.

Puleius minor. serpillus folia eorum cum oleo & sale frixa caputungis dolores sedat.

Rafana. radix cum oximello data. uomitum prouocat.

Rapa. napa. semen eius cum mulso tritum pota. uariola sanat.

Ruta semen eius cum uino tritum uenenum excludit.

Pulius[1] eius foliorum naribus insufflatibus sanguinem crassum stringit.

Radafine. librae sucus foliorum eius uulnus in fundis uermes occidit. emis calami eius cum oleo mixtus caliginem tollit.

Subtussilla. agaona cum uino bibata folia eius. guttam malignam spargit.

Sinapis albus. tritus cum uino bibe. uentrem mouet. educat urinam.

Saxifrica id est grumilium.[2] nascitur locis harenosis. folia uiridis. calamus longus. semen candidum. tritum uesicam purgat. cauculum frangit.

Ungula caballina in aquis nascitur. folia rotunda & lata super aquas natantia. radix grossa. de ipsa radice manipulum. i. pota febres discutit.

Wlgaginis.[3] folia rotunda. semper uirent. de ipsa folia. .xi. cum uino pota. uomitum prouocant.

(14) *List of Diseases with their Appropriate Remedies.*

[folio 176 verso, column *b*.]

Ad passionem in angulos oculorum. Allia in aceto cocta trito super impones. inueteratos & recentes egi lippas curat.

Item ad caliginem oculorum. Condera terrestria sucus. pinpenella sucus. oleo. ana. coclea. i. cum his oculos lines probatum est.

Item ad caliginem. Condere sucus cum pipere tere. ungis acra est. cessare non debet interuallo biduum uel triduum. mirum est.

[folio 177 recto, column *a*.]

Ad ordiolum in macula. qui super oculos nascitur. farina ordei cum mulsa misces locum imponis. a multis expertum est.

Item farina de faba cum sapone teris & imponis.

Ad oculos lacrimos. rutam siccam cum melle & aceto equaliter mixtum per linthea colatum. oculos inungis certum est lacrima stringit.

Potio probata ad paralisi. Saluia. saùina. pinpenella. subtus ille. caprifolia. pulpodia radix. semina iuniperi. ac grana piperi. ccc. cum

[1] Pulegium?

[2] Milium gruinum.

[3] Vulgago, vulgagina, "officinal" name for asarum europaeum.

uino facis potionem mel dispumato calicem plenum. da bibere ieiuno & pro cena nocturno. & iacet in lecto bene coopertus ut sudat.

Contra ficum herba triangula.

Enema sciaticis id est relaxatio. & ad fleuma educenda hoc est alteae[1] radicibus [gloss in same hand uismalua]. Abrotana. centauria. absinthium. betarum. radix & folia. ana manipulum. i. aqua sexta. ii. coquis addens. furfuras. triticeas galoxinas. ii. salis manum plenam simul bulliant modice. & iterum colans addes mel mundo calix. i. oleum. calicem. semis. commiscens tepidum uncias.

Enema. ad eos qui stercora non reddunt. Maluis. betis. brioniae radix minutatis. ana manum. ii. furfuribus triticeis. goloxinas. ii. sal manum plenam aqua sexta. i & semis. coques ad tercias & colans exinde calices. ii. inter oleum & mel calix. i. commisces. & tepidum inicias.

Collirium ad stercora dura. & squibalci[2] deponenda. mel stercus soricinum. & salem mittens in popia ferrea coques ut pastellus fiat. & exinde mandaliones. iii. aut. v. inicias.

Collirium ad hoc mittes. mel partes. ii. atraminta partem. i. coques in patella qui spissum sit. & faciens mandalionem. i. & soluet utiliter.

Collirium lenissimum. Vesicam porcinam mundatam & latam extorquens mitte intus fellis porcini quantum uis. & ipsam uesicam in umbra suspende donec fel arescat. & cum opus fuerit exinde inter digitos macerabis & facies collirium dormitum eunti iniciat.

Hac potio omni corpori dat sanitatem. & fortitudinem. Si quis illam usitauerit nullam egritudinem incurrit. Utilis enim est ad reuma uertigines. caligines humores incutaneos rufo fel cauculosos epaticis. spleneticis. sanguinem erraticum qui uarietatem facit lumborum dolori ne freneticis uesicam purgat. lapides frangat. neruos geniculorum dolorem. paraliticis. urinam mouet. somnum facit & in omnes infirmitates mundat.

Recipit apü semina. leuindola.[3] uiola oderata. feniculi semina. liuestici semina. uetonica fenogreci. coste. manus. i. ana m. i. petrosilino. centaurea. ruta. semina brasice. semina lauribus. pastinaci semina. iusquiami semina. [folio 177 recto, column *b*.] ana-
| ti. semina. asaro. gariofile. piper. spica. mastice. ana uncias. iii. His omnibus puluerem faciens. addes uino tertia parte amphora. mel obtimo libras. xv. & commisces repones in uaso picato. da exinde ieiunis per singulos dies calicem unum. Qui desiderat sanitatem. hoc habeat in usum.

Potio ad humorem melancolicum & guttam erraticam. hictericis. artreticis. recipit agrimonia radice & folia. uetonide radix & folia. nepita. pinpenella. cardopane radix. milfolium. pentafilon.[4] benedicta. subtusilla. apia radix & semina. petrosilini semina. petroniola. ambrosia. bladona radix. glicono.[5] coculi. artemesia. feniculi semina. cardonis fullonicis

[1] Altheae. [2] σκύβαλον. [3] Lavandula = lavender. [4] πεντάφυλλον. [5] γλήχων.

radix. cammetrius. minta nigra. saluia. febrofugia. balsemita. sercullo. ana. manus. i. teris. addes uino sext. j. mel calicem dimidium commisces & colas. addes piperis grana. lx. da exinde & sero calicem. j.

Ad uentrem soluendum. ℞ purgatorie grana. l. teres cum melle & aqua calida dabis bibere. temperate prime quod fortiter soluit.

Gargarismum probatum pro raucitudine. Fenogrece semina cocta saluia siccas. teris simul & caucto pauco melle distemperas & tepidum gargarizet. fistulas curat & fleuma tollit.[1]

Probata potio ad paralysim. Saluia. sauina. febrifugia. minta nigra. ypiricum id est ruta agrestis. betonica. agrimonia. sarminia id est cerfolium. satureia. britula. pimpinella. agaone. id est uermicularis. uel uerbena. cardo. faracfolia. millefolia. ana. manus. i. tere cum uino & pipere. da bibere mane & sero.

Ad stringendum uentrem. Fac potionem de cerefolio spissam prius cum oua. iii. & pro potionem bibe. & postea balneare.

Pro uermibus in uentre hominis. habeba mercurii ex aqua temperata bibat. uermes eicit. Pro vermibus in ventrem hominis. Item centauriam in aqua & vino coctam ad tercias.

Ad bonum malanum [probably for malagnum *μάλαγμα*]. Testiculos porci tolle. & finde. & appone.

Ad spinam seu sagittam extrahendam. Carōni radices uel folia teris cum resina & albumen oui cataplasma impone.

Ad tumorem brachii qui male sanguinatus est. Farina tritici cum melle & lacte compasta per triduum super pones.

Ad malanum radunculatum. Liuesticam[2] cum aqua & sale benedicto bibe.

Item. Porri capillos tere in mortario & postea misce uetus unctum & inde facies pastellum. & liga ubi dolorem radunculi senseris.

Ad ficum. Vetonica manipulum. i. costum bonum bene tritum simul cum uino bibe per dies. ix.

[folio 177 verso, column *a*.]

Ad ficum qui in ano nascitur. Canis caput cinis factum desuper aspergatur.

Ad calorem oculorum. Vetonica cum aqua calida bibat ciatos. iii. ieiunus.

Vrina spumosa quasi uisciculis super aqua ex pluuia facta & nichil malum sentiens in corpore. egritudinem futuram significat.

Vrina sicut furfur apparens uel nubem nigram habens longam egritudinem significat.

[1] Marginal glosses in same hand as text: Tres partes salis & quarta aquae hoc est mare.

[2] Liuesticam: modern French, lavage.

Vrina pura & desuper natantem nebulam quasi caliginem signum malum est.

Vrina ydropis similis ius fabe pessimum est.

Ad rupturam. Si quis ruptus fuerit in genitalibus & ipsa intumuerint. accipiat de pilo leporis & conglutinans inserto melle ad quantitatem unius fabae & absorbeat. confectionem illam procul dubio sanabitur.

Si epatica uena male incisa fuerit & intumuerit: sic curabitur. Fabam coques in uino ut resoluat. addes oleum quantum sufficit & panno induces. sicque calidum ligabis & succuret.

Si quis medicus uulnera non potest curare. agrimoniam tundat & mel immisceat. & imponat. & sanabitur.

Ad purgationem sanguinis. Cerfolium cum sale bibat.

Ad muris & araneae morsum. coagulum agninum ex uino bibatur.

Ad morsum canis. Nitru. axungia. resina. aceto equalo pondere imponatur. Item. Marubium cum axungia uetere tritum.

Ad serpentium & hominis morsum. & omnium ferarum. Cinerem cribatum cum aceto impone.

Vt aues manu capias. Frumentum in fece uini macerabis. feces dico mixtas cum suco cicutae. & auibus sparge. Siquidem inde aliquid gustauerit; non uolabit.

Ad carbunculos. Porri sucus & cinerem simul misce & pone. Item. Calcem uiuam ex aceto solutam impone.

Ad mulierem que partu laborat. Millefolium cum uino bibat.

Ad cauculum uel lapidem frangendum. Lac capricorni bibe calculum cum mulsum fuerit. aut tepidum faciat ad focum. Item. Capilli porri succo calice pleno in calido balneo bibe.

Ad sanguinem stagnandum de naribus. Millefolia manus. i. bibe cum uino.

Ad renum dolorem. centauria cum aqua frigida bibat.

Ad glandolas. erbas. ebulo. cataplasma impone.

Ad cordum pulsum. Bacas lauri. iii. cum uino coque & bibe. Item Marubium. rutam. abrotanum in mortario trita cum melle ieiunus per triduum singula coclearia accipiat.

[folio 177 verso, column *b*.]

Ad tercianas febres. Rutam. pinpernillam. iouis barbam. plantaginem. coliandri semina uel folia cum piperis granis. xxv. accipe ieiunus cum aqua calida ante horam accesionis.

Ad quartanam. Iecor leporis tritum & potum cum aqua datum. febres tollere manifestum est. Ceruinam medullam in aqua calida resolutam bibere da. non plus quam unam unciam. Item. Rute manipulos octo in uini sestario coque usque ad tres staupos perueniat. Hora suspecta da bibere.

Ad caliginem oculorum. Pulegium diligenter teres. ieiunus bibe. Item. Feniculi radicem. uetonicam. ederam terrestrem. in aqua bene coques. & postea mel adicies. cola diligenter. & oculos unge.

Ad aurium dolorem. Medullam uituli recentem tritam infundis auribus. & sanat.

Ad surditatem. Accipe rutam & exprime uis illius. & misce cum sageno [? for sanguine] anguille. & colloca te in lecto. & pone predictum sucum in sanam auriculam. & obdormi. Item. Vrinam recentem pueri parui. in auriculas infunde.

Ad dentium dolorem. Radicem plantaginis commastica.

Ad scabiem. Accipe. ix. rodellas de elno & manduca per. iii. dies. Et tunc accipe radices ipsius herbae & mitte in stuppa hudata & coque subtus brasas calidas. & sic cum lardo ueteri uel uncto commisce ac deinde te perunge usque quando sanus sis.

Ad uerucas. Tange illas & superfunde de semine ebuli. & deficient. Item. Solsequiam teris. & cataplasma cum sale uerrucas & deficient. Item. Vrinam recenti canis. cum suo luto illinita. Similiter. Vrina asini recenti cum suo luto. Sanguis de pullo calidus superpositus uerrucis & cum digito fricatis prodest. & quando siccant de alieno sanguine iterum tange.

Ad eos quibus uox intercluditur. Diptannum dabis cum uino & aqua & proficit. Herba nimphea cessare facit. Item. Herba restologia.[1] bona est etiam fistularis omnino sanat.

De uerme uel farcim.[2] Singillatam herbam in ceruisia decoquis. da bibere pro solum oocasum. & superligas emplastrum. Necat uermem;

(IV) Partial Translation and Commentary.

(1) *On the Constitution of the Body from the Four Humours.*

"The four humours rule each in its own place. Thus *blood* rules in the right side, especially in the hepar which we call jecur (liver) as well as in the heart. The *red bile* rules also in the right side, and notably it fills the gall-bladder. *Black bile* rules in the left side where the spleen lies. Phlegm reigns partly in the head and partly in the [urinary] bladder.

"Blood is hot, moist and sweet; red bile bitter and lively (viridis); black bile acid and cold; phlegm cold, salt and moist.

"They each have their special seasons: blood the spring, red bile the summer, black bile the autumn, phlegm the winter.

"Again each humour has its own place in the body where it is exhaled: blood through the nose, red bile through the ears, black bile through the eyes, phlegm through the mouth.

"The four humours produce special types of temperament, thus (mores tales faciunt): Blood makes a man of goodwill, simple, moderate, reposeful, and sturdy. Red bile [makes a man of even temper, just, lean of figure, a good

[1] Aristolochia.

[2] Vermis was a mediaeval and renaissance term for boils or farcy.

masticator of his food and of strong digestion. Black bile] . . . make a man irascible, greedy, avaricious, sad, envious and often lame. Phlegm makes a composite type watchful, introspective and growing early greyheaded.

"The body has four qualities and four tissues, to wit bones, nerves, vessels and flesh. The qualities are cold, heat, moistness and dryness.

"The desires are four: Hunger, thirst, concupiscence and repose.

"Also there are four natural processes: Eating, drinking, generation and sleeping."

The passage summarizes the doctrine of the humours and the temperaments and illustrates admirably the analogical character of mediaeval science.

It is practically identical with a short work attributed to Giovanni Monaco, a disciple of Constantine the African.[1] The doctrine is clearly taken from Galen, and despite the alleged contact with Constantine it shows nothing of Arabian influence. The evacuation of the superfluous humours in relation to the four organs of special sense is a favourite Salernitan doctrine[2] and is mentioned in the Flos scholae Salerni:—

> "Phlegma per os, oculos respicit cholera nigra
> Sanguis per nares, sed cholera rubea per aures."[3]

(2) *On the Regimen of the Four Temperaments.*

"For those in whom red bile rules we should order blood-letting in spring and purgation in autumn. There is nothing better than a right good purge."

Here follows the composition of purgative draughts suitable for those of bilious temperament. These purges should be taken first thing in the morning in warm water and fasting.

"Black bile gives rise to *melancholy*, which produces internal lesions and indigestions. For these nothing is better than abstinence and suitable diet. Such may take mutton but not goat, hare or pork, for these increase melancholy, especially pork which is a very poison to the melancholic and causes weight on the stomach. Beef may be taken."

The special purges suitable to the melancholy are then given. The

[1] The text attributed to Giovanni Monaco is printed by de Renzi, loc. cit., ii, p. 411.

[2] It is illustrated by a drawing taken from a fifteenth century Roger Bacon MS. and reproduced in an article on Renaissance Anatomy by the present writer in "Studies in the History and Method of Science," Oxford, 1917.

[3] De Renzi, loc. cit., v, p. 49. The couplet is from a MS. discovered by Baudry de Balzac, of which, however, de Renzi does not give the date.

relationships of the remaining two temperaments, the sanguine and the phlegmatic, are simple. Blood is warm, moist and sweet, phlegm is salt, acid and dry.

As regards quantity there are in the body 12 lb. of humours. Of these the blood itself comprises 6 lb. and the other humours 6 lb. between them. This is in health. If the blood exceeds the other humours *elephantiasis* ensues. If the humours exceed the blood *dropsy* is the result. To keep the balance even, blood-letting on the one hand and purging on the other must be judiciously regulated.

There then follows the short outline of a blood-letting and purging calendar of which unfortunately some parts have been destroyed by the binder. It explains which days are unsuitable for either purpose. The outline of the subject is, however, repeated in the chapter which follows. The idea of a calendar for purgation and of different purges for different types and temperaments lasted on well into the sixteenth century. It is the subject of perhaps the first piece of printed matter on a medical topic that has yet come to light.[1]

The doctrine of lucky and unlucky days, applied not only to purgation and phlebotomy but to the general conduct of life, is ancient and widespread. It leads us back into the most remote historic antiquity, and can be found in modern times among the more primitive aborigines in both hemispheres. As exhibited in relation to blood-letting it is traceable in Galen, and there is much of it in Anglo-Saxon medical literature. A separate work on the subject is attributed to Bede,[2] while a part of a Calendar devoted to this topic is contained in our MS.[3]

[1] Coniunctionis & opposicionis solis et lune ac iniciones electre nec non dies pro medicinis lanativis sumendis in anno domini Mcccclvij. Mr. Arundel Esdaile has kindly given me the particulars of this work. The unique known copy was discovered by Gottheil Fischer in 1805 among the Mainz Archives, and given by him to the Bibliothèque Nationale. The volume is imperfect, but the surviving fragment has been published in facsimile by G. Zedler, "Die alteste Gutenberg type" (Gutenberg-Gesellschaft, 1902, i, pp. 34-41, and Plate V); and again, with a bibliography by Seymour de Ricci, "Catalogue général des premières impressions de Mayence," (Gutenberg-Gesellschaft, 1911, viii-ix, p. 11) by K. Sudhoff, *Arch. f. Gesch. d. Med.*, Leipz., 1908, i, p. 219.

[2] J. A. Giles, "Venerabilis Bedae opera quae supersunt omnia," Lond., 1843-44. The "De minutione sanguinis sive de phlebotomia," is printed in vol. vi, p. 349 of that work, and is contained also in our MS.

[3] On folio 4*a*.

(3) *On Blood-letting.*

The section on blood-letting is of especial interest as containing the "De phlebotomia" of the Venerable Bede. This work is regarded by most authorities as genuine. The texts hitherto available of Bede's Tractate on Blood-letting are however imperfect, and that before us appears in many passages to be preferable to those generally received, from which it differs both in containing previously unknown passages and in excluding some that appear in the printed versions.[1]

"If there be need, phlebotomy may be performed at any time, but especially from the eighth kalends of April to the kalends of July. It is especially good to let at that season because the blood is then on the increase.

"At other times, however, the season as well as the character and phase of the moon needs to be closely watched. Especially beware of the 5th, 10th, 15th, 20th, 25th, and 30th days of the month. On these days, *quinnones* as doctors call them, no blood should be let . . . for many physicians assert that multitudes have perished by neglecting to observe this rule. But above all beware of the period from the fifteenth kalends of August to the nones of September. These are called the *dog days* after the dog star, and in them no purge should be given and no blood be let since then the humours are mixed; only if jaundice should set in should a vein be opened and the superfluous or injurious blood be let . . .

"Purges are good in the autumn from the eighth kalends of October to the eighth kalends of December, for since in summer and winter the body produces phlegm and black bile or melancholy as we call it, in autumn it should be purged of these.

"Furthermore stale phlegm and bile both black and red accumulate in winter and should be dealt with by medicaments or fasting. The treatment of black bile should continue until the next autumn."

This extract may suffice to give an idea of the contents of the work on phlebotomy, a line of treatment, or rather regimen, universally practised throughout the Middle Ages.

The passage is followed by a list of the suitable sites for blood-letting, derived ultimately from Galen. Twenty-four vessels[2] are selected. Those named are: Two arteries in the occiput for pain in the head; two vessels near the outer canthus for trouble in the eyes; two under the tongue for "reuma" of the gums; one in the mid-forehead for headache and "alienation"; one in the nose for "heavy

[1] In the Latin text we have prefixed [B] to those paragraphs which are to be found in Giles's edition of Bede's "De phlebotomia."

[2] The text says twenty-three, but names twenty-four.

head"; two in the neck for "accumulation of humours" in the head, eyes or gums; four (two on each side) in the hands; the vessels in the thumb for "inflation of the lungs" and two in the little finger for "inflation of the spleen and for toothache"; two in the nose for ophthalmia, for pain in the heel, for kidney disease, for sciatica, and for sterility; two in the feet for swollen testicles; six (three on each side) in the arm; these are the most important of all, and are named by our author cephalic, mesaon and epatica. These vessels correspond roughly to our cephalic, median and basilic veins, and were those on which the Salernitan school "specialized."

The whole doctrine of blood-letting in the Dark and Middle Ages is a difficult and complicated subject. Large numbers of veins were used, and different veins for different diseases. The general course of practice in the Dark Ages appears to have been to restrict bleeding to the vessels in the antecubital space, and this is notably the case in writings of the Salernitan school. With the re-introduction of the old Greek learning at the hands of the Arabians, the methods of blood-letting again became very complicated. The tradition, however, carried through the ages by Bede's phlebotomy, appears somewhat to separate the practice of phlebotomy in England from that in other parts of Europe.[1]

(4) *On the Prognostication of Life or Death.*

"The true prognostica from a book of Galen. It is a sign of death when the forehead grows reddish and eyebrows sink, the left eye decreases, the tip of the nose becomes white, the chin falls, while the pulse runs, feet grow cold and the belly is relaxed.

"If a youth will not wake or an old man will not sleep, it is a fatal sign.

"A prognostic of every sickness so that you may know whether a man will live or die. If the testicles grow cold or their nature contracts or they both depart out of their place, it is a fatal sign.

"For anal disease. Take the root of dolsa and give it him to drink in holy water. If it hurts him in the passage he will die. If not he will live.

"To know whether a sick man will live. Smear his hands with must. Then give it to a dog to eat. If he eats it the man will live, if not he will die."

The first part of this extraordinary prognostic is contained in the Salernitan poem "Flos Medicinae," a work of somewhat earlier date than our MS. The parallel lines in that poem run:—

[1] It is proposed on another occasion to discuss at greater length the practice of phlebotomy in mediaeval Europe.

His signis moriens certis cognoscitur aeger:
Fronte rubet primo, pedibus frigescit in imo,
Inde supercilium deponit sine propinquo.
Decidit et mentus, laevus lacrimatur ocellus,
Deficit auditus nasus sum motenus albet.
Sponte suo plorans, mortis pronunciat horam
Ante venit pulsus de currens propero nisu.
Excubias patitur juvenis noctuque diuque,
Signe senes dormit, designat nocte resolvi.[1]

The passage is clearly a dim echo of the famous description of the "facies Hippocratica" which the Father of Medicine sets forth in the second chapter of his book of "Prognostics."

The remainder of the section is typical Anglo-Saxon medicine and can easily be paralleled from that literature, in which also, as here, the dog finds a large place.

(5) *A Glossary of Herbs.*

This list of plant names is wholly Salernitan and appears to contain no true English plant names, though some of the spellings have been anglicized. Practically all these names can be traced to the Alphita[2] or to other of the Salernitan writings, and ultimately to Greek sources. The linguistic elements involved we have already discussed in Chapter II.

(6) *A Table of Weights and Measures.*

The system of weights and measures here given is excessively complicated, and characteristic of Salerno. In another part of the book, in the course of a work on the Abacus, the system is further explained, together with the symbols adopted. This table is of complex origin, and is an attempt to equate weights and measures derived from different sources, for it can hardly be supposed that all those mentioned in the table were actually in use at the date when the MS. was written. One phrase in this table is derived from Isidore of Seville, while the "gomor" is a recognized Anglo-Saxon measure, the word being derived from the biblical Hebrew measure the "omer."

[1] De Renzi, loc. cit., p. 491.

[2] Printed in de Renzi, iii, p. 271. An interesting fifteenth century English MS. of this work has been edited by J. L. G. Mowatt, "Anecdota Oxoniensia: Mediaeval and Modern Series," Oxford, 1887, i, pt. 2. Another similar work, but of earlier date, edited by the same author in the same series, is the "Sinonoma Bartholomei," Oxford, 1882.

(7) *Recipes for Electuaries.*

Electuaries were always a special feature of the Salernitan pharmacopœia, and those here given are composed of drugs which are included in the Salernitan repertory, though we have not traced the preparations themselves.

(8) *Recipes for Plasters.*

The composition of the plasters is much less complex than that of the electuaries, and the more homely remedies of Anglo-Saxon medicine are here encountered. This chapter is therefore in contrast with that on Electuaries.

(9) *An Anglo-Saxon Charm to Stop Nose-bleeding.*

This charm is of a type common enough in Anglo-Saxon literature. "If blood runs from his nose, mark on his forehead on Christ's eve" . . . and here follows the charm written in the form of the cross.

(10) *On the Relation of Fevers to the Humours.*

Our author distinguishes four types of fevers, the quotidian (or hectic) caused by excess of phlegm, the tertian caused by red bile, the quartan by black bile, and lastly the synochal or continuous (*συνέχειν*) which he describes as "semper febrens," identifies with ephemeral fever and attributes to blood. The character and symptoms of these four fevers correspond to the humours that originate them, and thus provide another illustration of analogical reasoning based on the "physiological fours."

(11) *On the Principles of Pathology.*

"The body of man is divided into four parts: the head, the chest, the belly, and the bladder. Spring is the enemy of the head, summer of the chest, autumn of the belly, and winter of the bladder.

"Now the nature of man and of beasts comprises warm and cold, dry and moist. Cold possesses the viscera by which we exhale, and heat the spirit by which we live and feel; dry are bones which give firmness to the body to support the labour of our life, while through the viscera run the vessels with their moisture controlled by blood.

"It is this blood which sustains the animal life but the spirit is the breath of life. The bones are provided with nerves, which furnish the strength of the body.

When blood abounds it brings health, and from it *sanies* is produced such as we see in incised wounds.

"A substance is also produced called acid or bitter bile, which is regarded as the substance of disease, for it excites the production of heat.

"Phlegm produces chafing and pain in the intestine. It causes distension, which inflates the body so that it seems as though it would burst.

"Blood becomes superabundant by reason of too much food and drink, and is corrupted by its ill concoction; and where it passes beyond its natural limits, it induces some bodily ailment; and in whatever part it settles it becomes corrupted and exhausts and injures the body. Thus corrupt blood causes the mouth to burn, vitiates the breath and makes it stink. For when the blood is healthy it contains the breath [or spirit], and maintains the body so that it may support either heat or cold. But when it is superabundant it produces *sanies* or *bile*, as it is called, which exhausts the spirit, and this gives rise to the bad odour. . . .

"The four parts of the body have their seat in the head, the chest, the belly and the bladder. . . .

"When a disease originates in the head we get aching in that part and the brows are heavy, the temples project, there is ringing in the ears and running of the eyes, while the nostrils are filled with a smell. When any of these occur, the head should be purged with a bunch of hyssop, or conula, or bubala, boiled to the third degree. Water should be held in the mouth and the head kept warm to promote the flow of phlegm. Should this be neglected, trouble in the eyes or teeth, or struma of the parotids, or diseases of the fauces and neck are likely to arise. . . .

"When a disease arises in the chest the head sweats, the tongue swells, or there is a bitter taste in the mouth or the tonsils are painful. There is constant yawning, yet no sleep or rest. There is torpor of the limbs and a disturbed spirit. The body itches, hands and arms tremble constantly, and dry, hacking cough is produced. A bad sign it is when in such a state the patient vomits on awakening (jejunus), or before food, or in the bath. But it is better if when fasting he vomits bile, for bile, I have explained, is the mother of morbidity. . . .

"When a disease arises from the belly these are the signs. The belly is turned and disturbed and suffers much pain, food and drink seem bitter, the knees sink, the loins are heavy, the shoulders are contracted, the whole body aches, the feet drag, legs are weary, the kidneys are painful, and there is fever. . . .

"When a disease arises from the bladder these will be the signs. They will seem full and easily sated, and inflation of the belly will follow. There will be much hiccough and appearance as of yawning. There will be stupor and groaning, and dark-coloured urine difficult to pass, and swelling of the private parts and formation of stone . . ."

The passage here partially rendered into English is of interest as the only known Western attempt of that period to explain in detail the

origin and cause of disease. A century or so later such attempts are not uncommon, but in Western Europe, in the first few years of the twelfth century, this passage stands alone. It is probably taken either from a Salernitan writer or else from Constantine.

The division of the body into four anatomical parts or regions follows naturally on the doctrine of the humours and of the elements. It is a teaching which persisted far into the Renaissance long after the essentially analogical nature of mediaeval science had ceased to provide any intellectual appeal. The principles of the origin of diseases are naturally developed from this idea.

Sections 12-14.—*Panaceas and a Dictionary of Diseases and Remedies.*

The remaining sections we need not discuss in detail. They contain extracts from the extensive Salernitan pharmacopœia, mixed with recipes of native origin, and they appear to form a separate work. Like the compendium as a whole, they illustrate how the influence of the Hippocratic city on the warm shores of the middle sea extended even in the Dark Ages to an island in the Western ocean. We place before the reader a few specimens taken from these sections that he may gain an idea of the mixture of Anglo-Saxon and Salernitan medicine of which they are composed.

A Panacea.—"Against the onset of all diseases. . . . There is a salutary draught which eliminates every infirmity of the body and establishes the health. This draught you should always have by you. Its composition is as follows:—

Myrrhae Feniculi	...	...	...	āā ℥iij
Piperi Anethi	...	...	...	āā ℥i

Crush and mix thoroughly, and then add sufficient clear wine to form a paste. After this, put in a clean cloth, which should be carefully sewn up. Put this in [a mixture of] weak wine and beer and leave to mature for twenty days, or the longer the better. Then remove the cloth and drink daily a measure of the remaining wine before dinner and before supper. If you take this potion you will never ail. Beware however to gather and compound the herbs in a waxing moon, for if you neglect this and the moon be on the wane, the potion will be of nought avail."

The idea of a panacea or all-healing medicine first took shape in Europe in the eleventh century, as has been shown by the learned

work of Mr. Robert Steele.[1] It is not a characteristic either of Greek or of Salernitan medicine, but seems to have evolved in the East under the influence of Arabian alchemy, and slowly filtered through Europe. In our manuscript, however, we have to do with a universal prophylactic or protection against the onset of disease rather than with a true panacea or cure for disease.

Mediaeval literature is full of such potions. The relation of the moon to the course of health or disease is perhaps the most fundamental doctrine of astrology. The reader will recall the passage in Macbeth when the witches cast into their hell broth:

> "Gall of goat, and slips of yew
> Sliver'd in the moon's eclipse." (Act iv, Sc. 1.)

Herbs gathered in a waxing moon were helpful, in a waning, harmful, but most baleful of all were those gathered when the moon was actually in eclipse; this doctrine was early popularized by Pliny.

"The juice of the mustard plant used as a gargle purges the head, pounded with nasturtium it cures yawning.

"Napa [Nepte] seeds pounded with mead [mulsa = honey water] cures variola.

"White mustard pounded up with wine opens the bowels and promotes urine."

These are Anglo-Saxon simples, and similar remedies are encountered throughout that literature. The gargle was a favourite Salernitan remedy, and the word (gargarizo) was probably introduced into the West from Salernitan writings, though it is encountered in ancient times in the works of Celsus, Pliny, and Varro.

"The horse tongue grows in water. Its leaves are round and broad and float on the water. It has a stout root. A hand full of this root, drunk as a potion, dispels fevers."

The Anglo-Saxon medical works contain many short descriptions of plants of this character. Some of the MSS. are illustrated with drawings to help the gatherer. Research has not yet demonstrated the origin of these drawings, which were certainly not made from the objects. It is, however, believed that they were ultimately taken from illuminated MSS. of Dioscorides, of which one magnificent specimen, dating from the fourth century, reposes in the Royal Library at Vienna, and has been reproduced in facsimile.

[1] See *Proceedings*, p. 93.

"For a stye in the eye (ad ordiolum in macula qui super oculos nascitur). Mix barley meal with honey water and place it on. It has been tried by many."

The remedy (farina ordei) is here clearly suggested by the name of the disease (ordiolum).

"Potion for melancholy humour and tears, for jaundice, and for arthritic disease: Take agrimony, root and leaves; betony, root and leaves; nepta; pimpernell; cardopane [?] root; millefoil; pentasilon benedicta; subtusella; apia, root and seeds; petrosilina seeds; petroviola; ambrosia; bladona root; gliconus; coculus; artemisia; fenicle seeds; cardonis fullonicin root; krameria [?]; black mint; sage; feverfew; balsam; sarcocolla. Of each of these a handful. Pound. Add a sextarius of wine; a half calix of honey. Mix and strain. Then add 60 grains of pepper. Take a calix on going to bed."

Here we have a prescription of twenty-seven items, of which an ambitious Salernitan physician might well be proud.

"For hæmorrhoids (ficum) arising in the anus. Burn the head of a hound to a cinder and bind it on."

This passage is certainly derived from the "De medicina animalium" of Sextus Placitus. In that ridiculous work we read: "Caput canis compostum cinis . . . ficus qui in ano nascuntur aspersus sanat & rhagades & omnem spurciciam."[1] Sextus Placitus seems to have appealed especially to Anglo-Saxon readers, and several MSS. are known, containing an English translation, or rather a version of his vain work. One of these dated from the middle of the eleventh century.[2] In it the nearest representative of this passage is the following:—

"Þiđ geswel Þæra gecyndlima hundes heafodpanne gecnucad & to gelegd wundorlice heo hoaleþ."

"For a swelling of the private parts a hound's headpan pounded and applied wondrous healeth."[3]

We will terminate our list of recipes with our author's version of that method of catching birds which so puzzled our childhood. It serves at least to illustrate how low the intellect had fallen in that age.

"To capture birds in the hand. Macerate cheese in the lees of wine, mix the lees with the juice of hemlock and sprinkle on the birds. If one of them tastes it he will be unable to fly."

[1] Gabriel Humelberg, "Sextus philosophus platonicus de medicina animalium," Zurich, 1539, p. 68.

[2] British Museum, Cotton, Vitellius, C iii, also Harleian, 585, and Bodleian, Hatton, 76,

[3] Cockayne, loc. cit., i, p. 370.

The history of medicine consists essentially of a succession of intellectual movements proceeding from different centres and each engulfing its predecessor. Our MS. exhibits the Anglo-Saxon medicine in the actual process of absorption by the doctrines of Salerno. In another century Salerno was swamped in the intellectual tidal wave that spread from Cordova and Bagdad. The Arabian system again gave place at the Renaissance to the revised Greek medicine that came in with the flood of Humanism. Lastly, Renaissance science was in its turn overwhelmed in that great inundation of ideas introduced by the Experimental method. In this majestic procession we have sought to fill one tiny gap.

The treatment of the theoretical aspect of medicine in our MS., absurd and childish as it is, marks a real advance on what had gone before, and is at least superior to the futile list of remedies contained in the works of Apuleius and Sextus Placitus on which the vernacular medicine so largely draws. The activity of the human intellect in Europe probably reached its nadir in or about the tenth century, and it seems unlikely that discovery will reveal material of that period more rational than the document which lies before us. Our MS. shows us that intellectual interest already in the ascendant and preparing for a brighter future.

[*Note.*—Owing to service abroad the writer has been unable to see this work through the press, but Mr. R. R. Steele has very kindly revised the proof sheets for him.]

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

EDITED BY

J. Y. W. MacALISTER

UNDER THE DIRECTION OF

THE EDITORIAL COMMITTEE

VOLUME THE TENTH

SESSION 1916-17

SECTION OF LARYNGOLOGY



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1917

Section of Laryngology.

OFFICERS FOR THE SESSION 1916-17.

President—

T. Mark Hovell, F.R.C.S.Ed.

Vice-Presidents—

James W. Bond, M.D.
Adolph Bronner, M.D.
James Donelan, M.B.
G. William Hill, M.D.
W. Jobson Horne, M.D.
D. R. Paterson, M.D.

Hon. Secretaries—

Cecil I. Graham, F.R.C.S.
F. A. Rose, F.R.C.S.
(Acting for Mr. W. D. Harmer, M.C., and Mr. C. W. M. Hope, F.R.C.S., on Active Service.)

Other Members of Council—

G. W. Badgerow, F.R.C.S.Ed.
Patrick Dempsey, F.R.C.S.I.
Cecil I. Graham, F.R.C.S.
Thomas Guthrie, F.R.C.S.
W. G. Howarth, F.R.C.S.
H. Buckland Jones, M.B.
Irwin Moore, M.B.
E. A. Peters, M.D.
F. A. Rose, F.R.C.S.

Representative on Library Committee—

Dan McKenzie, M.D.

Representative on Editorial Committee—

L. H. Pegler, M.D.

SECTION OF LARYNGOLOGY.

CONTENTS.

November 3, 1916.

T. Mark Hovell, F.R.C.S.Ed. PAGE

Presidential Address 1

Dan McKenzie, M.D., John Mackeith, M.B., and Wyatt Wingrave, M.D.

Cases and Specimens illustrating Work on Atrophic Rhinitis (Ozæna) and Tuberculosis 8

W. Stuart-Low, F.R.C.S.

Three Cases exemplifying some likely Common Causes of Cancer of the Throat, and some Facts about Treatment by Diathermy ... 17

Sir StClair Thomson, M.D.

(1) A Braun's Artificial Larynx worn by a Patient for Five and Three-quarter Years in a Case of Laryngofissure for Epithelioma, followed by Complete Excision of the Larynx 20

(2) War Injury of the Nose to show the Result of Treatment for Stenosis caused by a Bullet Wound 21

(3) War Injury of the Nose to show Result of Treatment for External Disfigurement and Stenosis caused by Shrapnel Wound ... 22

(4) Large Choanal Polypus removed through the Mouth in a Case of Suppuration of the Right Maxillary Antrum 24

Irwin Moore, M.B.

(1) Foreign Body in the Right Maxillary Antrum for Twenty-five Years causing Facial Neuralgia, discovered by X-rays and removed by Operation through the Canine Fossa... 26

(2) Sarcoma of the Right Maxillary Antrum; Lateral Rhinotomy performed (Moure's Operation) 29

(3) Retention Cyst of the Nasal Floor 31

(4) Carcinoma of the Nasopharynx removed by Operation 32

(5) Case of Cystic Goitre 34

H. Lawson Whale, Captain R.A.M.C. PAGE

Two Cases of Fatal Wounds involving the Carotid Vessels and presenting Unusual Features 35

W. M. Mollison, M.C.

(1) Case of Carcinoma of Nasopharynx in a Girl, aged 17 37

(2) Laryngeal Case for Diagnosis 38

William Hill, M.D.

Double Abductor Paralysis 38

H. Lambert Lack, M.D.

Case of Recurrent Ulceration of the Mouth... 39

February 2, 1917.

E. W. Scripture, M.D.

Case of Speech without the Use of the Larynx 41

Frank E. Taylor, M.D., and W. H. McKinstry, M.B., Captain R.A.M.C.(T.)

The Relation of Peri-dental Gingivitis to Vincent's Angina 43

Sir StClair Thomson, M.D.

Four Cases of Laryngofissure for Epithelioma of the Larynx, shown at Intervals of Three and a Quarter Years, Two and a Quarter Years, Ten Months, and Three Months after Operation ... 48

Wilfred Trotter, F.R.C.S., and Sir StClair Thomson, M.D.

Extrinsic Cancer of the Larynx Two and a Half Years after Operation through the Side of the Neck 53

Sir StClair Thomson, M.D.

(1) Epithelioma of Left Maxillary Antrum and Left Ethmoid, Four Years after a Moure's Operation (Lateral Rhinotomy) 57

(2) Round-celled Sarcoma of Right Maxillary Antrum, Ethmoid and Nasal Cavity, Two Months after a Moure's Operation (Lateral Rhinotomy) 58

Cecil Graham, F.R.C.S.

Carcinoma of the Right Maxillary and Ethmoid Sinuses, Seven Months after Operation by an Oblique Facial Route 59

Irwin Moore, M.B.

Carcinoma of the Right Maxillary Antrum : lateral Rhinotomy (Moure's Operation) performed 60

NORMAN PATTERSON, F.R.C.S. PAGE

Epithelioma of the Nasopharynx; Operation; Diathermy 64

W. JOBSON HORNE, M.D.

Specimen of Antro-choanal Polypus 65

IRWIN MOORE, M.B.

(1) A Choanal Polypus, originating in the Sphenoidal Sinus of a Child, aged 6 65

(2) Laryngeal Mirror used by Manuel Garcia, the Discoverer of Auto-laryngoscopy; also the Apparatus used by him to demonstrate the Physiology of the Vocal Cords 71

F. A. ROSE, F.R.C.S.

Hæmatoma (?) of the Posterior End of the Inferior Turbinate ... 73

H. LAWSON WHALE, Captain R.A.M.C., F.R.C.S. (Shown by Dr. DUNDAS GRANT.)

Specimen from a Case of Fatal Hæmorrhage from Gunshot Wound involving the Superior Thyroid Artery 73

F. A. ROSE, F.R.C.S.

Carcinoma of the Floor of the Mouth treated by Diathermy 74

CYRIL HORSFORD, F.R.C.S.

Case of Laryngeal Cyst 75

H. J. BANKS DAVIS, M.B.

Sarcoma of Ethmoid and Superior Maxilla in a Man aged 39 76

March 2, 1917.

J. DUNDAS GRANT, M.D.

(1) Case of Laryngeal Stridor due to Chronic Osteo-arthritis, relieved by Dilatation 79

(2) Case of Symmetrical Fibromata on the Vocal Cords, removed simultaneously by means of the Exhibitor's Forceps 83

IRWIN MOORE, M.B.

Lupoid Tuberculosis of the Pharynx in a Boy, aged 8, affecting the Soft Palate and Uvula 84

HERBERT TILLEY, F.R.C.S.

A Series of Twenty-four Foreign Bodies removed from the Air and Food Passages, with Indications as to the Lessons to be derived from some of the Exhibitor's Experiences 86

HUNTER TOD, F.R.C.S. PAGE

Pin in Bronchiole of Posterior Lobe of Right Lung (read by Mr. Norman Patterson, F.R.C.S.) 90

G. W. DAWSON, F.R.C.S.I.

Ethmoiditis causing Blindness 94

IRWIN MOORE, M.B

Report of Two Cases of Mutton Bones impacted in the Œsophagus; Œsophagoscopy; Recovery... 94

W. H. JEWELL, M.D.

Infiltration and Ulceration of Vocal Cords: Malignant ? 97

May 4, 1917.

T. MARK HOVELL, F.R.C.S.Ed. (President).

(1) Retainers for Tracheotomy Tubes 99
(2) Remarks on Treatment of Irritative Coughing 99

G. W. DAWSON, F.R.C.S.I.

(1) Case of Adherent Soft Palate... 100
(2) Case of Nasopharyngeal Fibroma 101

E. D. D. DAVIS, F.R.C.S.

(1) Case of Right Optic Neuritis caused by Suppuration in the Right Posterior Ethmoidal Cells and Sphenoidal Sinus 103
(2) Case of Left Optic Atrophy caused by Suppuration in the Posterior Ethmoidal Cells, Sphenoidal and Maxillary Sinuses of the Left Side 105

W. M. MOLLISON, M.C.

(1) Case of Acute Osteomyelitis of Frontal Bone secondary to Acute Frontal Sinusitis; Operation; Recovery 106
(2) Case of Carcinoma of Maxillary Sinus, Three and a Third Years after Operation, with no Recurrence 111

HERBERT TILLEY, F.R.C.S.

Skiagram of the Skull of a Patient suffering from Tumour of the Pituitary Body, with Brief Notes of Operation and Result ... 111

DAN MCKENZIE, M.D.

Skiagrams illustrating Spasmodic Stricture of the Thoracic Œsophagus 113

IRWIN MOORE, M.B. PAGE

(1) Retro-pharyngeal Abscess; Injury to the Cervical Sympathetic—an Unusual Complication following Operation by the External Route 115

(2) Syphilitic Disease (Gumma) of the Larynx, treated by Tracheotomy and Galyl Injections, complicated by Arsenical Poisoning; Recovery 118

A. L. MACLEOD, M.B.

Case for Diagnosis 120

L. H. PEGLER, M.D.

Case of Nasal Fibroma (of the Right Fossa) with Specimen and Microscopic Section 121

G. C. CATHCART, M.D.

Case of Aphonia 122

The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers

LONDON:
JOHN BALE, SONS AND DANIELSSON, LTD.,
OXFORD HOUSE,
83-91, GREAT TITCHFIELD STREET, OXFORD STREET, W. 1.

Section of Laryngology.

President—Mr. T. Mark Hovell, F.R.C.S.Ed.

(*November* 3, 1916.)

PRESIDENTIAL ADDRESS.

By T. Mark Hovell, F.R.C.S.Ed.

As all the Fellows of this Section are teachers, it is their views and methods which, in a great measure, mould the opinion and lines of practice of the younger members of the profession who are learning this branch of their work, and therefore, as a Section, a considerable amount of responsibility rests on its members.

Many of the diseases dealt with in this Section are not local in their origin, and it was mainly due to this fact that Sir Morell Mackenzie had to encounter an immense amount of opposition when laying the foundations of the specialty of laryngology. I well remember Sir Richard Quain, who, by the way, was a very shrewd and capable practitioner, ridiculing the idea of diseases of the throat being regarded as a specialty, for he argued that most of them had their origin below the diaphragm, and, of course, there are ample grounds for this statement.

We all know that the commonest cause of functional aphonia in women at or past middle life, and in some cases of young women, is due to displacement of the uterus, or some other affection of that organ, displacement of the uterus being by no means uncommon in unmarried girls, and likely to become still more frequently met with now that they ride astride.

Granular pharyngitis also is not the result of improper voice production, but of derangement in either the gastro-intestinal or utero-vaginal tract or both of them combined, so that dyspepsia, constipation,

or bowel derangement producing looseness, &c., displacement of the uterus, or other uterine affections, and leucorrhœa, all require to be seriously treated if the throat affection is to be cured.

Then, again, the connexion between nasal irritation and bowel irritation is known to almost every old woman, and if she sees a child picking his nose she says without the slightest hesitation, "that child has worms," and her diagnosis is very often correct. As specialists we know that the cases of nasal obstruction due merely to hypertrophy of the soft tissues, which are not relieved by cauterization, in many instances are the result of gastro-intestinal irritation which has been overlooked or insufficiently treated, and it is felt in many cases when this region has been thoroughly dealt with that if this had been done in the first instance, cauterization might have been unnecessary.

Although the facts alluded to by Sir Richard Quain are known to all of us and acted upon, yet the great progress which has been made in the treatment of throat affections could not have taken place if the specialty had not been created. The greatest advance has been made in surgical procedure, especially with regard to laryngo-fissure, the submucous resection of the nasal septum, and operations in connexion with the sinuses contiguous to the nose. Then, again, the improvements in electrical apparatus have enabled foreign bodies to be removed from the œsophagus and air passages, with the aid of direct vision, from a situation which was previously inaccessible without exposing the patient to grave risk.

With regard to the direct method, I cannot help feeling that it would be greatly to the advantage of the younger members who practise in our branch if they were to train themselves also to the indirect method, which is most useful for the removal of papillomata and the treatment of nodules on the vocal cords, as well as other laryngeal conditions. It is quite easy and merely requires the hand to work in accord with the object in the mirror, and with practice the action becomes automatic, the fact that the image is reversed being entirely disregarded.

It is to be regretted that general surgeons do not more quickly adopt the improved methods of procedure introduced by specialists. I have in mind at the moment local anæsthesia to assist the operation of tracheotomy as opposed to general anæsthesia. It is more than forty years since Sir Morell Mackenzie introduced local anæsthesia for this operation, the result being obtained by freezing the skin with a preparation known as "anæsthetic-ether," which was prepared by a firm of chemists named Robbins in Oxford Street. When the skin has been

frozen the trachea can be easily reached with but little discomfort to the patient, who is best seated in an arm-chair. The youngest patient that I remember having operated upon by this method was a boy aged 14. Now that local anæsthesia can be produced by subcutaneous injection of drugs it is to be hoped that this method will become universally adopted, for its advantages are obvious.

Before leaving the subject of tracheotomy I should like to refer to another detail introduced by Sir Morell Mackenzie which greatly adds to the comfort of the patient. It consists in inserting a piece of ½-in. elastic about 3 in. long into the tape on each side which holds the tube in position, the tape being fastened to the tube by means of the snap fasteners such as ladies use for fastening their dresses, and the tape being fastened behind by either tying or having a hole in one tape into which a hook on the other is inserted. But I still see only tape used systematically by general surgeons, which is no improvement in the method of a century ago.

I am one of those who feel that patients with malignant disease ought not to be allowed to suffer unnecessary pain, and therefore mention the treatment employed in the case of a lady by a physician who died a few years ago. He began to relieve her pain with the ordinary doses of morphia, but as she became tolerant to the drug, and the extension of the disease made the pain more severe, he gradually increased the dose until the daily dose given amounted to between 30 and 40 gr., and on days when the pain was more than usually severe the doses given amounted to between 40 and 50 gr. He told me that the patient used to awake, take her food, chat, and be quite comfortable until the return of the pain made another dose of morphia necessary, when she again relapsed into sleep, and thus she was kept to the end comparatively free from pain and in as comfortable a condition as it was possible to keep her whilst suffering from what would otherwise have been a most painful as well as mortal disease. The physician to whom I have alluded, as is evident, did not trammel his actions with the posological table of the Pharmacopœia, but based his methods on common sense, which is the rock bottom of successful medical treatment.

Although curative measures cannot always be disassociated from pain, every care should, I think, be taken to produce as little as possible. At the present time almost every practitioner considers himself competent to operate for adenoid growths, and, from what I am told, there appears to be far more pain following this operation than is necessary. The cause is obvious: the operation is begun before the patient is sufficiently

anæsthetized, and the soft palate as a consequence thoroughly relaxed, and therefore it becomes torn and thus the subsequent pain is produced. If the operator were to wait perhaps only another minute the soft palate would become completely flaccid and could be lifted out of the way. A skilled anæsthetist has no difficulty in maintaining the soft palate in this condition throughout the operation and yet allowing the reflexes of coughing and swallowing to be retained.

When the tonsils are removed there is, of course, slight pain on swallowing for a day or so, but this can be practically abolished by the method which I introduced many years ago, of placing the palm of a hand over each ear with the fingers pointing upwards and then making very firm pressure whilst the patient swallows, the greater the pressure the greater being the relief from the pain. This simple method applies to all cases of odynophagia, no matter what the cause.

I gather that the removal of the posterior extremity of each inferior turbinated body when enlarged is still not universally regarded as an essential part of the operation for the removal of adenoid growths, but I take this opportunity again to repeat, as a result of extended experience, that since I performed this very simple addition to the operation for the removal of adenoid growths I have not had cases of recurrence.

I must repeat that no harm can result from passing a snare through the nostril, even should it not encircle hypertrophied tissue, and that it cannot be ascertained whether the ends are enlarged sufficiently for removal or not by touching them with a finger. I still hear that it is thought that the procedure is difficult, but on the contrary it is extremely simple if the point of the loop is steadied against the side of the choana with the left forefinger, and the snare is held lightly in the right hand and run forward as the loop is contracted.

We all know that it is not only the duty but the privilege of members of our profession to relieve pain, and as all of us are treating, almost daily, cases of granular pharyngitis, we are therefore, aware of the frequency with which girls and unmarried women suffer pain at their periods, as it is a condition which is so frequently associated with this affection, although it is common amongst other individuals who do not exhibit throat symptoms. From the frequency of its occurrence it is evident that throughout the profession there is at the present time a large amount of indifference to this periodical suffering, which we all know at times to be severe, as otherwise patients would not be suffering in this way when they come to us for throat treatment.

I feel, therefore, that we as a Section, having to deal so largely with these cases, may not only teach how to relieve this suffering, but continue to set the example of relieving it. In the large majority of cases it is so simple a matter that it entails writing only one prescription. Drachm doses of liquor caulophylli et pulsatillæ taken every two hours, as soon as the pain at the period commences, or three times a day if there are twinges of pain before the period comes on, not only relieve the pain in the course of a few hours, but if the remedy is repeated with each succeeding period, in many cases the pain becomes a thing of the past and remains entirely absent for years. If liquor caulophylli et pulsatillæ is combined with spirits of chloroform, compound tincture of cardamoms and glycerine, the mixture is not disagreeable. The neuralgic cases are, of course, much more difficult to treat, but fortunately they are in the minority, though most of them only require a little thought as to the selection of the remedies which are tabulated in most pharmacopœias and a proper adjustment of the dose to obtain relief.

There may be gynæcologists and others who will say that treatment of this ailment belongs to their department and does not concern us as throat specialists, but to these I would point out that it is the duty of every medical man to relieve pain and it would not be necessary for us so to treat these patients if they had not been neglected, as otherwise the pain would not exist or they would be in possession of the remedy. I would also remind them that it was the Priest and the Levite who professed philanthropy and goodness, but nevertheless when they saw the man lying wounded and half dead by the roadside, passed by on the other side, and that it was the Samaritan who did not profess to be better than his neighbours who relieved the man's suffering and took care of him.

Although surgery must claim the greatest improvement in the methods of treatment of the affections dealt with by our specialty, yet from time to time medicinal measures are discovered which are of great service, and as I consider it is the duty of all to make them known, I propose to mention a few remedies which I am inclined to think are not as generally known as they might be.

Some cases of loss of smell and taste and of paroxysmal sneezing are relieved by tincture of aconite, which for this purpose may be given only three times a day; it is not an infallible remedy but a useful one to remember. For the relief of the effects of coryza, oil of peppermint volatilized by heat and the fumes inhaled, is recommended by

Martindale in the "Extra Pharmacopœia" in preference to menthol, a free airway being frequently obtained after a few sniffs. Used in this manner it frequently cuts short a nasal catarrh and allows patients to obtain sleep who have previously been kept awake by the blocked condition of the nostrils; it also removes headache when due to a congested condition of the lining membrane of the frontal sinuses. The simplest way to use the oil is to place two or three drops in a spoon and then warm it; this amount will serve several times.

The cases of tickling cough which not only are very common after influenza, but which may follow a simple cold, or occur without known cause, are frequently due to enlargement of the lingual tonsil, the cough apparently being produced by the swollen tissue coming into contact with the epiglottis. A solution of chloride of zinc, 15 to 20 gr. to the oz., with a trace of dilute hydrochloric acid to dissolve the salt thoroughly, is in many cases sufficient to remove the trouble, but I am indebted to Mr. Morley Agar for calling my attention to the best remedy for this condition—namely, trichloracetic acid. A very small quantity only of this drug is required and it is best applied on a wool holder bent at a right angle, with only a very thin layer of wool attached to it so as to ensure the quantity of acid being small. It is best applied with the aid of a mirror so that it can be accurately placed on the swollen tissue.

Enlargement of the lingual tonsil is not usually recognized as a source of throat irritation so far as I am aware, for if this were so the cases of paroxysmal cough which are so frequently met with would not be allowed to continue as they do. I have recently come across a case which I think is pathetic, considering the ease with which a cure has been effected. For fifteen years, a lady, aged 58, has been subject to violent paroxysms of coughing both by day and by night, an attempt to enter into a conversation or certain positions whilst lying down immediately starting the cough, and as a consequence life had become a burden to her. The cough has now almost ceased. When I first saw her the lingual tonsil was considerably enlarged, and from previous experience I felt sure that this was the seat of the trouble. The cough began to lessen after the first application of trichloracetic acid, and as the swelling contracted the greater was the relief obtained, until now the cough is only occasional. Still more recently I have had a similar case in which the cough has lasted for seven years. In this case also the cough diminished as the swelling of the lingual tonsil decreased.

A few months ago, at the repeated requests of many friends, to

whom I had suggested the treatment and who had found it very effective, I sent a short letter to the medical press calling attention to the beneficial effects of garlic in whooping-cough. The remedy, as you know, is by no means new, it being known to most old women and herbalists. Immediately I had done so, I heard of an outbreak at a school, and I at once communicated with the medical officer suggesting the use of garlic. The opinion of all the masters and both matrons is that it had an immediate beneficial effect in many cases, but on some it appeared to have no effect. The garlic was administered by peeling the segments of the root, called cloves, cutting them in thin slices and wearing them beneath the sole of the feet between two pairs of socks, for if worn next the skin the irritation of the juice results in sore feet. One of the masters mentioned that a boy who used to cough, previous to the administration of garlic, until he was, as he termed it, black in the face, after using garlic for two days, no longer experienced extreme discomfort.

I have recently seen two patients who had suffered from whooping-cough in whom the cough persisted, and in each case I found that the lingual tonsil was considerably enlarged. Knowing the manner in which this condition produces violent fits of coughing, it occurs to me that this may be the cause of the paroxysms in whooping-cough. As in the case of paroxysmal cough to which I have just referred, the cough began to lessen after the first application of trichloracetic acid, and as the swelling contracted the greater was the relief obtained, until now the cough is only occasional.

If, therefore, I am right in my conjecture that these violent paroxysms of coughing are the result of enlargement of the lingual tonsil, the terrors of whooping-cough have ceased to exist, for I think, without doubt, the juice of garlic passed into the circulation in the raw condition through the skin destroys the micro-organisms of whooping-cough, and if a strong astringent is applied to the swollen lingual tonsil, the source of irritation which produces the cough will be removed. It is probable that any strong astringent would produce the desired effect, and therefore tannic acid, nitrate of silver, iron, zinc, or any other astringent might prove sufficient; but in all cases, of course, care must be taken to prevent the application from trickling into the larynx, a spasm, however, being quickly dispelled if the patient can be induced to speak —"London" being a word which usually answers the purpose.

I am not in a position to see many patients who are recovering from whooping-cough, but I hope that those who do so will carefully examine

the condition of the lingual tonsil, and if it is found enlarged record their experience of the application of astringents to the swollen tissue.

I must not detain you longer because there are many cases under discussion, but I hope that every member of this Section will record from time to time his experience of remedies which assist in the alleviation of suffering.

(*November* 3, 1916.)

Cases and Specimens illustrating Work on Atrophic Rhinitis (Ozæna) and Tuberculosis.

By Dan McKenzie, M.D., John Mackeith, M.B., and Wyatt Wingrave, M.D.

For a full account of the work done and conclusions arrived at members are referred to the articles which appeared in the *Journal of Laryngology*, May, June, and July, 1916.

(I) Cases of Atrophic Rhinitis treated with Tuberculin.

By DAN McKENZIE, M.D., and JOHN MACKEITH, M.B.

Case I.—Patient, a female, aged 21 (A. P.). Has had atrophic rhinitis of several years' duration, subsequent to cortical mastoid operation. Has had course of tuberculin treatment, from January to September, 1913. In addition to the improvement in the condition of the nose, hearing has greatly improved.

Case II.—Patient, a female, aged 23 (F. K.). Has had atrophic rhinitis for about twelve years. Mother died of phthisis, also an aunt. Had course of tuberculin treatment, October, 1914, to June, 1915. In addition to the improvement in the condition of the nose she does not have "colds" as she used to do.

Case III.—Patient, a male, aged 13½ (B. T.). Had course of tuberculin treatment, March, 1913, to February, 1914, which was followed by violent reactions. No history of tuberculosis in his family. Had enlarged gland (tubercle?) of neck.

Case IV.—Patient, a female, aged 13 (M. F.). Mother died of phthisis. Had four courses of tuberculin treatment: (*a*) July, 1912, to February, 1913; (*b*) June, 1913, to August, 1913; (*c*) October, 1913, to December, 1913; (*d*) March, 1914, to July, 1914.

Case V.—Patient, a female, aged 24 (M. S.). Tubercle bacilli in sputum, May, 1913. No tubercle bacilli in sputum, March, 1914. Had a course of tuberculin treatment, May, 1913, to January, 1914.

Charts and records of the cases are shown.

Mr. Mackeith was solely responsible for the management of these cases during the treatment by tuberculin.

(II) Cases of Atrophic Rhinitis (Ozæna) in Sisters; Acid-fast Bacilli absent; Wassermann Reaction Positive.

By DAN McKENZIE, M.D.

Case I.—R. A., aged 22. Typical appearances of ozæna. Acid-fast bacilli were not found either in the nasal crusts or in the sputum. Wassermann reaction positive.

Case II.—F. A., aged 20. Conditions similar to those of the first patient, whose sister she is.

Since the appearance of the above-mentioned papers fifteen unselected cases of atrophic rhinitis of different types have been examined, with special reference to the acid-fast bacillus—crusts and discharges from the nose being sent to Dr. Wingrave. Of these fifteen cases the acid-fast bacillus was found to be present in eight and absent in seven. Of the seven *negative* cases, two were post-operative cases with very mild nasal phenomena. In three the Wassermann reaction was positive—these include the cases now exhibited. In two, although they presented the clinical appearances of typical ozæna, no evidence either of tubercle or of syphilis could be found. All eight *positive* cases were diagnosed clinically as ozæna. Two were children, aged 7 and 8 respectively, the youngest patients we have so far seen. Another interesting case was that of a girl, aged 13, the tuberculous family taint appearing in her mother, who was a patient at the Central London Throat and Ear Hospital with lupus of the nose. A maternal aunt and a sister of the girl had died of phthisis. In the nose of this patient there was very little crust deposit, but the fœtor was unmistakable. Attention is directed to the negative cases in which the Wassermann

reaction proved positive. This finding may be compared with that formerly reported, in which a partial positive Wassermann reaction was found in a patient who responded negatively to all the tests for tuberculosis.[1]

(III) Specimens of Atrophic Rhinitis (Ozæna).

EXHIBITED BY WYATT WINGRAVE, M.D.

(1) Acid-fast films prepared direct from ozæna crusts: (*a*) coarse type; (*b*) slender type. Stained by (*a*) fuchsin and methyl green; (*b*) fuchsin and picric acid.

(2) Reinforced clumps.

(3) Growth on Dorset egg and fœtid broth medium.

(4) Sections of gland from inoculated guinea-pigs. "Tubercle" bacilli *in situ*. Stained by orthodox Ziehl-Neelsen (alcohol).

(5) Typical examples of discharge, or ozæna crusts (macroscopic).

Histological Changes.—Alterations in the nasal mucous membrane are not so profound as one would expect. The early or exudative stage, when the discharge is fairly abundant and somewhat "creamy," is not attended by any striking histological feature beyond a distinct increase of the lymphoid and vascular elements. Later, however, when atrophy and fœtor are pronounced, there is decided shrinkage in the whole mucous membrane. Lymphocytic and endothelial infiltration of the glands is now replaced by atrophy of the acini and general fibroplastic tissue, all structures—nerves included—sharing the general atrophy. But the most striking feature is metaplasia of the surface epithelium. The columnar and ciliated cells are entirely replaced by many layers of stratified squames which often block the ducts. These changes extend by continuity to the accessory sinuses, the nasopharynx, the larynx, and even the trachea, but they never pass beyond the muco-cutaneous junction. Ulceration or necrosis never occurs, and bone is never attacked. In this respect it differs entirely from lupus, although it somewhat closely resembles lupus erythematosus, with which it has been found in conjunction. Giant cell systems are never seen.

The Discharge.—In early stages it is somewhat free and contains plenty of mucin, but never resembles pus. So that, even at this early

[1] *Journ. of Laryng.*, May, 1916, p. 184.

period, it cannot be called "purulent." Later on, it becomes more and more scanty, loses its mucin, and can only be detached with difficulty. Its dark colour may be due to anthracosis or to various chromogenic bacteria, and its degree will vary with its retention *in situ*. Chemically the lipoids predominate, with cell globulin and keratin granules, much being derived from the metaplastic epithelium, which shows the characteristic acid-fast reaction of epidermal cells. For diagnostic purposes, small portions of the green crusts should be spread with normal saline solution on slides, which are then firmly pressed together and separated by sliding. Fixing may be wet or dry. If the fixing is by heat, this should be radiant, and not exceed 40° C., otherwise the bacilli swell up and become deformed. Weak formol alcohol is preferable, which should be well rinsed with water. Stain by carbol fuchsin on a hot plate for at least ten minutes, wash in 25 per cent. sulphuric acid, and counterstain with saturated solution of picric acid in water, or if other structures are to be shown, use 1 per cent. methyl green in preference to methylene blue. Both of these stains are preferable to the orthodox Ziehl-Neelsen, since they afford much better differentiation. The films cannot be too thin. These stains will show the "acid-fasts" brilliantly retaining the fuchsin. Other films should also be stained in the same way, but no alcohol should be used after the acid bath. They should have at least three washings with acid. Many of the "acid-fasts" will be found to be alcohol-fast as well, even so far as to be "colour true," i.e., they will not take up the counterstain. The picro-fuchsin is the more precise process. A great variety of bacteria, chiefly saprophytes, will usually be seen, but for the present purpose the acid-fasts only demand description. In a well-advanced case the acid-fast will at once be seen, in earlier cases they may require some patience. They are somewhat polymorphic, being long or short, thick or thin, solitary or fasciculated, straight or bent, but nearly always "beaded,"—i.e., not uniform in staining. In size and general appearance they strongly resemble both types of tubercle bacilli and in some films no difference can be found. Much of the variation in thickness is due to the fixing and staining; if heated too much they will swell up like rice or pop-corn, assuming various fantastic shapes; some may be distinctly "clubbed." Tinctorially and morphologically, they therefore closely resemble tubercle bacilli, which also are not infrequently amphophile to the Ziehl-Neelsen stain, and but feebly alcohol-fast.

Cultivation.—So far they have resisted the orthodox methods, either aërobic or anaërobic, but by imitating their natural habits they multiplied

slowly but freely, growing in felted masses resembling streptothrix. A sterile emulsion was first made from fœtid crusts, this was then mixed with Dorset egg medium and "planted" with crust material. Reinforcement by simply inoculating the "crusts" or by adding a few drops of sterile fœtid broth is generally successful in from five to ten days.

Inoculation Tests.—By the courtesy of the Lister Institute several series of guinea-pigs and rabbits have been inoculated. The research is far from complete and is being continued. The results so far obtained are, briefly, as follow: In September, 1915, Dr. Henderson Smith injected six guinea pigs with a reinforced emulsion (five days) made from a typical case of ozæna. No. 1 died during the sixth week, showing well-marked tuberculosis of the spleen. The five surviving apparently healthy guinea-pigs were killed during the tenth week. No. 2 revealed extensive caseation of liver, spleen, retroperitoneal and inguinal glands. No. 3 showed well-marked caseation of the inguinal glands, but the viscera were apparently unaffected. Nos. 4, 5, and 6 appeared to be quite healthy. Nos. 1, 2, and 3 were injected subcutaneously, Nos. 4, 5, and 6 intraperitoneally. Histological examination of the caseated foci showed characteristic tubercular lesions, there being giant cell systems with true tubercle bacilli, both intra- and extracellular. The bacilli were strongly alcohol-fast and of the slender type. Dr. Henderson Smith reported: "The smears from the spleen nodules and the glands both showed acid-fast bacilli very like tubercle. I have very little doubt they are tubercle. I was interested to find that you obtained histological tubercle in the sections." Emulsions were then made from the tuberculous viscera of Nos. 2 and 3, with which three guinea-pigs were inoculated on the day of the necropsy. No. 1 died in the eighth week, No. 2 died at the twelfth week, both showing extensive gland caseation with "bacilli both acid and alcohol-fast, obviously tubercular." Guinea-pig No. 3 was mislaid. Nos. 1 and 2 were injected subcutaneously, No. 3 intraperitoneally. In each group of tests those injected beneath the skin developed lesions, while the animal injected intraperitoneally escaped. There were no pulmonary lesions in any one of them, nor could any intranasal change be found. Excluding one animal which was lost, *five out of eight developed lesions which were indistinguishable from tubercle.* In each test the strictest precautions were taken against contamination at every step, from collection of the material at the hospital to the final necropsy.

A stage has therefore been reached which must not be considered conclusive, although collectively the evidence may be viewed as

strongly presumptive. It has been established that certain organisms, morphologically and tinctorially resembling tubercle bacilli, are present in ozæna which we have never found in any other nasal disorder.

DISCUSSION.

Mr. HERBERT TILLEY: We should not allow this occasion to pass without expressing our thanks and congratulations to Dr. McKenzie, Mr. Mackeith and Dr. Wingrave for the excellent piece of work which they have done in connexion with the relationship between atrophic rhinitis and tubercle. Probably most of us have read their monograph which was published in the *Journal of Laryngology*, and since then when dealing with cases of atrophic rhinitis have laid more stress on getting all the details of the family history from the point of view of tubercle. In many cases I have been surprised to find such a history in the forebears. The question, however, has arisen in my mind whether, if we were to investigate the family history of any one of us here, we should not be able to find some relative who had had tubercle. But I think the percentage in cases of atrophic rhinitis would be greater than in any other form of nasal disease. I have carefully examined the cases exhibited to-day, and, with the exception of the little girl—who has not had many injections, and would scarcely yet be claimed as a success—the nasal cavities seemed to be in a very good condition. Presuming that they have had no other treatment, the result of the tuberculin injections has been, on the whole, better than in the other methods we have been in the habit of using. Perhaps it is as yet too early to speak of what the ultimate result is likely to be; but whatever may be the result, whether positive or negative, I cannot withhold my admiration for the useful work which these gentlemen have done.

Dr. WATSON-WILLIAMS: I should like to add my thanks for the monumental investigation which has been undertaken by Dr. McKenzie, Mr. Mackeith and Dr. Wingrave. It requires a great deal of enterprise to start an investigation on this subject nowadays, which has been the cemetery of many laborious hours; and we can only hope that their conclusions, which certainly seem warranted, will prove to be justified. No one has taught us more than has Dr. McKenzie himself to be very cautious in arriving at conclusions, and I think we cannot read these papers without feeling that they are the product of balanced minds. Dr. McKenzie has at least given us food for reflection, and has gone a long way towards leading up to some conclusive evidence in the treatment of the disease, the *bête-noire* of laryngology. He has so far carried me with him in his way of thinking that I shall give his methods a careful trial.

Dr. DUNDAS GRANT: I also feel very strongly, with those who have already spoken, that we have had here an honest endeavour to get further towards the truth of the conditions known as ozæna and atrophic rhinitis. A weak point

in the work lies in the fact that Dr. McKenzie has taken the terms "ozæna" and "atrophic rhinitis" as identical. Ozæna without the peculiar smell is scarcely worthy of that name. Ozæna does certainly occur in subjects of pulmonary tuberculosis, though seldom; whereas chronic atrophic rhinitis with crusts, dryness, and pus, is common. If you put the two together under the heading of ozæna for statistical purposes, the result will be to invest ozæna as such with a degree of anxiety for the unfortunate sufferers from it which, I think, is not justified. I would remind you that for years the French have spoken paradoxically of an ozæna without smell, and they assert that, as a rule, "ozæna without smell" is generally tuberculous. It is probably lupoid in character, and might react to tuberculin. Apart from that, the results which have been obtained in the cases before us are worthy of every consideration. I should be glad if members would express their opinions as to whether ozæna and chronic atrophic rhinitis should be classed together for the purpose of drawing statistical deductions of either pathological or clinical import.

Mr. F. A. ROSE: I should also like to congratulate Dr. McKenzie and his colleagues on this excellent piece of work. I also want to ask a question. I am not clear, after reading the notes, whether it is claimed that these acid-fast bacilli are in the tissues of the nose, or only in the secretion inside the nose.

Dr. DAN MCKENZIE: I pointed out in my paper that we have never yet succeeded in finding these bacilli in the tissues of the nose.

Mr. F. A. ROSE: Assuming that the bacilli are present in the secretions only, is it not possible that they are there only by accident? A nose which is the subject of atrophic rhinitis is one which cannot protect itself from infection, so that all the bacilli in the dust in the streets may be deposited there. And if the patient only lives among enough acid-fast bacilli, there is no reason why those should not be found in the nasal secretions. We know that if a careful search be made for diphtheroid bacilli they are found in 90 per cent. of cases of atrophic rhinitis; and it is impossible not to find streptococci, staphylococci, and *Micrococcus catarrhalis*, and other organisms. The question is, what relation have these bacteria to the disease?

Dr. JOBSON HORNE (commenting on the statement in the paper, "It has been established that certain organisms, morphologically and tinctorially resembling tubercle bacilli, are present in ozæna which we have never found in any other nasal disorder") said: If I may be permitted, I would refer my listeners to the "Descriptive Catalogue of the Museum of the British Congress on Tuberculosis," held in London in July, 1901. On that occasion I was fortunate in bringing together a representative collection of cultures of bacilli, acid-resisting and morphologically and tinctorially resembling the tubercle bacillus. Amongst these was one isolated by Karlinski (Maglaj, Bosnia) from nasal secretion. Perhaps Dr. Wingrave will tell us whether the bacillus to which

he refers has not been isolated before, or whether it is perhaps the same one which was isolated some years ago from the nose. The case for tuberculosis will stand or fall on the animal experiments, but they are, as the authors admit, far from complete, and must not be considered conclusive. Even a negative result will be valuable in elucidating the facts about ozæna. The question is whether this acid-resisting bacillus is merely an accessory to the crusts and discharge or whether it is a factor in the production of the disease. I am inclined to the former view.

Sir StClair Thomson: All research work is to be admired, even when the results are negative. I am not qualified to join in this debate, because I also have, so far, neglected to read these communications *in extenso*. But of the cases I saw in the next room none was free from suppuration, and more than one had the ozænatous odour. In connexion with tubercle I do not know whether it is of value to this debate, but I have had exceptional opportunities of seeing cases of tubercle, and for some years I have interested myself in a clinical research which I hope to publish some day: I mean the condition of the nose and pharynx in persons who have, or have not, tubercle of the larynx. I have quoted that there is a sort of *a priori* statement that the patient with stenosis of the nose, or other trouble in his nose or in the pharynx, is more likely to get tubercle, and tubercle of his larynx, than is anybody else. I see the throats of 300 tuberculous patients per annum, and I have been doing that for five years, and the result has been, to my surprise, that patients with tuberculosis of the chest or the larynx have got no more marked nasal deformity from disease than has the healthy individual, and that such affections of the nose as sinusitis or ozæna are no more common in sanatoria than they are anywhere else.

Dr. Brown Kelly: I think we are indebted to Dr. McKenzie and his co-workers for so thoroughly investigating the nature and treatment of ozæna, our ignorance of which is not creditable. I regret that my own work on behalf of the International Collective Investigation of Ozæna has apparently come to nought. The only part which proved of practical benefit was that in connexion with the employment of a vaccine of Perez's bacillus. This vaccine is undoubtedly specific in some cases; it causes the fœtor to pass off, the nose to discharge more freely, and the crusts to disappear. Some of the cases which I treated in 1913-14 still remain cured. Perez's bacillus can be produced by injecting a mixed culture of ozæna crusts into the marginal vein of the ear of the rabbit. The animal usually dies in a day, and the bacillus can be recovered from the turbinals. If the animal survive, it has a chronic nasal discharge, and if it be killed later, the anterior turbinal is atrophied.

Dr. D. R. Paterson: I have also taken part in a collective investigation into ozæna, and have received a definite impression of a connexion in family histories between tubercle and ozæna. I examined a large number of school children, confining my attention to cases in which the characteristic smell

existed, and was struck by the frequency of the association. I have long noted the liability of ozæna patients to perish of tuberculosis.

Dr. DONELAN: Would it not be well for the Section to avoid using the term ozæna in cases where there is not the characteristic smell? Should we not speak of ozæna simply as a symptom occurring in atrophic rhinitis, or are we to run the risk of having it cropping up at every moment as an alternative disease? I understand we are dealing with atrophic rhinitis. Whether it be due to the bacillus as worked out by these investigators, or not, remains to be seen. Several of the cases shown to-day seemed to be at least secreting, if not actually suppurating, and one woman has the distinct ozænic symptom, and appeared to have some suppuration in the left sphenoidal sinus.

Dr. DAN MCKENZIE (in reply): On behalf of my fellow workers and myself, I thank those who have spoken for the very kindly way in which they have treated our attempt at research. The criticisms which have been expressed we not only expected, but have actually experienced in our own minds. I began this research with the idea that I should arrive at a negative finding. When I began I thought there was only an accidental connexion between tuberculosis and ozæna. As time went on, however, my experience brought me round to the belief that there is a real connexion between atrophic rhinitis and tuberculosis. And when I use the words "atrophic rhinitis" in connexion with this research, I mean ozænatous or fœtid atrophic rhinitis. The items in detail have been dealt with in our papers and there is no need to recapitulate the grounds for our belief. We are so convinced by our experience that we say we are content to leave the results of our research to time; we believe that in fifteen to twenty years general opinion will agree that ozæna is associated with tuberculosis in this respect, that they are part and parcel of one pathological process, that they are related as pathological entities. With regard to the point raised by Mr. Rose, as to those bacilli found in the crusts being due to accidental contamination from the air, that is answered by the fact that we have searched for this bacillus in other diseases with crust-formation and have not found it in them. It only occurs, as far as we have gone, in those which show the clinical signs of ozæna.

(*November* 3, 1916.)

Three Cases exemplifying some likely Common Causes of Cancer of the Throat, and some Facts about Treatment by Diathermy.

By W. Stuart-Low, F.R.C.S.

Case I.—A man, aged 52, who has been before this Section on two former occasions with a rapidly growing epithelioma of the soft palate. It began midway between the uvula and the base of the anterior faucial pillar, and rapidly and superficially spread to the uvula and middle line, and appeared so unusual that experienced observers mistook it for a syphilitic condition. When first seen in February, 1915, the growth was only the size of a sixpenny-piece, then it rapidly involved the anterior faucial pillar and uvula. Dr. Wyatt Wingrave pronounced the specimen removed to be a rapidly growing epithelioma. Pain has been very severe from early in the history of the case, and hard glands at the angle of the jaw also appeared early and caused an unusual degree of pain. The history of syphilis was proved, and the Wassermann test was positive. At our meeting in March attention was drawn to his habit of drinking scalding hot coffee. When he first came to the hospital his mouth was in a very septic state from septic gingivitis, dental caries, and pyorrhœa alveolaris. The chemical reaction of the fluids in the mouth and throat were distinctly acid. Dentition was defective. He has always taken a large quantity of common salt with his meals, and been a very heavy smoker. Two weeks ago all the uvula and palpably affected tissue were dissected away under a general anæsthetic, laryngotomy first having been performed and the larynx firmly plugged. The diathermy cautery was then freely applied to the surface of the exposed tissues, and punctures made into the freshly exposed parts at various spots. The method of diathermy puncture of malignant glands, first practised by myself, was then employed—viz., the hard mass of the glands having been exposed by freely reflecting the soft structures, the diathermy point was pressed into the glands repeatedly at various points, and the skin replaced and stitched up. Healing by first intention invariably resulted as in this instance.

Case II.—A man, aged 54, a railway guard at Basingstoke, was sent to the clinic a month ago. He only complained of increasing inconvenience in the throat and of some difficulty in swallowing. Pain was almost absent except at night, and there were no enlarged glands to be felt. The growth had affected the whole of the soft palate, which was largely destroyed, and what remained was sloughy and craggy when touched by the finger. It had also extended to the tonsillar region on both sides. The implication of the palate was very similar to that in the case just described, but the absence of pain and glandular involvement was in marked contrast. Wassermann's reaction was found to be positive, and there was a clear history of syphilis. He had always been a heavy smoker, often consuming 1 oz. of tobacco a day. The mouth was very septic from badly cleaned teeth, septic gingivitis, and large accumulations of septic tartar. The saliva was very acid. There were many carious teeth and dentition was defective, and he had always taken quantities of common salt with his food, and been fond of hot food. Three weeks ago laryngotomy was performed, and the larynx plugged, and the whole of the affected parts cut away; the tonsils which were affected being dissected out. Diathermy puncture was freely applied to the surface of the wound, and the point pressed into any parts that felt unusually resistant or firm on examination with the finger. A week later the diathermy puncture was again applied to a few sloughy looking parts. Dr. Wyatt Wingrave found the growth to be a rapidly growing variety, and a specimen is exhibited under the microscope. It is peculiar in showing many large glands in its structure.

Case III.—A man, aged 45, who came to the clinic last May, complaining of increasing difficulty in breathing; the history was that he had been affected and under treatment for six months previously. A large mass of fungating growth which proved to be epitheliomatous was seen to be implicating the right vocal cord and ventricular band. A low tracheotomy was performed. He was found to have a positive Wassermann reaction. He had a very septic mouth with acid reaction, and he had smoked a great deal. Diathermy puncture has been done, the laryngeal growth having been punctured regularly at intervals of a week both by the direct method and in the suspension position.

Diathermy has been proved to be very helpful in these cases and in a number of others in which it has been employed. It undoubtedly relieves the pain both internally in the throat and in the neck and head

after puncture of the masses of glands by the method I introduced. It seems to be best to repeat the application at intervals of a few days, and not to apply it at any one time too extensively or severely. The growth is not only destroyed in places, but grows more slowly at the deeper sites where cure is not effected nor possible. Diathermy should be employed as early as possible in the history of such cases. In more advanced cases sloughy portions of the tumour are got rid of after the puncture, and in this way fœtor is lessened and sepsis of the mouth and throat diminished.

As regards contributory causes of cancer, syphilis has been present in every case that the exhibitor has investigated. Sepsis of the mouth, generally in an aggravated form, has invariably existed, and the saliva has been found to be quite acid in reaction, and excess of common salt has mostly been taken. Dental deficiency seems also to be a contributory cause, leading to the food being improperly masticated, and thus irritating or wounding the pharynx. It has also been noticed that these patients accustom themselves to taking very hot food.

DISCUSSION.

Dr. WILLIAM HILL: The results are very good. But we see the cases rather early, and it is desirable we should see them later as well. I have had some experience with diathermy, thanks to the work which has been done at St. Bartholomew's Hospital, and it seems to be very useful. But the plan of dealing with glands is not one upon which I have embarked, and I ask whether it is worth while to diathermize the glands, as Mr. Stuart-Low has done; I have only carried this out in my cases to relieve pain. I think it is better for the patient after these extensive excisions; he is more comfortable, and the scarring is not so great. But you must be prepared for tumefaction of the pharynx and trachea, although Mr. Harmer said he did not meet with this condition, which is one that may necessitate temporary tracheotomy. It is well to remember that in diathermy a little more tissue is always destroyed than is apparent at the time.

Mr. NORMAN PATTERSON: I think that in this case treatment of the glands by ordinary surgical methods would have been eminently satisfactory. It was not an inoperable case and removal by dissection was preferable to applying a cautery.

Mr. STUART-LOW (in reply): It is a method which I use after employing the knife first. By that means one is enabled to see exactly what one has done, and little harm is likely to result from the diathermy.

(*November* 3, 1916.)

A Braun's Artificial Larynx worn by a Patient for Five and Three-quarter Years in a Case of Laryngo-fissure for Epithelioma, followed by Complete Excision of the Larynx.

By Sir StClair Thomson, M.D.

The patient, J. S. P., was aged 43 when he came under my observation in February, 1909. The record of his medical history is as follows: 1909—February 25, laryngo-fissure; May, stenosis started in glottis; November 28, tracheotomy; December 6, excision of larynx; 1915—November 5, gland in neck; 1916—August 18, died.

N.B.—No local recurrence.[1]

DISCUSSION.

Mr. E. D. D. Davis: I should like to ask whether members have had experience of Braun's tube. I have a patient of Major Waggett's under my care, who has worn a Braun's tube for about seven years, and he complains that saliva escapes into his trachea, causing violent attacks of coughing, so that the passage between the trachea and the pharynx is a disadvantage, and he has several times asked to have it closed. He speaks in a toneless whisper, which it is difficult for a stranger to understand, and speaking is laborious for him. The history of the case is interesting. In 1903 Sir Felix Semon did a laryngo-fissure. There was a recurrence in six weeks. Major Waggett and Mr. Stabb did a second and extensive laryngo-fissure three months after the first operation. He did well until 1907, and then Major Waggett and Mr. Stabb performed a laryngectomy, and he has worn a Braun's tube ever since. The result is very encouraging.

Sir StClair Thomson (in reply): The patient was able to breathe both through his mouth and through his neck, so he obtained a double supply of air. Indeed he was able to bicycle. When sitting at home and not wanting so much air, he put the stopper in his neck, and so had a good rough whisper speech. This tube also enabled him to enjoy a smoke. He did not complain of the tube leaking, and he lived a useful and happy life for five and half years. The question of recurrence is important. I did laryngo-fissure in February. The patient was getting some stenosis in his glottis by May. Sir Henry Butlin saw the case in consultation, and he could not make up his mind. But by

[1] Case published in full in the *Brit. Med. Journ.*, February 17, 1912.

November the man had such a degree of stenosis that I had to do tracheotomy in a hurry. Sir Henry Butlin saw him again, and said: "Take out the whole larynx." I took it out, and the man did well. But when the larynx was examined pathologically, there was no evidence of malignant recurrence. (This was before the days of the Wassermann test. Later we found that the patient was an old syphilitic subject.) I still have the larynx, if any further investigations are required. This was a great blow to me, because tracheotomy would probably have done just as well. Sir Henry Butlin told me not to worry, as the man was probably much better without his larynx. Now, it is to be noted that five and a half years later this patient came back for cancer in the glands of the neck, in spite of excision of the larynx. But there was no recurrence in the throat. To his last day he breathed comfortably, dying of asthenia owing to recurrence in the glands of the neck. People who are not versed in laryngo-fissure say: "If a man has malignant disease at all in his larynx, take out his whole larynx." But here is a man who had his larynx taken out in very good time, yet that did not prevent recurrence five and a half years afterwards. Laryngo-fissure, in suitable cases, gives better results in cancer than any other operation; and the case shows that even a complete laryngectomy is not an infallible guarantee against recurrence.

(*November* 3, 1916.)

War Injury of the Nose to show the Result of Treatment for Stenosis caused by a Bullet Wound.

By Sir StClair Thomson, M.D.

Captain C. R. V. J. received a bullet wound in January, 1915, which penetrated the nose from the right to the left side. When he came under observation fourteen days later there was almost complete stenosis from adhesions between the septum and outer wall in each nasal chamber. It was also clear that the patient had a very deviated septum. On August 19, under chloroform, I performed a resection of the septum, which was very irregular, having evidently been comminuted by the bullet. Several pieces of cartilage were replaced, as has been my custom for many years. The adhesions were then divided, and rubber sponge plugs introduced. There has been some tendency to scar contraction, chiefly on the floor and roof, which is so common in the nose and larynx after these war injuries. This has been corrected by the persevering use of Lake's rubber splints, which the patient is still wearing.

(*November* 3, 1916.)

War Injury of the Nose to show Result of Treatment for External Disfigurement and Stenosis caused by Shrapnel Wound.

By Sir StClair Thomson, M.D.

Lieutenant F. H. B., aged 21. On May 17, 1915, while in a trench at Givenchy, a shrapnel burst over him and wounded him in the nose. I was asked to see him in consultation at the Chichester Hospital in June, 1915. The wound on the external nose had been so badly put together in France that the result was an unsightly disfigurement, as is well shown in the two photographs exhibited. There was no respiration through the left nostril, and the right was so occluded with adhesions that it admitted only a fine probe. In addition, the shrapnel had produced a condition like harelip, and a fracture of the superior maxilla. I suggested a plastic operation to Dr. D. Ewart, of Chichester, and a photograph, as well as the patient's present condition, will show how very admirably he carried out this delicate piece of surgery. The patient was transferred to my care in London in January, 1916. As the nasal respiration, although improved, was still unsatisfactory, the adhesions were divided under chloroform, and I found the septum so fairly plumb that it was left alone. Since then he has been persevering with the use of Lake's rubber splints, with the result that he has now a good thoroughfare, although there is still the usual tendency in these cases to some return of cicatricial stenosis.

DISCUSSION.

Dr. Dan McKenzie: I suggest diathermy in treatment of these adhesions. Has anyone tried to graft with cellulose? I thought those who have had experience with war cases might have tried it.

Dr. Brown Kelly: I have seen five or six cases in which a bullet traversed the bony bridge of the nose without causing trouble. In others, however, in which the projectile traversed the lower part of the nose, almost hopeless damage resulted, owing to the inferior turbinates and septum being united. If these cases are not seen within ten days of being wounded, thick synechiæ form. I think that is one of the reasons why there should be rhinologists at the Front.

Dr. DUNDAS GRANT: The plan of treatment is so good, and it is so easily carried out in a number of cases, that there is scarcely any call for improvement upon it. After doing the submucous resection of part of it, I find ordinary cotton wool smeared with vaseline is good, and better still is a little indiarubber finger-stall put upon the blades of a Killian's long nasal speculum, pushed through and packed with gauze. It can be pulled out without the slightest discomfort.

Mr. HERBERT TILLEY: I should like to ask Sir StClair Thomson what are his reasons for preferring Lake's rubber splints. I have given them up for the past seven or eight years, because patients complained of neuralgia when these splints were used. I have since employed thin celluloid plates which cause no discomfort: they also leave room for nasal breathing, and prevent the opposing parts coming into contact. If the rubber splints are used because they are thicker, and give support, I would suggest the use of a gauze wick wrapped round with oil silk: they can be made of a size to fit the part, mould themselves to the surroundings, and can be removed without any pain.

Dr. PEGLER: Those who can look back far enough will remember that what is called a Lake's splint was a later invention, and that for some years previously he used indiarubber sheeting, cut to required shape, and adapting thickness and size to suit the particular case. I employed them very successfully in a case of a soldier whom I showed early last session, who had had very bad traumatic stenosis, and had required two or three operations for its relief. If Mr. Tilley has had to complain of neuralgia in his cases, it is probably because there was a want of due proportion between the passage desired to be kept open, and the size, particularly the thickness of the splint. The adaptation must be perfect, neither too tight nor too loose; never loose enough to work back into the nasopharynx. How my confrères manage to cure synechiæ cases requiring lengthened treatment without such a splint of rubber I do not know. Lake's ready-made article, though occasionally one may find a size and shape adaptable to a special case, has never appealed to me, and I therefore make a point of keeping a stock of sheeting of various thicknesses by me, and cutting out the kind of thing I happen to require at the time.

Mr. J. F. O'MALLEY: It is necessary to leave in the substance used for ten days. I do not cut through with scissors, but get in Grünwald's forceps as wide as the nasal passage which is clear of the nasal synechiæ, and in that way one establishes the complete width of the nasal passages as they existed before, and then one applies a piece of rubber which is loosely fitting. [Dr. PEGLER: We all use Grünwald's forceps.] I avoid using a local anæsthetic previously, so as to ensure that one is dealing with the actual size of the nose.

Dr. WILLIAM HILL: Captain Hastings has contrived ingenious devices for these cases; he has made large use of ordinary hairpins with thin indiarubber tubing, attaching them to the forehead. This prevents depression of the nose.

I have been much impressed by the delicate work he has done in preventing deformity from both the tip and the bridge of the nose falling in. But there may be deficiencies which have to be supplemented by plastic flaps. Mr. Hern, dentist to the same hospital, has also some ingenious devices.

Dr. DONELAN: I should like to support what Dr. Pegler has said about india-rubber sheeting. For many years I have used it of the thickness of surgical gloves; in fact, in the hospital we use up old surgical gloves for this purpose. In treating synechiæ one can pack it exactly as wanted. Mr. O'Malley's suggestion of taking out the adhesion entirely with the forceps is an important element in the treatment. If you put in rubber sheeting, and pack nicely with gauze, the clearance is maintained; in two or three days you can take out the dressing and put in a fresh one, and so maintain the separation as long as necessary during healing.

Sir STCLAIR THOMSON (in reply): With regard to the cosmetic result in the second case, Dr. Ewart, of Chichester, is responsible for it. The patient was wounded at Loos, and his nose was put back on to his face so badly that he was almost hideous. He was engaged to be married, and his fiancée begged the hospital staff to take away some of the ugliness, and you can see in the photograph how excellent a result Dr. Ewart has achieved. With regard to the stenosis, the bullet went through high up, and much of the stenosis occurred at the top. There was a constant tendency to closure at the top, and I used Lake's splints simply because I was brought up on them! I have used a celluloid plate, but that was when the adhesions were *vis-à-vis*—one on the septum and one on the turbinals—and it was only necessary to keep the surfaces apart. I agree that Lake's splints are rather unpleasant—one patient has had to wear it since January, and the other a year. One patient had a splint made out of aluminium, and it acted admirably.

(*November* 3, 1916.)

Large Choanal Polypus removed through the Mouth in a Case of Suppuration of the Right Maxillary Antrum.

By Sir STCLAIR THOMSON, M.D.

THIS large growth was removed on October 25, 1916. The patient had noticed a polypus in her right nose for the last ten years, and it had been twice operated upon, under chloroform. The polypoid mass blocked up the right nostril, and hung down into the post-nasal space, below the level of the soft palate. It was plucked out through the mouth. The ethmoidal region appeared normal. The cause of previous failures is doubtless due to the fact that the right antrum had not been operated on. I found it full of fœtid pus.

DISCUSSION.

Dr. IRWIN MOORE: At the meetings of the Section, on May 7 and November 5, 1915, I reported in detail the case of a "Choanal Polypus originating in the Right Maxillary Antrum of a man aged 42."[1] Since the post-nasal portion of this polypus was fixed by inflammatory adhesions to the choanal margin, it was impossible to grasp and remove it by forceps, or snare through the nasopharynx. I therefore opened the antrum through the canine fossa, and removed with forceps first the pedicle, which was soft and necrotic, next the nasal portion through the anterior naris, whilst the projecting nasopharyngeal portion was removed through the mouth. This case supports the view first suggested by Killian that the maxillary antrum is nearly always the seat of origin of these polypi, also it shows that these growths may be subject to inflammatory and necrotic changes.

Dr. D. R. PATERSON: How young are the patients in which such cases occur? A fortnight ago I saw a child, aged 5, who was very ill with a large bulging swelling in the nasopharynx, and it was said to have been there only three weeks. Before touching it, as the breathing was much impeded when the patient was under a general anæsthetic, I did a tracheotomy, and then attacked it. I found it was fairly firm, and on puncturing it with a syringe I got nothing out of it. I passed up a post-nasal forceps, and twisted it out. It turned out to be a large nasopharyngeal polyp, originating in left nostril, but the parents seemed positive that there had been no symptoms of nasal obstruction until about three weeks previously. That it had been growing a long time was evident from the pressure effect on the left posterior naris, which was much enlarged.

Dr. BROWN KELLY: These polypi, which I think should be called naso-antral polypi, are common. If a polypus is present in the posterior part of the middle meatus, and, on snaring it, there is a discharge of translucent liquid from the nose, it is almost certainly a naso-antral polypus. These polypi are usually single; they may attain so great size as to hang down into the pharynx; and they occur in children, as Dr. Paterson has said. With very rare exceptions, polypi in children under the age of 10 come from the antrum. On removing these naso-antral polypi, no bleeding-point can be seen in the nose which would indicate their seat of origin. A misleading feature is that transillumination shows the antrum on the affected side to be brighter than on the presumably healthy side. Cysts of the lining membrane of the antrum are often associated with accessory ostia, and it is through one of these that the polypus escapes from the antrum.

Mr. FRANK A. ROSE: In answer to Dr. Paterson, I would confirm Dr. Brown Kelly's remarks. In my experience, a polypus in a small child is invariably single, and of the antro-choanal type. One exception to this

[1] *Proc. Roy. Soc. Med.*, 1915, viii (Sect. Laryng.), p. 104; also 1916, ix, p. 15.

occurred in a patient aged 9. With regard to the results of transilluminating the antra, the one on the affected side is frequently brighter than the other. I had the curiosity to take a patient back, after the removal of the polypus and transilluminate again; the antrum was then dark, on account of the blood which was escaping from the root filling the antrum.

Sir StClair Thomson (in reply): I am glad to have originated this discussion, because we have Dr. Brown Kelly here, and when I wrote the chapter in my book on this subject it was chiefly founded on his work and publications. I should have liked to have asked him and the other speakers whether it is necessary to do an operation on the antrum in these cases. This patient had fœtor and pus, and of course I did an endo-nasal operation. I have removed these growths from children, in whom I do not know whether there has been recurrence or not. [Mr. Rose: Recurrence, in my experience, is uncommon.] I think there is no need to operate on the antrum unless it is diseased, or unless there is recurrence.

(*November* 3, 1916.)

Foreign Body in the Right Maxillary Antrum for Twenty-five Years causing Facial Neuralgia, discovered by X-rays and removed by Operation through the Canine Fossa.

By Irwin Moore, M.B.

Patient, a lady aged 67, was brought to me by Mr. Millican, L.D.S., of Surbiton, with a history of facial neuralgia for twenty-five years. While residing in the West Indies in 1891 she had a right upper molar extracted; this was followed by very severe pain under the right eye, from which she has never been entirely free. In 1897 she had a nervous breakdown, and a "lump" was said to have been felt over the right maxillary antrum, accompanied by much pain and tenderness. In consequence the remaining upper molars were extracted. In 1901 the patient came to England and consulted a throat specialist, without any benefit. She returned to the West Indies, seldom free from pain, with occasional exacerbations. In 1911 she went to New York, and had the right infra-orbital nerve resected. In 1913, while residing at Bexhill, following another acute attack of pain she noticed that the "lump" was loose and moved about. Recently she has been treated by means of alcoholic injections. In September, 1916, she consulted Mr. Millican, who extracted the two upper central incisors which were loose and



FIG. 1.

Radiogram showing the piece of metal in the right maxillary antrum.

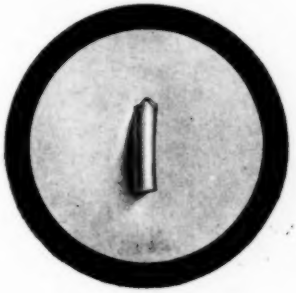


FIG. 2.

The piece of aluminium.

septic, and took her to Mr. Charles A. Clark to be X-rayed. The radiogram showed a foreign body in the right maxillary antrum lying against the nasal wall. It appeared to be cylindrical in shape, about 1 in. in length and $\frac{1}{8}$ in. in diameter.

On October 12 I was asked to see the patient in consultation. There was no history of nasal catarrh or suppuration, and both nares were found to be perfectly healthy and normal. On inspecting the radiogram (fig. 1) it was apparent that a rod-shaped body was present in the right maxillary antrum and I advised that the antrum should be opened and a search made for the foreign body. Two days later I opened the antrum through the canine fossa and found the piece of aluminium now shown lying loose in the angle formed by the meeting of the posterior and inner walls. The lining membrane of the antrum was found to be perfectly healthy. The foreign body was cylindrical in shape, and consisted of pure aluminium, having a length of 1·34 cm. = 0·524 in. (just over $\frac{1}{2}$ in. in length), and a diameter of 0·29 cm. = 0·135 in. The specific gravity was 2·666 and the weight was 0·24 grm. = 3·69 grains. One extremity was smooth, whilst the other appeared to have been cut by pliers (fig. 2). This piece of metal had apparently been buried or fixed for twenty-five years, and (as suggested by the patient's symptoms) only became loose and movable in the antrum three years ago.

This case is of great interest as showing that: (1) The maxillary antrum will tolerate a foreign body for twenty-five years without causing catarrh or suppuration, the usual accompaniment of foreign bodies in these cavities. (2) The shadow of this rod-shaped body could be distinctly seen when the antrum was transilluminated. (3) The appearance of the aluminium foreign body (according to the opinion of expert radiographers) was not compatible with its being metal. (4) Cases of facial neuralgia may be treated for years without any suspicion of being caused by antrum trouble. (5) The progress which has been made during recent years in radiography has proved of great service in the diagnosis and treatment of diseases of the accessory sinuses of the nose.

A stereoscopic view of the maxillary antrum by Mr. Charles A. Clark is shown, also the piece of metal removed.

Mr. P. MILLICAN (introduced by Dr. Irwin Moore): The trouble started after the patient had had her first right upper molar removed by an American practitioner twenty-five years ago. She has very large antra, and I believe that the anterior buccal root of that molar was in intimate relation with the

antrum—possibly it projected into it. It is my opinion that, having discovered that he had made a passage into the antrum, the practitioner attempted to plug the socket with a piece of aluminium rod. This rod he found to be longer than was necessary, so he snipped off the excess with pliers while it was in her mouth—the mark of the pliers may be distinctly seen on the aluminium rod taken from her antrum by operation. This piece of rod escaped from his reach, passed into the antrum, and remained embedded there for twenty-five years. I should like to add (what is useful to note) that the patient suffered periodically two kinds of pain: one of a stabbing nature caused by the sharp ends of the foreign body irritating the mucous membrane of the antrum, and the other a "lump sensation," the site of which was variable.

(*November* 3, 1916.)

Sarcoma of the Right Maxillary Antrum; Lateral Rhinotomy performed (Moure's Operation).

By Irwin Moore, M.B.

Patient, a female, aged 50, was first seen on July 14, when she complained of swelling of the right cheek for six months, accompanied by considerable muco-purulent discharge from the right nostril. There was marked bulging outwards and upwards of the right maxillary antrum, some displacement of the right eye, and œdema of the lower lid. The right nasal cavity was completely filled with polypi.

Transillumination showed the right maxillary antrum to be opaque. X-ray examination by Dr. Finzi also showed that it was opaque, its upper outline ill-defined, and the bone at its upper part partly absorbed. The right sphenoidal sinus was absolutely clear (though it looked rather dark, this being due to a shadow across the cavity). Both ethmoids also appeared clear. Patient declined operation, but on account of the swelling increasing she was admitted to hospital on September 18, when a lateral rhinotomy was performed.

The growth, which appeared to have arisen from the right ethmoid region, occupied the greater part of the antrum, had penetrated its bony wall, and extended into the soft parts of the cheek, but had not yet infiltrated the skin. The floor of the orbit was intact, though somewhat roughened. The growth was thoroughly removed, together with the ethmoid cells and a portion of the muscular tissue of the cheek. There was no difficulty in controlling the hæmorrhage, and the skin incision



FIG. 1.
Sarcoma of the right maxillary antrum. Showing marked distension of the antrum.



FIG. 2.
Sarcoma of the right maxillary antrum. Showing distortion of the face and distension of the antrum.

was sutured and dressed with strips of gauze and collodion. Healing occurred by first intention, the stitches being removed in four days. The œdema of the lower eyelid has not yet quite cleared up. The nose (now six weeks after operation) appears quite clean and healthy, and there is no discharge.

Microscopical Report.—The sections show dense masses of a round-celled tissue with a fibrous stroma. The cells vary in size, some of them are large, all possess large nuclei rich in chromatin. The growth is a small round-celled sarcoma, for several of the sections, evidently taken from the tissues outside the antrum, show striated muscle which is undergoing destructive infiltration by masses and lines of the round-celled tissue (Dr. Eastes' laboratory).

Radiograms by Dr. Finzi are shown, together with microscopic sections of the growth. The photographs show the swelling of the right cheek on July 7 and the increase in the growth during the following two months.

DISCUSSION.

Dr. W. HILL: In cases of doubt I recommend radium: in nine cases out of ten it acts like a charm.

Sir STCLAIR THOMSON: Recurrence can be best judged from the nose. It is a great thing to make a free opening into the nose. In cases like the two I published, in which the patients lived five and six years afterwards, you can pass the Eustachian catheter into the cavity and feel how it is lined with smooth mucous membrane. Interference with lymphatics leaves patients with a pseudo-œdema on the lower eyelid for three to twelve months, so it does not do to judge of recurrence by the outside appearance. Radium can be introduced to the original site, which is generally the ethmoid.

(*November* 3, 1916.)

Retention Cyst of the Nasal Floor.

By IRWIN MOORE, M.B.

PATIENT, a male, aged 53, first seen on September 12, 1916, complained of a growth on the right side of the nose and the adjoining portion of the cheek. The case is shown previous to removal by sublabial rhinotomy (Rouge's operation). Photographs and radiogram are also shown.

Note.—This case unfortunately could not be shown at the meeting, for while attending the dental department of one of the general hospitals to have some septic teeth removed before operation, the cyst was incised by the dental surgeon. Patient is now attending that hospital daily to have a gauze dressing inserted. I hope to show the case at another meeting, if and when the cyst re-fills.



Retention cyst of the nasal floor.

(*November* 3, 1916.)

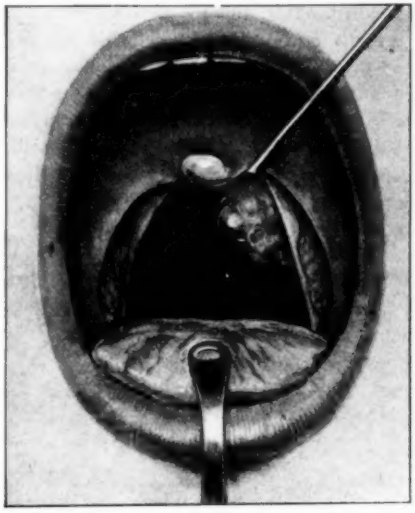
Carcinoma of the Nasopharynx removed by Operation.

By Irwin Moore, M.B.

Patient, a female, aged 70, attended the London Throat Hospital on July 5, 1916, complaining of frequent attacks of inflammation of the throat for six months, together with dryness of the tongue. Three weeks previously she had felt discomfort in the throat, "as if there was something there," and on taking a deep breath she noticed a swelling behind the soft palate. On retracting the palate on the left side an

irregular nodular growth was seen, which was of hard consistence and was attached by a broad base to the lower margin of the Eustachian cushion and lateral wall of the nasopharynx. Under an anæsthetic it was found impossible to draw a wire snare round the growth on account of its wide attachment, so it was removed in pieces by punch forceps, partly through the nose and partly through the mouth, patient being in the "hanging head position."

Patient now complains of shooting pains reaching from the left side of the back to the top of head. The left tonsil is seen to be enlarged,



Carcinoma of nasopharynx.

somewhat congested, and feels hard on palpation. This was, however, noticed the day following the operation, and does not appear to have increased in size. No enlarged glands in the neck can be felt, but the question of recurrence in the tonsil is a point on which opinions are invited.

Histological Report of the Growth.—The specimen consists to some extent of lymphoid tissue, but this is invaded by a new growth having the structure of a carcinoma. The cancer cells are rounded and arranged in compact solid masses of varying size infiltrating the tissues.

There is a good deal of associated inflammatory reaction (Dr. Eastes' laboratory). Microscopical sections are shown.

A drawing from life is exhibited, showing the growth before operation.

DISCUSSION.

Dr. BROWN KELLY: I would suggest a trial of radium in cases of malignant disease in the nasopharynx. Dr. James Adam and I have published cases of very striking disappearance of growth after radium treatment.

Dr. W. HILL: At the Radium Institute a good deal is effected by means of small needles. Radium is introduced through the nostril and embedded in the growth and then passed behind. In one or two cases I have put it into the palate. The next day one may find the growth has disappeared over an area of ½ in. from the needle. I had a patient who was sent me by Mr. Horsford, and I wanted to send the case back after a couple of days that he might see it again, but there was no growth. Two or three days later there was evidence of scarring, even in the nose itself. It was an endothelioma, and extended to the level of the epiglottis. The usual result in carcinoma is not good.

Dr. IRWIN MOORE: I ask whether members think there is a recurrence of the growth in the left tonsil. The question is, whether this should be dealt with now, and enucleated.

(*November* 3, 1916.)

Case of Cystic Goitre.

By IRWIN MOORE, M.B.

PATIENT, aged 39. Enlargement of the thyroid began thirteen years ago during her first pregnancy. A year later she underwent an operation at the Metropolitan Hospital. Seven months ago the swelling recurred, and is now growing rapidly. She has occasional shortness of breath, otherwise no other disturbance.

(*November* 3, 1916.)

Two Cases of Fatal Wounds involving the Carotid Vessels and presenting Unusual Features.

By H. Lawson Whale, Captain R.A.M.C.

Case I.—Private S. was admitted to No. 13 Stationary Hospital on July 13, 1916, with a shrapnel bullet wound at the junction of the free margin of the right ala nasi and the cheek; there was no exit wound. Instantly on his admission there was a copious hæmorrhage from the mouth, from behind the palate. On inspection it was impossible to say from which side of the nasopharynx the blood came. Since, moreover, a probe showed that the direction of the bullet was towards the left mastoid, responsibility could not be fixed on the branches of one carotid more than on those of the other. Pressure on the common carotid, however, was much more successful in controlling the gush when made on the right side, so the right external carotid was tied forthwith just above the origin of the superior thyroid. His condition being too grave to warrant his transfer to the X-ray room, he was carried straight back to bed as soon as I had tied the artery, so that we were even yet ignorant of the position of the bullet. Within six hours he developed complete *right* hemiplegia, and without showing any reaction to stimulants, &c., passed through coma to death twenty hours after his arrival.

Autopsy.—The bullet had passed through both antra, and the nose, and then deeply to the left vertical ramus of the mandible. On impinging on the anterior surface of the left mastoid the intact shrapnel bullet had turned downwards deeply to the carotid sheath, and was lying exactly in the bifurcation of the common carotid. The common and external carotids were normal, but, from its origin up to the base of the skull, the internal carotid was completely thrombosed. This thrombus was continued into the left half of the circle of Willis, and into the middle cerebral artery as far as this could be traced up the Sylvian fissure. There was a sub-pial layer of red cortical staining over the Rolandic area. The necropsy showed nothing else noteworthy. On the right side the external carotid and its branches above the superior thyroid were naturally collapsed.

Case II.—Sergeant W. was admitted to No. 13 Stationary Hospital on July 16, 1916, with a shrapnel entrance wound one inch external to the left nostril, and an exit wound in the left sub-occipital triangle. The bullet had traversed the left antrum and mastoid. The skiagram gave no useful information. He had left facial paresis of infranuclear type, cerebrospinal otorrhœa, and fine horizontal nystagmus of the first degree on deviation to the left. He had no giddiness, vomiting, ataxia, atonia, nor dysdiadokokinesis. His superficial reflexes were sluggish. A slight diffuse swelling occupied the parotid and temporal regions. Hearing, by air or bone conduction, was absent on the left; temperature, &c., were normal. Ordinary cleansing and expectant measures were instituted. July 18: The temporal œdema had subsided, that over the parotid was harder, and was continued down the anterior border of the sternomastoid as a brawny mass. His pulse was 110, temperature 105° F. with a rigor, and four hours later 103·8° F., with another rigor. The pupils were equal, sluggishly reacting to light; and at no time now or subsequently did the fundi oculi show anything abnormal. July 19: Operation. Through the ordinary post-aural incision the outer shell of the mastoid (which was found cracked) was removed. The bullet in passing had smashed the facial "bridge," with the rest of the bony posterior meatal wall. The lateral sinus was laid bare with the chisel; no perisinuous pus was found, but the sinus wall was hard, pulseless, covered with shreddy granulations, and nearly black. Bone was cut away backwards until healthy sinus was reached (1½ in.), and downwards to the bulb. The insertion of a needle showed that the thrombus was only mural, not complete; and before putting gauze plugs between the dura and skull behind, and into the sigmoid sinus itself (which was slit open for this purpose), blood was allowed to flow for a few seconds. The internal jugular and common facial veins were tied in the neck. I did not turn out the clot between the ligature and the bulb. Subsequent history: For ten days there was a gradually diminishing flow of cerebrospinal fluid from the wound, the external auditory meatus, and into the mouth (? *via* the Eustachian tube). Besides ordinary dressings, &c., he was given Wright's antisepsis vaccine, and champagne. The cerebrospinal flow and the nystagmus slowly disappeared, and in all respects he steadily improved. But on July 26 the temperature and pulse became irregular, and his condition suggested septicæmia. A blood culture was made, and an autogenous vaccine of streptococci prepared from it. August 1: Hæmorrhage from the lowest angle of the post-aural wound, apparently from deeply in the

parotid region. The patient only lost 4 oz. of blood, and the flow was easily stopped by pressure; but his pulse became thready, and he died within an hour.

Autopsy.—The lateral sinus was occluded back to the torcular. The straight, occipital, and superior longitudinal sinuses were healthy. The bulbar opening of the inferior petrosal sinus was well occluded. Brain: There was slight bruising of the *right* cerebellum and temporo-sphenoidal lobe (? by contre-coup). The wounds, both bullet and operative, were quite clean. The heart was much infiltrated with fat. The bullet, in traversing the left parotid gland, had completely severed the external carotid artery at the point of its final division into superficial temporal and internal maxillary. This hole in the artery was clean-cut, and from it there was a blood track up to the post-aural wound.

Comment.—There was never any indication that the bullet had involved any large artery. The clean and complete severance of the vessel, the track of the bullet, and the absence of macroscopic sepsis, made it clear to all who saw the necropsy carried out that this was not a case of secondary ulceration, but of direct trauma. I do not know whether the patient's life would have been saved if I had tied the artery when I tied the vein. Probably not, since he had a blood infection.

(*November* 3, 1916.)

Case of Carcinoma of Nasopharynx in a Girl, aged 17.

By W. M. Mollison, M.C.

F. C., aged 17. A hard mass of glands in the left side of the neck; fixed, occasionally painful and tender. The tonsils and adenoids were removed at a London hospital six months ago. She was seen at Guy's Hospital in October, 1916. A small mass was seen in the nasopharynx, chiefly on the left side. This was removed mostly through the nose, but partly with an adenoid curette. To the naked eye the mass resembled simple adenoids, but section proved it to be a squamous-celled carcinoma (Dr. Nicholson). A section is shown.

(*November* 3, 1916.)

Laryngeal Case for Diagnosis.

By W. M. Mollison, M.C.

A. B., aged 35, has been attending in the throat and ear department at Guy's Hospital for some months; he is hoarse and complains of a little pain at times. The left side of the larynx is swollen and fixed; the mesial surface of the swelling is superficially ulcerated. The Wassermann reaction has been negative at two examinations. No tubercle bacilli have been found in the sputum on repeated examination.

The exhibitor would be glad of suggestions as to the diagnosis and methods of treatment. Potassium iodide has been given, also arsenic and iron, without any effect.

Sir StClair Thomson: This appears to me to be a case of typical tuberculosis of the larynx. This patient has a fixed vocal cord, and there is an infiltration of the epiglottis, the left cord (with ulceration) and left arytænoid. This process, arising in three different parts of the larynx, could not be anything but tubercle.

(*November* 3, 1916.)

Double Abductor Paralysis.

By William Hill, M.D.

This man had portion of a large goitre removed twelve years ago by two vertical incisions in the neck, and both recurrent nerves have probably been involved in deep contracting scars. It is noteworthy that only the abductor fibres are affected. Alarming attacks of laryngeal obstruction have occurred at times on exertion and more especially at night when he suffered from a severe cold. These attacks have become worse recently. The question is raised whether unilateral chordectomy and removal of the vocal process of the arytænoid will relieve the

obstruction, and whether the voice will necessarily be reduced to a whisper, seeing that adduction is still apparently perfect. Good results have occasionally been reported following unilateral chordectomy, but not by well known laryngologists, as far as can be gathered. Suggestions are invited.

DISCUSSION.

Mr. F. A. ROSE: I was present at the operation undertaken for the relief of the paralysis of the vocal cord on a horse some years ago, and I helped Professor Hobday to operate. The result in that case was not such as to encourage doing the operation on the human subject; there was no material benefit to respiration.

Dr. DONELAN: I think that if the patient would consent to the loss of his voice there should be no difficulty about cordectomy, any more than there would be in respect of removal of cords in epithelioma of the larynx.

(*November* 3, 1916.)

Case of Recurrent Ulceration of the Mouth.

By H. LAMBERT LACK, M.D.

THE patient, a woman aged 20, has been under my care since May for repeatedly recurring ulceration in the mouth, which commenced last Christmas. She has had similar attacks ever since she had chicken-pox at the age of 10. All the previous attacks have been mild compared with the present one, and have lasted one to two months. Then there has been an interval of at least six months. The spots commence as lenticular or rounded ulcers, with a white sloughy base and surrounding inflamed area. They are very tender and slowly increase in size. So far as I have observed, the spots show no tendency to disappear, except under treatment; one spot which was untreated for three or four weeks attained the size of a sixpence, with a very thickened base. They occur on the mucous membrane of the lips, inside of the cheeks, gums, palate, and tongue. Three years ago they were found on the vagina. The treatment has been the local application of nitrate of silver, which has

always seemed effectual, but not in preventing fresh attacks. The Wassermann reaction is negative. The patient is in excellent health otherwise.

Suggestions as to diagnosis and treatment would be welcomed.

Section of Laryngology.

President—Mr. T. Mark Hovell, F.R.C.S.Ed.

(*February* 2, 1917.)

Case of Speech without the Use of the Larynx.

By E. W. Scripture, M.D.

The patient, a girl, F. H., aged 18, has worn a tracheotomy tube since the age of 3. She breathes entirely through the tube; if it is closed or removed she begins to suffocate. With the laryngoscope the walls of the lower pharynx are seen to obscure the larynx. On retching, the larynx is raised into view. Apparently she cannot make air pass through the larynx. She can speak quite distinctly in a faint, almost toneless voice. It is of interest to inquire if the passage through the larynx can be reopened. There seems to be no fundamental reason why this should be impossible. If it is reopened, can the vocal cords be brought into action? If so, will the patient readily learn to speak normally, or will there be a conflict with the old method?

Apparatus for the Graphic Record of Speech.

Inscriptions of speech are made by the phonautograph method. The person speaks into a mouthpiece. The vibrations and puffs of air travel down a wide tube to a special recorder. The movements of the air are registered by a fine lever on a rapidly revolving blackened cylinder. The records are afterwards varnished.

Speech requires in the first place a supply of air with a difference in pressure before and behind some obstruction. Ordinarily the lung pressure is increased and the obstruction is formed in the larynx or the mouth. The patient shown uses the pharynx instead of the thorax. To produce "t," for example, she places the tip of the tongue against the front of the roof of the mouth in the normal way. Then she

contracts the pharynx—as can be seen from outside—and compresses the air. When the tongue contact is released, a "t" is heard. She likewise produces all the occlusives, such as "p," "k," and the fricatives, such as "f," "th," "s," "sh," in a normal manner except for the supply of air. As the amount of air in the pharynx is very small, the sounds become weak in long phrases.

To produce a vowel, a vibrating body is necessary. This she obtains by raising the back of the tongue against the velum palati. When the air is compressed in the pharynx, she allows it to escape by slightly relaxing the tongue-velum contact in such a way that the contact edges vibrate. The tongue-velum contact thus forms a pseudo-glottis. The different vowels are produced by adjusting the mouth cavity.

The patient can even indicate differences of pitch, though in an imperfect way, as in singing "ah-ah-ah-ah" on four notes of an octave.

This girl has never received any instruction in speech, and has evolved this method of talking quite automatically.

DISCUSSION.

Mr. HERBERT TILLEY: Dr. Scripture says in his notes: "There seems to be no fundamental reason why this should be impossible," i.e., the reopening of the larynx. The case illustrates what is the not uncommon but evil result of doing a high tracheotomy, or, more strictly speaking, a high laryngotomy. This patient seems to have had the tube inserted in the cricoid or thyro-cricoid region, with the usual result—viz., excessive formation of granulation tissue followed by cicatrization with eventual blocking of the lumen of the air-way. In University College Hospital there is at the present moment a boy who has been there four months for the treatment of this same condition. Under an anæsthetic we could pass only the finest Lister's urethral bougie from the tracheotomy wound through the larynx. We substituted a low, for the previously high tracheotomy. Laryngofissure was then performed. In the subglottic region I found a passage admitting only the small steel bougie. We dissected out some fibrous tissue, and then inserted a small intubation tube, leaving in also the low tracheotomy tube. The child was then able to breathe through the tracheotomy tube as well as through the intubation tube; the intubation tube has now been in, on and off, for four months. We have dilated the stricture so that it is now possible to insert a full-sized intubation tube, and to dispense with the tracheotomy tube for increasing periods of time. If we leave out the intubation tube for three days, the stridor recommences, and we have to re-intubate. I think we shall be able to continue with the intubation tube until the child has grown a little more, and then dispense with it altogether. If not, the question will arise as to whether we can turn in some skin flaps or graft the narrowed area of the laryngotracheal region. Perhaps Dr.

Scripture's patient has a narrow stricture of the subglottic region, and as she has had it for fifteen years it will mean that whoever treats the condition must be persevering, for it will probably occupy many months. Examination should be made by the direct method, to see what the glottic and subglottic regions are like, and then exploration from below to ascertain if there is any passage from the trachea through the subglottic region into the lower throat. It may be necessary to split the larynx and cricoid, to remove any obstructive or redundant masses of connective tissue, and to determine the possibility of either dilating the stricture by intubation tubes or by instituting some plastic operation which shall prevent future narrowing of the air-way. My hospital patient has a good voice when the intubation tube is removed.

Dr. W. HILL: It is a difficult matter to undertake a case such as that described by Mr. Tilley. The treatment may extend over two years, or even four, and the result is often disappointing. In one of my cases, in spite of the use of an intubation tube for six months, the tracheotomy tube had to be re-inserted after all apparatus had been dispensed with for a few weeks, and I gave the case up in despair, as far as dilating the strictured larynx was concerned. I am aware, however, that some good results have been recorded in such conditions. If the larynx grows and one can get a wider passage, the functional disability may possibly be reduced to a minimum.

Dr. W. H. KELSON: Why is it said that apparently no air passes through the larynx? The voice she has probably indicates that a very little air does pass that way. Some years ago I showed here the case of a man who had cut his throat above the larynx, causing a big aperture, and in that case the man could not speak at all when the opening was exposed. But when it was covered with the hand or with a handkerchief he could speak well, showing that the larynx played a minor part in the act of speaking.

Dr. SCRIPTURE (in reply): The patient cannot get air through the larynx. She collects air in the back of the pharynx and then squeezes it out with the muscles of the neck. When the tube is closed she begins to suffocate.

(*February* 2, 1917.)

The Relation of Peri-dental Gingivitis to Vincent's Angina.

By FRANK E. TAYLOR, M.D., and W. H. MCKINSTRY, M.B., Captain R.A.M.C.(T.).

THAT ulcero-membranous inflammatory affections of the buccal cavity may be widely distributed on the mucous membranes of the mouth and gums, and not merely confined to the pharynx and tonsil

in accordance with the classical descriptions of Vincent's angina, has been recognized by many observers since this was pointed out by Vincent himself in 1898. In addition to an ulcero-membranous pharyngitis and tonsillitis (Vincent's angina) he described an ulcero-membranous stomatitis and an ulcero-membranous gingivitis. In all these conditions, two or more of which are not uncommonly seen in the same patient, he found that the same micro-organisms were present—viz., the spirillum and *Bacillus fusiformis* of Vincent. Although Vincent's name is usually associated with these organisms, they would appear to have been first seen and described by Miller, an American dental surgeon practising in Berlin in 1882.

At the Queen Alexandra Military Hospital, during the last few months, we have bacteriologically examined nearly 300 cases of fuso-spirillary infection of the mouth, and have abundantly confirmed the previous findings of the constant presence of these organisms in all forms of ulcero-membranous inflammation of the mouth, either alone, or associated with various forms of cocci, bacilli, and leptothrices.

In addition to the diffuse ulcero-membranous gingivitis previously described by Vincent and others, which is frequently found in association with Vincent's angina, there is also frequently met with a more restricted infection of the gums with these fusiform and spirillary organisms, in which the lesions are limited to those parts of the gums which are in immediate contact with the necks of the teeth. This condition constitutes, we believe, a distinct clinical entity, and may accurately be designated "fuso-spirillary peri-dental or marginal gingivitis"—a condition which was named and fully described by us in a paper read before the Odontological Section of this Society on November 27, 1916.[1]

In addition to its prevalence, this affection is of additional interest and importance in that it is often confused with pyorrhœa alveolaris, although it differs from this condition in the absence of pus and pus-pockets. Further, the treatment, course, duration, and prognosis of these two affections are essentially different.

In our earlier examinations of cases of Vincent's angina the investigations were restricted to the ulcero-membranous lesions of the pharynx, and to the differential diagnosis of the condition from other ulcerative membranous infections, particularly diphtheria and syphilis. Later the ulcero-membranous lesions were found not to be confined to the

[1] *Proceedings*, 1916-17, x (Sect. Odont.), p. 8.

pharynx, and many cases of fuso-spirillary gingivitis were observed, often associated with the typical Vincent's angina. It is, however, the restricted peri-dental or marginal gingivitis which we have particularly found associated with Vincent's angina, although in a few cases the accompanying gingivitis was of a more diffuse character.

Since our attention was first directed to this association of peri-dental gingivitis and Vincent's angina, we have made a systematic examination of the gums in seventy cases of Vincent's angina, and in every case, without a single exception, we have found the gums to be affected, the condition in the great majority of these cases being a localized peri-dental or marginal gingivitis. Conversely, out of 150 cases of fuso-spirillary gingivitis, the characteristic lesions of Vincent's angina have been present in the pharynx or tonsil in seventy cases. In all these cases the clinical findings have been confirmed by careful bacteriological examination.

An investigation of the histories of these cases shows the same sequence of events in them all—namely, that the gums were always infected first, and that the sore throat was the more recent condition, having been infected, it would seem, from the gums. In many cases the patients themselves would make no complaint of the gums, but on inquiry it would be found that the gums had been sore and bled more or less freely whenever an attempt was made to use a toothbrush or clean the teeth. This condition of the gums may have been present for days, weeks, or even months, and in a few cases years, before infection of the tonsil or pharynx supervened. In some of these chronic cases of peri-dental gingivitis the patients had repeatedly suffered from attacks of sore throat, and examination in these cases usually revealed evidences of old ulceration of the tonsil or pharynx, with loss of tissue, deep crater-like depressions in these organs being often found. In this connexion the following are interesting and instructive histories:—

Case I.—Second-Lieutenant D., Lovat's Scouts, presented himself for treatment suffering from Vincent's angina of the left tonsil. Peri-dental ulceration of the gums was also present about the lower incisors. The teeth were sound, clean, and in good condition. After two weeks' treatment by the topical application of an alkaline solution of salvarsan, the throat was quite healed, and the condition of the marginal gingivitis was much improved, though slight bleeding of the gums when touched still persisted, and fusiform bacilli were still present, though in greatly reduced numbers. Owing to the fact that he was then recalled to his regiment no further treatment was undertaken. About the middle of November the right tonsil became sore and painful. The regimental officer applied boroglyceride, tincture of iodine, and other solutions

without success, and so referred the patient back to us on December 5. We found a deep ulcer on the right tonsil, and a relapse of the peri-dental ulceration about the lower molars. Smears made from both the throat and the gums showed the usual picture of fusiform bacilli and spirilla. Swabbing the throat and gums with salvarsan solution was again regularly undertaken, and by December 18 the throat was completely healed. On December 22 the organisms had disappeared from the alveolar margins, and there has been no recurrence either on the pharynx or the gums.

Case II.—Private B., A.I.F., complained of sore throat, and two deep ulcers were found on the right tonsil. A soft whitish membranous exudation covered the ulcerated surface, and could readily be removed with a throat swab or platinum loop. This slight manipulation was sufficient to excite somewhat free bleeding. The throat had been sore for two days, and great pain on swallowing was experienced. The breath was very fœtid, and the lymphatic glands beneath the left angle of the lower jaw were swollen and tender. The gums also showed peri-dental gingivitis about all the lower incisors, and on examination it was found that bleeding of the gums had been present for the last two years. The teeth were in good order, the first and second left upper molars having gold fillings. Smears from both gums and throat showed the presence of fusiform bacilli and spirilla. The teeth were thoroughly cleaned by the dentist, and the gums and throat were then painted daily with salvarsan solution. By November 23 the condition of the throat was greatly improved and no bleeding occurred on instrumentation of the gums. On December 6 the throat was well and the gums were nearly healed. On December 15 both throat and gums were free from Vincent's organisms.

Case III.—Private C. was sent for the bacteriological investigation of a sore throat of four days' duration. An ulcero-membranous inflammation of the right tonsil was found. The gums, he stated, had been tender and bleeding for a week. Smears prepared both from the tonsil and the tooth margins of the gums showed the typical *Bacillus fusiformis* and spirilla. In ten days the throat was healed and free from these organisms, and a week later the peri-dental gingivitis was healed, and the tooth margins of the gums were free from these organisms.

Case IV.—Captain K., R.A.M.C., complained of sore throat of three days' duration, and of bleeding sore gums of three weeks' duration. The sore throat was a typical Vincent's angina, and the sore gums typical peri-dental gingivitis. The characteristic organisms were present in both conditions.

These few cases may be taken as examples of many which we are encountering daily, and the histories of the cases keep repeating the same sequence of events with monotonous regularity—first there are bleeding sore gums, which may, or may not, have been diagnosed as pyorrhœa, and which may have been present for some considerable time, and then the throat becomes affected and the typical ulcero-

membranous pharyngitis and tonsillitis of Vincent's angina is seen. The diagnosis is confirmed by finding the typical organisms in both conditions.

Our experience has been confined to adults, but Vincent's angina is also seen in children, and it would be interesting to learn if the same frequency of peri-dental gingivitis is observed in association with Vincent's angina in children, and whether the same sequence of events—viz., the peri-dental gingivitis preceding the Vincent's angina also occurs in them.

As the result of our investigations we suggest that when a patient complains of a sore throat which presents the characters of Vincent's angina, it is essential to examine carefully the tooth-margins for evidence of peri-dental gingivitis, or conditions diagnosed as pyorrhœa, and that smears be made from both sources and examined microscopically for the detection of the causal micro-organisms. When these are found to be present the peri-dental gingivitis should be adequately treated as well as the Vincent's angina, otherwise the condition is likely to persist indefinitely, or to cause repeated recurrences of the sore throat.

Our thanks are due to Mr. J. C. Potter, laryngologist to the Queen Alexandra Military Hospital, for sending us many of the cases, and confirming our results, and to the Commanding Officer, Surgeon-General J. Dallas Edge, C.B., A.M.S., for permission to publish them.

DISCUSSION.

Dr. DAN MCKENZIE: The association of peri-dental gingivitis and Vincent's angina seems to add one more lesion to the number already described in which this organism is present in large numbers. If Dr. Wingrave had been present he would have been able to discuss the matter, as he has devoted attention to it for many years. He has also found the organism of Vincent's angina in ear discharges, and in one or two brain abscesses. In a case I reported here the organism was found by him in the cellulitis of the neck following upon an operation for tonsillitis and adenoids, which proved fatal. Though it is present in large numbers in these lesions I do not think that its pathogenicity has yet actually been established. It is found normally in the mouth, and the question is as to what is responsible for the conversion of the organism from a harmless parasite into a factor in a troublesome lesion. Dr. Wingrave believes both the fusiform organism and the spirillum to be modifications of one species.

Dr. WATSON-WILLIAMS: May a too free brushing of the teeth with a stiff brush cause an infection of the gums by producing abrasions? A number of cases of pyorrhœa alveolaris are possibly due to this cause. Less vigorous brushing is often desirable, whereas the contrary is frequently urged.

Dr. W. HILL: How many typical tonsillar lesions has Dr. Taylor seen associated with this special type of Vincent's disease? We do not often see it. Several of my cases seen before the War were soldiers living in camps.

Mr. O'MALLEY: Has Dr. Taylor made any sections showing cellular changes in the tissues due to the activity of the organism mentioned? I have removed tonsils in which a similar condition existed, and the report I received was that the tissue changes showed an excessive amount of lymphoid cells, a small-celled infiltration, in the parts surrounding the ulcerated areas.

Dr. FRANK TAYLOR (in reply): We have seen more than 300 cases of infection by these organisms, and in every case, in our experience, the lesion has been limited to the buccal cavity. With regard to the presence of these organisms in a healthy mouth, one is tempted to ask, What is a healthy mouth? Even in well-groomed mouths we occasionally see fusiform bacilli and spirochætes, but the microscopic picture of the film from such a condition is quite different from that in which there is an infection. In the former case the films require much searching to detect the organisms, while in the latter there are masses of them. There is a difference of opinion as to whether these are two distinct organisms, or whether they are different stages of the same. We have actually found evidence of infection in toothbrushes, and in several cases we have associated the infection with the use of such brushes. In the last few months we have seen about 150 cases of Vincent's angina. We have not had the opportunity of making sections of the gum tissue, because the effect is the reverse of a hypertrophy, so that there has been no redundancy of tissue to remove.

(*February* 2, 1917.)

Four Cases of Laryngofissure for Epithelioma of the Larynx, shown at Intervals of Three and a Quarter Years, Two and a Quarter Years, Ten Months, and Three Months after Operation.

By Sir STCLAIR THOMSON, M.D.

Case I.—Laryngofissure three years after operation. Captain H. O., now aged 61, had been hoarse for twelve months when he presented himself on October 17, 1913, with a cupped growth occupying the posterior half of the right vocal cord. The cord moved freely. The Wassermann reaction was negative and there were no

tubercle bacilli in the sputum. The projecting portion of growth was removed by the indirect method with Mackenzie's forceps, and reported to be an undoubted squamous-celled carcinoma. November 10, 1913 : Usual laryngofissure ; tracheotomy tube removed at end of operation. Microscopic examination revealed structure of typical squamous epithelioma, not invading the muscle planes and showing a margin of healthy tissue all round. The patient's excellent voice is remarkable.

Case II.—Mr. F. H., now aged 71. This gentleman has already allowed himself to be examined by the Section three times. He was first shown on November 6, 1914, before operation, with the whole of the left cord replaced by a red, knobby, ulcerating infiltration. The cord moved well. The infiltration was not suitable for removing a portion for microscopic examination, and the diagnosis therefore depended entirely on the naked-eye appearances and exclusion of other possibilities. It will be remembered that the microscopic examination of the growth, removed by a first laryngofissure on November 12, 1914, suggested that the growth had spread close up to the line of excision in the posterior subglottic region. A second laryngofissure was therefore performed on November 20 (i.e., a week after the first) and the patient was shown to the Section a fortnight later (viz., December 4, 1914). The tracheotomy tube was removed at the end of the operation on each occasion, and, as will be seen by the dates of his appearance before the Section, his recovery each time was rapid. He again allowed himself to be exhibited before the Section for a third time on November 5, 1915.[1] The patient has not been seen for a year, but when last inspected he had an excellent voice and no trace of recurrence.

Case III.—Mr. E. N., aged 68, was sent to me by Mr. Johnson Taylor, of Norwich, who had diagnosed malignant disease of the larynx. The anterior four-fifths of the left vocal cord were replaced by an ulcerating, raised, indolent, pale pink infiltration, slightly white in the centre. The cord moved freely. April 4, 1916: The usual laryngofissure was carried out in the presence of several Members of the Section. The operation was practically bloodless and no vessels were tied from the beginning to the end of the operation. It occupied one hour. The tracheotomy tube was removed before the patient left the table. He was sitting up reading the paper in bed the

[1] *Proceedings*, 1916, ix, p. 6.

same evening, and next day was out of bed and eating solid food. The microscopic examination revealed an undoubted epithelioma and that the excision was well clear of the growth.

Case IV.—Commander F. I., aged 57½. The history of the onset of this patient's trouble is very interesting. He consulted Dr. W. R. Gibson, in Madras, who found a "papilloma" between the anterior ends of the vocal cords together with slight ulceration of the right vocal cord. This papilloma was about the size of a green pea. The diagnosis lay between papilloma and malignancy. The patient was advised to return to England at once for advice and possible operation. *En route*, about three weeks before coming under observation, he had an attack of acute tonsillitis, with much pharyngeal straining and his voice suddenly became "perfectly all right." About this time he brought up some blood on one occasion, but is not aware of having ever coughed up anything special. The patient was referred to me by Mr. G. Jackson, of Plymouth. On October 12, 1916, he presented himself with an almost natural voice, yet it was found that the anterior two-thirds of the right cord were occupied by a red, beefy, abraded, shallow infiltration. The cord moved freely and the condition was not suitable for removing a portion for examination. The diagnosis therefore rested entirely on clinical appearances and the exclusion of other factors. October 13, 1916: The usual laryngofissure was carried out in the presence of several Members of the Section. The tracheotomy tube was removed at the end of the operation. Patient was sitting out of bed and eating solid food the next day. He left the nursing home at the end of twelve days. The patient has not been seen since and therefore the present condition cannot be reported on.

DISCUSSION.

Dr. WATSON-WILLIAMS: The results in these cases are excellent. In the operation for laryngofissure I consider the preliminary injection of cocaine, before making the incision into the larynx, very useful in abolishing the cough reflex which may cause so much inconvenience. Dr. Irwin Moore has recently introduced improved or new instruments, such as the new saw and shears for dividing the cricoid, &c. Amongst Sir StClair Thomson's cases we find one where there is an apparent growth with appearances suggesting innocency, and in an unusual situation for a malignant growth of the larynx—namely, in the anterior commissure. Hence we ought to reconsider the view which has been put forward, that if the anterior third of the vocal cord is occupied by a growth it is far less suspicious of malignancy than if it is in the posterior half. It does not seem that location is a guide as to the character of a growth.

Dr. D. R. PATERSON: The whole operation, as carried out by Sir StClair Thomson, is a very satisfactory procedure. It has been much simplified in technique, and altogether it is one which is more desirable than it was formerly. One of the points upon which Sir StClair appears to lay stress is that it is a practically bloodless operation on account of the use of local anæsthesia. Of course he has been fortunate in getting the cases at so early a stage, and the lesson these cases teach is that we should try to educate the profession generally not to allow cases of hoarseness to run on in the way they are permitted to do, and that hoarseness may indicate a serious condition, which the sooner it is tackled the better.

Mr. BADGEROW: I have seen a number of cases upon which Sir StClair Thomson has operated in the last eighteen months, and three points particularly impressed me: (1) The operation is practically bloodless; (2) there is little, if any, shock—the patient is well the same evening, and next day is sitting up; (3) the voice is exceedingly good. I ask whether Sir StClair expects, in the case operated upon three months ago, a better voice than the patient has now, which I consider very good.

Dr. JOBSON HORNE: In commenting on the fact stated in the notes that in all four cases the affected cord "moved well" or "moved freely" at the time the patient came under observation, may I say that is not in accordance with general experience in cases of malignant disease of the larynx. It shows that the growth has not infiltrated the subjacent intrinsic muscles, and it also shows that an early diagnosis is everything in obtaining a good result from the operation.

Dr. W. HILL: Everything goes smoothly, and there is very little bleeding in this operation, as carried out by Sir StClair Thomson. It appears to be followed by little shock if not done with violence. Much of his success is probably due to the fact that he does not put the patients in the Butlin posture afterwards, but makes them sit up at once. Butlin's posture does not, as the originator hoped it would, prevent the septic secretions going down into the lungs, and if the patient has a weak heart, there may ensue œdema of the lungs, and in one of three cases death has resulted. In Sir StClair Thomson's cases there are no complications, so far as I can ascertain. These cases of his were most suitable for operation—the lesion not too large, but just large enough for easy diagnosis. In some of my cases, when I first saw them, invasion of the pharynx, and occasionally of the œsophagus and the mediastinal glands also had occurred, so that operation had no chance. Though I do not depreciate the value of his technique, yet I think his after-treatment is even more important.

Mr. FRANK ROSE: I should like to confirm Dr. Hill's remarks about the attitude in which these patients should be placed after the operation. I have tried keeping the patient lying down with the head very low; I have also adopted the method of making the patient sit up immediately after the operation, and I have no doubt that the last named is the better method—he is more comfortable, and gets better more rapidly.

Dr. DAN MCKENZIE : I ask Sir StClair, as I have asked on previous occasions, as to the advisability of removing a piece of the growth before operating. In one of these cases a projecting portion of growth was removed, but in the other cases no portion was removed because "the infiltration was not suitable." I would ask when it is suitable, and how we are to tell.

Sir STCLAIR THOMSON (in reply) : It is striking that not one of the four cases shown this afternoon had fixation of the cord : it is very important to remember that. Yet I had a case in which flagging of the cord was the only suspicious symptom. It does not always mean that the case is advanced ; the case I have in mind was in an early stage. In two of to-day's cases the growth was on the anterior four-fifths of the cord. The old idea that malignant disease selects by preference the posterior aspect of the glottis is, in my experience, quite a mistake. Perhaps the growths in half my cases were more in the anterior than in the posterior half of the larynx. With regard to previous removal of a portion for examination, in one case where a piece was projecting there seemed no reason why I should not make assurance doubly sure, but this is not my usual practice ; one might nick a piece of mucous membrane, the effect of which might be to stir up the disease. Of course part of the success has been due to the fact that I know when to hold my hand. That is shown in the case which Mr. Trotter will tell you of presently. I saw it was not suitable for laryngofissure, so I turned it over to him. Though Dr. Hill has seen some of my most favourable cases, I have had others which have been troublesome. In one or two I had to split the cricoid as well as the thyroid, and these are the more serious cases, because we get recurrences among them, though some have remained indefinitely without recurrence. Mr. Badgerow spoke of the absence of shock, and I have asked myself why. I show you a typical chart. The patients are operated upon at 9 in the morning, and at 6 p.m. the same day they are sitting up in bed reading the evening paper. Nearly all are out of bed the next day. I think this absence of shock is due to the use of cocaine before the operation. The skin is injected with eudrenine beforehand, and for some years past I have also made an intratracheal injection of cocaine before doing the tracheotomy : it seems to abolish the shock, and there is no coughing. It makes it as quiet and bloodless an operation as septum resection. Some of the patients have left London again within a fortnight. I do not know how I came to adopt this position for patients, because twenty years ago the teaching of Butlin, which was largely followed, was to put the patient low in the bed, and he was not allowed to lift his head, so that he lay there slobbering in blood and mucus, which he was unable to cough up, and the blood and mucus were sucked down into the lung. My first experience of the sitting-up posture was in a German in whom the anæsthetic was not given elegantly, and blood got into the base of the lung and produced consolidation. He refused to lie down after the operation, and he got well because he sat up and spat the blood up! The only improvements I feel I have made are merely simplifications. Dr. Irwin Moore, to whom I am deeply indebted for details connected with these operations, injects the patients' skin an hour before the

operation at 9. This is one of the reasons why the operation is so bloodless. It is a question of having all the preliminaries well done, not hurrying the operation, and taking care to restrict beforehand any indulgence in tobacco and alcohol, and having the blood-pressure determined. One man thus operated upon had double aortic trouble, albuminuria and cirrhosis of the liver, yet his operation went as smoothly as in any other case: he was thoroughly prepared for five days beforehand in a nursing home. We occasionally meet with a growth which is semi-pedunculated. The second patient was a case of the kind. I have published a case in which the patient came into my study with such a pedunculated little " currant " growth of one vocal cord, so simple looking that on the spot I lifted it off with Mackenzie's forceps, but when the report was that it was epithelioma, I did laryngofissure. In another case the patient came to the out-patient room with a pedunculated growth, which I regarded as malignant. I took it off and it proved so, and I did laryngofissure, but what we then removed proved to have no cancerous elements in it at all. This shows how superficial some of these early cases may be. But most of the cases I have had have been those in which the cord was simply infiltrated, just as it might be by a syphilitic deposit, and preliminary removal of a portion for microscopical examination would be impossible. You would have to take out a piece right through the middle in order to get a satisfactory microscopical examination.

(*February* 2, 1917.)

Extrinsic Cancer of the Larynx Two and a Half Years after Operation through the Side of the Neck.

By WILFRED TROTTER, F.R.C.S., and Sir STCLAIR THOMSON, M.D.

G. H., AGED 58, presented himself at King's College Hospital on February 24, 1914, with a reddish, slightly cauliflower growth of the left aryepiglottic fold, well limited and only extending a little way down towards the pyriform fossa. The Wassermann reaction was negative. Under cocaine a good portion of the growth was removed by the indirect method and reported to be a squamous-celled carcinoma. One gland was felt under the sternomastoid, just behind the left angle of the jaw. The case was transferred to Mr. Trotter, at University College Hospital.

First operation, March 31, 1914: Glands removed from the left side of the neck and found to be reaching from the base of the skull to below the clavicle. The sternomastoid was removed with the glands. The thoracic duct was cut and ligatured. This was followed by leakage of chyle into the wound, which was a long time in healing.

Second operation, June 9, 1914: A somewhat low tracheotomy was done. The larynx was approached through the old wound in the neck. The left ala of the thyroid cartilage was removed. Growth was found to be limited to the left aryepiglottic fold and about the size of a threepenny-piece. This was removed with the clear area of a third of an inch all round. The pharynx was sutured with catgut and a drainage tube inserted. The tracheotomy tube was left in for two days. The patient was discharged from hospital on July 3.

Although subject for many years to chronic bronchitis, and a heavy smoker, the patient remains well, with a good rough voice. In spite of the removal of the left arytænoid and left thyroid cartilage, it is remarkable that there is no glottic stenosis. There is a good fixed cicatricial band on the left side, which acts as a vocal cord.

DISCUSSION.

Mr. W. G. HOWARTH: I have operated upon one or two of these cases by Mr. Trotter's method, as he described it in his College of Surgeons lectures. In some cases the growth has been too high or too low for my ability. I would ask Mr. Trotter how he deals with the cases in which the growths are in the pharyngeal wall, up to the angle of the jaw; whether he does a snipping of the jaw and a turning up of the ramus, or whether he has some other method of getting at them? How does he reach the growth when it extends downwards behind the pyriform fossa and deeper down on the edge of the œsophagus?

Dr. WATSON-WILLIAMS: Is Mr. Trotter in the habit of removing the sternomastoid with the glands? And, in removing the ala, was the incision carried sufficiently high to divide the superior laryngeal nerve going beneath the ala on that side? If one can preserve that nerve, it is very helpful, because one of the greatest dangers in connexion with all these laryngeal operations is the loss of cough reflex. That is one of the reasons we remove our tracheotomy tube after laryngofissure, and why Sir StClair Thomson's patients did well when sitting up, because they had the best chance of responding to the calls of the superior laryngeal nerve. I show a rough drawing of a case in which there was a malignant growth which appeared to be in the back of the left arytænoid. To approach it from the front by laryngofissure would have made it difficult to deal with the posterior aspect of the arytænoid, especially as the growth extended downwards to the back of the cricoid, so I approached it by opening the pharynx laterally. I got sufficient space by exposing the inferior constrictor and making an incision from below the level of the upper border of the thyroid ala to nearly the lower border, and dragging the thyroid ala forcibly forwards, so as to get some rotation of the larynx. As intratracheal anæsthesia was used no tracheotomy tube was needed. Blood was prevented from getting into the trachea or larynx from the

occlusion by the tracheal anæsthetic tube. In this way the left arytænoid was removed *in toto*, and the back of the cricoid cleared. The ala of the thyroid cartilage was intact, and hence there was no chance of subsequent falling in laterally, and, more important, the limited incision left the superior and external laryngeal nerves intact.

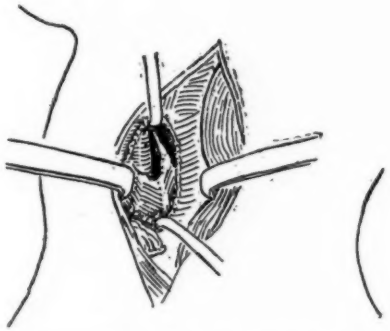


Diagram showing exposure of lateral and posterior aspects of larynx on the right side of neck, obtained by dividing the inferior constrictor along the posterior border of the thyroid ala, which is then forcibly drawn forwards. The dotted area on the left posterior cricoid wall shows the region occupied by the malignant growth. Above this the intratracheal anæsthesia tube is seen, and to the right of it the right arytæno-epiglottidean fold.

Sir STCLAIR THOMSON: I suppose it was inevitable, but I notice that there was a lapse of nearly four months between the diagnosis of the nature of the case and the removal of the disease. Mr. Trotter removed the glands, leaving the other operation till later. I suppose it was due to the leakage of chyle: otherwise, I take it, he would push from one procedure to the other as soon as possible. It was a great surprise to me that only one gland was felt under the sternomastoid, yet at the operation it was found that the glands reached from the base of the skull to the clavicle. Mr. Trotter said other cases promised well: I remember one which promised best of all, and I forget why, but the patient suddenly died. Is the shock more marked in these cases after this extreme dissection? And is it a warning which should put us on our guard? The whole of one half of the thyroid cartilage was removed on one side, and the left ala was removed. There is no stenosis. In some of my laryngofissures, where I had to take out the whole of one vocal cord, and perhaps three-fifths of the other, there was some stenosis. Formerly the patient could bicycle, and run upstairs, but after the operation he could not.

Mr. WILFRED TROTTER (in reply): In reference to the situation of the growth in relation to the incision when it is approached by this trans-thyroid operation, I may first remind you that normally we remove the ala of the thyroid, and then usually the great cornu of the hyoid as well. If that does

not give enough room upwards, I divide the jaw also, though where this is necessary it is very improbable that the case would be cured by operation. When these growths get beyond a certain size, although it is technically possible to remove them and the result looks encouraging, they almost always recur. When a primary growth demands some procedure which is unusually heroic the prospect of cure is not generally good enough to recompense the patient for the discomforts of the operation and convalescence. Hence it is unusual for me to divide the jaw after removing the thyroid ala. There is no difficulty in obtaining increased access downwards. Free division of the infra-hyoid and sternomastoid muscles with removal of the lateral lobe of the thyroid gland give access to the œsophagus as far down as the clavicle. With regard to extensive operations, the subject of removal of glands is on an entirely different footing. I am not at all alarmed by extensive gland infection, provided the glands remain isolated, and are not fixed to surrounding structures. Under such circumstances, we can almost guarantee a cure if we do an operation which is sufficiently drastic. The cases must be divided into two groups: The first group includes those cases in which the glands are not palpable; in this the ordinary operation is done without removal of the sterno-mastoid. The second group includes those cases in which the glands are palpable, and here we divide the sternomastoid and jugular vein close to the clavicle and remove the glands, fat, connective tissue, muscle and vein in one mass up to the skull. This case had one of the most extensive glandular involvements I have ever seen, but owing to the fat muscular neck it was practically impossible to detect the condition. We had even to divide the thoracic duct, in order properly to clear the lower end of the posterior triangle, and yet there is complete freedom from any evidence of glandular recurrence. As to dividing the superior laryngeal nerve, this is not usually important since we are usually about to remove the area to which the nerve is distributed. In a purely exploratory operation the nerve of course should be preserved until we know whether a radical excision of the growth is possible. With regard to rotating the larynx, I have done that several times in exploratory operations on the epilaryngeal region, but it gives a less satisfactory access to the region in question and I see no objection to removing the ala of the thyroid; it does not cause subsequent stenosis and it gives admirable access to the upper larynx. With reference to the dangers of the operation in comparison with those of laryngofissure, the difference is a very obvious fact of experience, and it is due to the fact that these epilaryngeal tumours are, technically, growths of the pharynx, not of the larynx. As long as we keep inside the larynx, we have nothing to do with such organisms as we heard of earlier in the meeting, the fusiform bacillus and the spirochætes of the mouth; but when the pharynx is encroached upon by the operation, they have always to be reckoned with and to be dreaded. Any wound involving the pharynx or œsophagus is liable to a sloughing infection which very small defects of technique may render extremely dangerous or wholly uncontrollable. The merest scratch of the epithelium of the pharynx leads, as is well known, to the formation of a slough. In laryngo-

fissure, there being no implication of the pharynx, it is not necessary to take any *special* precautions against sepsis, whereas in operations done on the pharynx almost the whole procedure is concerned with protection against the effects of the peculiarly virulent local types of infection. The special dangers of the pharyngeal infections are well seen in an operation apparently so simple as the removal of a pharyngeal pouch. If the closure of the gap left by the excision of the sac is not such as to render leakage impossible, there is the gravest risk of very serious and even fatal sloughing cellulitis and mediastinitis. Such forms of cellulitis spread rapidly, and often without much external evidence, and even if the patient survive the immediate danger, are particularly apt to cause secondary hæmorrhage. The length of the period between diagnosis and operation on the primary growth was due to delay in getting a bed in the hospital for the patient, but chiefly to the fact that resection of the thoracic duct at the time of the gland operation led (through escape of chyle) to interference with the healing of the wound. Until the wound made for the neck dissection, which was very extensive, had healed soundly, the operation on the pharynx could not be undertaken.

(*February* 2, 1917.)

Epithelioma of Left Maxillary Antrum and Left Ethmoid, Four Years after a Moure's Operation (Lateral Rhinotomy).

By Sir StClair Thomson, M.D.

This case is published in full in the *Lancet* for May 13, 1916, and was shown before the Clinical Section of the Royal Society of Medicine, February 14, 1913, and the Clinical Congress of American Surgeons, July, 1914. The interesting points will, therefore, be referred to briefly.

The subjective symptoms were slight and recent; grumbling pain in the cheek for two months. In the Moure operation two facial incisions were made, instead of the single one now recommended. Alarming symptoms, suspicion of serous meningitis, followed the operation, but subsided in a few days. The growth was a squamous-cell carcinoma, and although it had broken through the canine fossa and the antro-nasal wall, there is no recurrence after four years. The absence of disfigurement and the comfort of the patient, especially when compared with the operations formerly employed in general surgery, are remarkable. There is no trouble in the way of atrophic rhinitis, in spite of turbinal removal, and there is no epiphora.

(*February* 2, 1917.)

Round-celled Sarcoma of Right Maxillary Antrum, Ethmoid and Nasal Cavity, Two Months after a Moure's Operation (Lateral Rhinotomy).

By Sir StClair Thomson, M.D.

E. C., aged 62. This case is also interesting on account of the slight, recent and even remote symptoms the patient complained of. He consulted Mr. Hugh Smith, of Farningham, simply because of right occipital headache and insomnia, and it was only incidentally that he referred to nasal obstruction on the same side. This was found to be caused by a fleshy, vascular growth on the roof and outer side of the right choana and was evidently an off-shoot of malignant disease of the right maxillary and ethmoidal sinuses. There were decidedly hard glands in the right neck.

On December 13, 1916, a Moure's operation was performed through a single facial incision. This gave easy and complete access to the antrum, ethmoid, and roof of the nose, which were all crowded with typical malignant growth. The os planum had been destroyed by it, and the cribriform plate had gone, so that the dura mater was exposed and its pulsations were easily watched. Removal may be incomplete, but the case is shown to illustrate the great advantage of a single incision, the relief of symptoms, and the absence of disfigurement.

Hæmorrhage was easily controlled during operation. The exposure and handling of the dura mater caused no after symptoms. A fortnight after operation the patient complained of blindness in the right eye and he was found to have a retrobulbar neuritis with papillary œdema. The glands in the neck have had three applications of X-rays, and can hardly be felt.

N.B.—The patient has not been seen since January 4, and conditions may have altered.

(*February* 2, 1917.)

Carcinoma of the Right Maxillary and Ethmoidal Sinuses, Seven Months after Operation by an Oblique Facial Route.

By CECIL GRAHAM, F.R.C.S.

C. S., AGED 50, was seen by Dr. Hill in May, 1916, on account of swelling of the right cheek, proptosis, and nasal obstruction, with blood-stained discharge. The right nasal fossa was filled by growth, which obscured the view beyond the vestibule. Dr. Spilsbury reported upon a piece of the growth sent for microscopical examination that it was "squamous carcinoma of a very malignant type."

After the removal of several carious teeth, operation was performed on May 24, 1916, Dr. Chaldecott giving ether by the intratracheal method. An osteoplastic flap, resembling that which Dr. Watson-Williams describes, was turned over the cheek on the left side, the septum was removed completely behind the vestibular portion, and a complete view, with easy access, was obtained, by which it was possible to remove the lateral mass of the ethmoid, the inner wall of the maxillary sinus, the floor of the orbit, to clear out the cavity of the maxillary sinus, to remove some growths adherent to the orbital periosteum interiorly, and to explore the sphenoidal sinus. The latter was not involved. The patient sat up in twenty hours, was out of bed in forty hours, and left hospital in twelve days after the operation, after an uneventful recovery. The method of anæsthesia was a great comfort during the operation, particularly with regard to hæmorrhage, although this was not great. The photographs shown were taken twelve days after operation.

A dissected specimen is shown in order to illustrate the easy access obtained in the foregoing case by the oblique route.

(*February* 2, 1917.)

Carcinoma of the Right Maxillary Antrum; lateral Rhinotomy (Moure's Operation) performed.

By Irwin Moore, M.B.

Patient, a widow, aged 62, attended hospital in September, 1916, complaining of gradual swelling of the upper part of the right cheek during the previous seven weeks. No nasal discharge was present nor any other symptom. On the upper lip was seen a scar resulting from an old sore occurring forty years ago. Patient was not seen again for a month. At this time she complained of pain in the right temporal region, ear, upper jaw, and under the eye. Transillumination showed both antra opaque. On puncturing and washing out the right antrum, the fluid passed freely and the washings contained a considerable quantity of pus. Slight bleeding followed the puncture.

On November 8 the antrum was explored through the canine fossa, and found to be partly occupied by a polypoid growth, infiltrating the anterior bony wall and extending upwards and outwards under the malar bone.

Portions of growth were removed for examination.

On November 23 a lateral rhinotomy (Moure's operation) was performed by means of the double incision. The anterior bony wall of the antrum was found to be involved and extensively necrosed; the soft parts of the cheek were not infiltrated. The antro-nasal wall, floor of antrum and orbit were apparently intact. The growth, together with the necrosed anterior wall, was removed by forceps and curettes; five days later the face wound had healed and the sutures were removed, only slight œdema of the lower eyelid being present. No rise of temperature occurred above 100·4° F. She returned home twelve days later.

Patient is now shown, three months after operation. There is practically no nasal discharge, and she only complains of slight pain in the right upper jaw.

Microscopical Report.—This growth has the histological structure of a carcinoma. The cancer cells are arranged in the form of solid

masses and columns, freely infiltrating the fibrous stroma. There is no evidence of invasion of the bone by the growth. Whether the carcinomatous cells take origin from the glandular element or the dermic element of the mucous membrane is not quite demonstrable, but many of the alveoli are somewhat cylindrical in form, suggesting the glandular element.

A similar case to this, in which the growth proved to be a small round-celled sarcoma, was shown by the exhibitor at the meeting of the Laryngological Section[1] on November 3, 1916.

This case is of interest in that :—

(1) Primary carcinoma of the maxillary antrum compared with sarcoma is rare.

(2) It further confirms (as in cases previously shown at the meetings of this Section) the great advantage of Moure's operation, with its free access and thorough removal of the disease, over complete resection of the upper jaw with its accompanying deformity and discomfort.

Microscopical sections of the growth are shown.

DISCUSSION.

Dr. WATSON-WILLIAMS : I pass round a specimen to show the degree of approach one can get to the inferior part of the nose by making an osteoplastic flap on the same side as the growth. This was a case of round-celled sarcoma, which was very hæmorrhagic. In 1907 I performed the first operation on this patient.[2] A growth the size of a bantam's egg appeared to have originated in the upper and posterior aspects of the ethmoid labyrinth : it involved the antrum, and extended to the septum. It seemed to have recurred in 1910, but a piece removed *per nasum* was pronounced by the pathologist not to be malignant. But it did recur in 1916—nine years after the operation. The lesson from that is, that a vascular sarcoma is not necessarily of a high degree of malignancy. The entrance gained by this osteoplastic flap, sufficient to enable one to get the growth entirely away, to clear the ethmoid labyrinth, and remove the central and sphenoidal sinus wall, affords all the approach that could be wished. The osteoplastic flap was replaced carefully, and it healed, leaving no scar nor depression whatever. One advantage of the removal of the septum nasi is that it enables us to get an oblique approach to the left side through the right nasal passage, should that be desired.

[1] *Proceedings*, p. 29.

[2] For description and illustration of my operation see *Rhinology*, p. 113.

Mr. E. D. D. DAVIS: I saw Sir StClair Thomson operate on the old man shown to-day, who was blind in one eye. He performed Moure's operation for sarcoma of the antrum, and I was surprised at the easy access he obtained. There was only one incision, down the side of the nose, and it could be continued into the lip or upwards. It was not necessary to make a second incision under the eye. I have myself had only a small number of cases; I have employed Moure's operation, and by continuing the incision in a straight line, I have obtained good access. I have found intratracheal ether a great comfort, and it adds to the ease of the operation. In this patient the dura mater was exposed, but he seems to have recovered in a very satisfactory way, and the operation was excellent.

Mr. FRANK ROSE: There is a remark in Dr. Irwin Moore's record of his case: "This case is of interest in that primary carcinoma of the maxillary antrum compared with sarcoma is very rare." My experience has been precisely the contrary. In cases of malignant disease of the maxillary antrum, the growth has proved, under the microscope, to be carcinoma. I can only recall two cases in which it was sarcoma, and even in those it was not certain that it did not begin in the ethmoid. On what grounds does Dr. Irwin Moore make the statement?

Dr. DAN MCKENZIE: I agree with Mr. Rose that malignant disease of the antrum is generally epithelioma, while the ethmoid is the seat of sarcoma Allusion has been made to the fact that those sarcomata are "'*query*' sarcomata," as their malignancy is so slight and they seldom recur after removal. The occurrence of serous meningitis in Sir StClair Thomson's case is interesting. A mild form of serous meningitis is, I believe, not uncommon after nasal operations. In cases of that kind of difficulty a hint might be borrowed from the otologists and lumbar puncture done, because of the very prompt relief it affords. It may be necessary to repeat it after two or three days' interval.

Dr. KELSON: When first I saw Dr. Irwin Moore's case, which is a very interesting one, there was a dullness on both sides on transillumination, and pus was present in both antra. As there was a history of syphilis in the case, I suspected a tertiary syphilitic lesion, but Dr. Moore was anxious to do a Moure's operation, and then it was found to be malignant.

Dr. PEGLER: In regard to what Dr. McKenzie has just said as to nasal sarcoma, I fear I am in total disagreement with him. Though far less malignant than carcinomata, these growths range themselves quite easily under the groups of small and large round-cell sarcomata, met with elsewhere.

Dr. DONELAN: As regards the ætiology, some of these cases may be due to disease of the ethmoid. Several years ago I removed in one case a large number of mucous polypi and curetted the right ethmoidal region. The patient

remained well for about ten years. Four years ago he wrote that his trouble had recurred, but he did not come up until early last year. He had then a large mass of apparently mucous polypi, with some hard red polypi amongst them, growing from the ethmoid and filling the antrum. I removed the whole mass by Moure's operation. The report stated carcinoma. Recurrence, however, took place in the orbit, and the patient died nine months later. This case would appear to confirm Mr. Rose's view that malignant growths of this region are generally carcinomatous.

Dr. W. HILL: Mr. Graham is to be congratulated on his technique, though I do not say it will supersede Moure's operation in every instance. One gets by it a very extensive and open lateral view, including the ethmoidal region, the cribriform plate, the sphenoidal sinus wall, and the whole antral cavity. All must be impressed by the æsthetic result, as the incision scars can scarcely be seen, and thus compare favourably with the evident scars in Moure's operation.

Dr. IRWIN MOORE (in reply): I think I must be wrong in stating that primary carcinoma in this situation is rare. I have just had a conversation with Mr. Trotter, and he considers that carcinoma is more common than sarcoma. I thank Dr. Kelson for transferring the case to me. It is now three months since the operation, and I find that during the last month there has been a recurrence. With regard to the question of the single incision it was impossible in this case to reach the growth and clear it away without a double incision, because the antral wall was so much infiltrated by the disease. As in the case of a sarcoma of the antrum, upon which I recently operated, and which I showed at the last meeting, the growth could not have been satisfactorily reached and removed by the one incision, owing to the muscles of the cheek being extensively infiltrated.

Sir STCLAIR THOMSON (in reply): In my first case, although I did not think I was near the cribriform plate, the patient got symptoms which were very alarming: she was apathetic the next day, and had a temperature of 104° F., but she recovered. In the second case Mr. Davis will confirm my statement that the cribriform plate was gone, the dura mater was freely exposed, and yet the patient was not the worse. Neither of the cases had suppuration: there was no sinusitis going on. The first case would have been in a different condition had I accidentally wounded the cribriform plate or the olfactory region in the presence of streptococci, for instance. In the second case the disease spread through the sphenoidal fossa, as shown by the complete ophthalmoplegia. I think Mr. Graham's method is ideal for approaching the ethmoidal cells, but otherwise I do not agree that it is an improvement, because there could not be a freer access to the antrum or to the fronto-ethmoidal cells than is secured by a Moure's operation: it is distinctly *en fosse*, instead of being oblique.

(*February* 2, 1917.)

Epithelioma of the Nasopharynx; Operation; Diathermy.

By Norman Patterson, F.R.C.S.

Patient, a male, aged 36, tailor. History of operation on nose five years ago, when a "piece of bone was removed." He was admitted to the London Hospital on November 15, 1916. He complained of obstruction on both sides of nose, thick discharge, and loss of smell for six months. There has been headache in the right forehead and temple for two months. He is losing flesh rapidly. An examination showed a large, irregular, cauliflower-like mass, ulcerating in places, occupying the whole nasopharynx. There were no enlarged glands in the neck. There is slight deafness on the right side; no history of aural discharge. A portion of the tumour was removed and examined by Dr. Turnbull, who reported "solid trabecular squamous-celled carcinoma."

Operation, November 17, 1916: The soft palate was split. The incision was made from the right side of the base of the uvula. The view obtained was insufficient. The incision was carried forward through the mucous membrane of the hard palate to within 1 in. of the incisors; the posterior $\frac{3}{4}$ in. of the hard palate was removed with a chisel, also a portion of the posterior edge of the septum nasi. The attachment of the tumour defined with the finger was found to be more or less circular, occupying the roof and the right wall of the nasopharynx, and extending up to the base of the septum. The tumour was plucked away with the forceps close up to the attachment; hæmorrhage was stopped by pressure; the base of the growth was clearly defined. Together with surrounding tissues, this was thoroughly destroyed by diathermy. The palate was afterwards united.

The sense of smell returned three days after operation; recovery was uneventful. The palate is firmly united, and beyond some crusts there is nothing to note in the nasopharynx.

(*February* 2, 1917.)

Specimen of Antro-choanal Polypus.

By W. Jobson Horne, M.D.

The patient, a woman aged 31, was sent to the hospital by her doctor on account of obstinate nasal catarrh of long duration. Anterior rhinoscopy disclosed material hypertrophy of the middle turbinal bodies, but no evidence of polypus or suppurative disease. Posterior rhinoscopy showed the post-nasal space to be almost entirely occupied by a polypus which became directly obvious upon partly raising the soft palate. The polypus was removed through the mouth. The anterior ends of the middle turbinal bodies were reduced. After the removal of the polypus the left choana was found to be wider than the right; the polypus originated from the left antrum. Upon transillumination the left antrum was as translucent as, or even more so than, the right. In the circumstances, it was considered to be better to await results than to open the antrum.

It is hoped that the case will invite a further discussion upon some of the points raised at the last meeting, when a similar case was shown.

The polypus, which is exhibited, is pear-shaped and was attached by a pedicle as long and as firm as the polypus itself. The polypus upon section was solid with the exception of a small central cavity. It is fibrous in consistency.

(*February* 2, 1917.)

A Choanal Polypus originating in the Sphenoidal Sinus of a Child, aged 6.

By Irwin Moore, M.B.

The patient, a girl, attended the London Throat Hospital in December, 1915, complaining of inability to breathe through the left nostril for three months. The exhibitor first saw her in May, 1916, when a large, flattened polypus completely filled the left nasal fossa. No nasal discharge was present.

On palpation of the nasopharynx, an irregular, hard swelling was felt protruding through the left choana. Under general anæsthesia the nasal portion was removed by forceps through the anterior naris, leaving a pedicle attached posteriorly, which was traced to its origin in the left sphenoidal sinus. The posterior portion was so tightly



FIG. 1.

Choanal polypus originating in the left sphenoidal sinus of a girl, aged 6. Shows left ethmoid region opaque.

wedged in the left choana that forceps, introduced through the mouth, could obtain no hold. It was removed with difficulty through the anterior naris. On examination, the growth was found to consist of two polypi, originating by separate pedicles from one common stalk,

the anterior portion being soft and gelatinous, the posterior being bi-lobed and of a fibrous consistence.

On exploring the posterior naris, the ethmoid region was found to be healthy and the antral wall intact. The posterior edge of the vomer was partly destroyed by the pressure of the growth. A large opening into the sphenoidal sinus admitted forceps, probes or gouge with freedom. The sinus was about the size of a hazel nut. The right nasal fossa was normal.

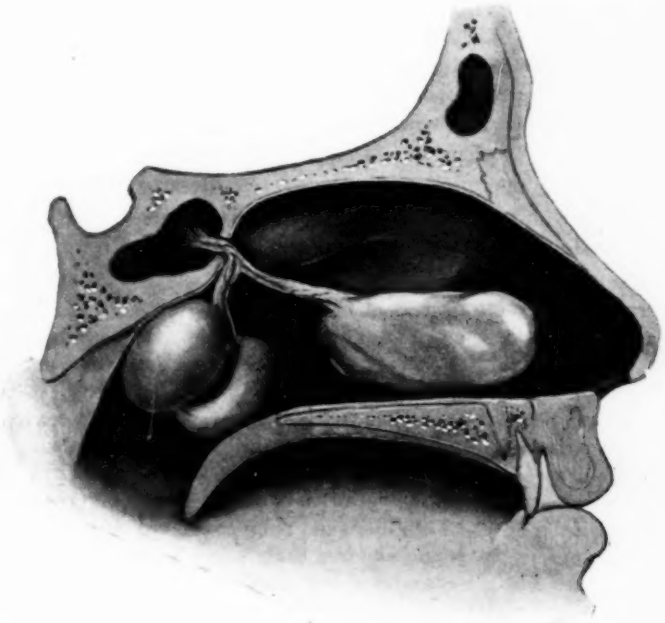


FIG. 2.

Choanal polypus originating in the left sphenoidal sinus of a girl aged 6. Shows a sagittal section through the left naris with the growth placed in position.

It was apparent that the polypus had originated in the left sphenoidal sinus, and the main pedicle, which was flattened on one surface, had probably been adherent to the spheno-ethmoidal recess. The case was seen at King's College Hospital two weeks later by Sir StClair Thomson, who confirmed this opinion.

Microscopical Report.—The polypus is composed of highly vascular fibrous tissue which is œdematous. It is fairly rich in connective tissue cells and in the superficial areas there is some leucocyte infiltration, and also a good deal of mucoid degeneration. Mucous glands are very few in number. It is a soft fibroma.

This case is of interest in that :—

(1) Though the maxillary antrum is the common seat of origin of choanal polypi, yet it is rare to find them in children under 10 years of age, only a few cases being reported.

(2) It is extremely rare to find a choanal polypus originating in the sphenoidal sinus, and the exhibitor has been able to find only two cases reported, and these were in adults. He is unable to find the record of any case occurring in childhood.

(3) There was no suppuration in the sinus.

A radiogram (fig. 1, p. 66) is exhibited showing the left ethmoidal region to be more opaque than the right. A life-size drawing is shown of the polypus after removal (fig. 2, p. 67), also a sagittal section through the left nasal fossa of a child, with the growth reconstructed and showing its origin and position.

Microscopical sections are also shown.

DISCUSSION.

Dr. WATSON-WILLIAMS: Sphenoidal polypus is sufficiently rare to justify my showing, stereoscopically, this polypus growing from the sphenoidal sinus. I obtained the specimen from Vienna. Dr. Irwin Moore's case is one of very exceptional interest, proving that even in a young child polypus may occur in the sphenoidal sinuses. I am more and more convinced that sinus infection is more often present in young children than is usually suspected. I believe recurrences after adenoid operations are sometimes of this nature. A child was brought to me eighteen months after a tonsil and adenoid operation by a very competent operator, and when I explored the antral cavities of the child, which was aged 5, with my antral suction syringe, there was antral streptococcal infection on the left side. I think it accounted for the re-infection of the lymphoid chain in the nasopharynx.

Dr. PEGLER: I do not quite follow Dr. Watson-Williams in his view of a possible cause of recrudescence of lymphoid tissue in the nasopharynx (adenoids), and I imagine there is a much more frequent cause of recrudescence of a remnant of adenoid tissue than infection from sinus suppuration. With regard to Dr. Jobson Horne's case, very soon after Sir StClair Thomson showed his specimen at the last meeting I had a similar one, in a young woman aged 21 ; it was a second recurrence. In the last instance it appeared

to be a polypus attached to the roof of the choana, but on examining it again I could make sure it was a polypus protruding from the ostium of the antrum. I removed it through the nose, without difficulty, by seizing it securely with Luc's forceps, and drawing upon it gradually. This was three months ago, and there is no sign of recurrence. There was no suppuration.

Dr. IRWIN MOORE: With reference to Dr. Jobson Horne's case, and the question whether the antrum should be opened in these cases, I think that it should be made an invariable rule to open the maxillary antrum through the canine fossa in all cases of choanal polypi when their origin has been definitely located in the antrum, in order that one may be certain of separating and thoroughly removing the pedicle from its attachment to the antral wall. This is advisable on account of their frequent recurrence after simple removal by snare or forceps. In connexion with these cases I have received letters from both Dr. W. S. Syme and Dr. Brown Kelly, who are unfortunately prevented by military duties from attending this meeting, the last-mentioned having, perhaps, seen more cases of antral polypi than anyone, and they both hold these views as to operative procedure. Dr. Syme sends me the following notes of an instructive case having an important bearing on this question. He says in January, 1908, he removed a solitary choanal polypus from a boy aged 9. The polypus quickly returned, and in May of the same year he removed another large growth. In January, 1911, the patient was seen again, and another large choanal polypus was present. This time Dr. Syme opened the maxillary antrum and traced the polypus to its origin in the antral cavity. There was also degeneration of other parts of the antral lining membrane. Since this operation there has been no further trouble with that antrum. This is only one of many similar cases which have been reported and confirms the necessity of radical extirpation of these growths.

Mr. E. D. D. Davis: I have investigated eight of these cases, two of them in children, and I have never found antral disease in any of them, though examined by transillumination and with the exploring syringe. The polypi have been removed, they have returned, and been removed again, and perhaps a third time, but I have not been able to establish the fact that there has been any disease of the antrum. The polypi grew from the middle meatus, they were post-nasal choanal polypi, and had a long pedicle. I do not agree that if you find the lotion aspirated from the antrum contains micro-organisms and no pus, it means that the antrum is diseased, because such organisms may normally exist in the nose, and contamination must frequently occur.

Dr. WATSON-WILLIAMS: I did not say that the fact of organisms being in the fluid washed from the antrum proves that it is infected, but that the absence of purulent discharge on washing out the antrum does not prove that it is not infected.

Mr. HERBERT TILLEY: With regard to the origin of these single choanal polypi (which often tend to become cystic and to burst when taken hold of), they generally have their origin within the antrum and near to the accessory

ostium. If you open the antrum through the canine fossa and take hold of the polypus from its choanal aspect, you will see that the pull is also being exerted on the mucous membrane of the antrum. Frequently they are unassociated with any purulent secretion. They seem to me to be a localized chronic inflammatory condition of the antral mucous membrane. In many cases a permanent cure may be brought about after one removal by forceps or snare, but recurrences can only be prevented by opening the antrum and removing the implanted pedicle of the tumour.

Dr. DONELAN: Seven years ago I operated upon a young man for a long pedunculated growth springing from the right accessory ostium, and I showed the specimen here. There was no suppuration in that case, and several growths of the kind have been recently shown before the Section in which it was stated they were not associated with suppuration. A fortnight ago I operated on the opposite antrum of the same patient, but the right antrum, from which the growth was originally removed, had remained quite healthy. That supports Mr. Tilley's statement that we should not always be so anxious to enter through the canine fossa. For my part I think it quite unnecessary and I have seen one or two cases in which after that procedure the levator anguli oris has been involved in the cicatrix of the canine opening, establishing a chronic smile on that side.

Dr. JOBSON HORNE (in reply): With regard to opening the antrum, we must remember that these cases of so-called choanal polypus are not in the same category as the more common cases of nasal polypus, and the pathogenesis is entirely distinct. Some of them appear to be congenital, although of slow growth, therefore they cannot be discussed on the same footing as suppurative disease of the antrum. I did not open the antrum in this case, because I felt I should like to do to my patient as I should like to be done by; after the polypus had been removed, I should like my antrum left untouched. If there should be a recurrence, then we could consider the advisability of investigating the antrum, and even if that were done I do not think a further recurrence would be prevented.

Dr. IRWIN MOORE (in reply): I am expressing the views held not only by Dr. Brown Kelly, who has records of forty cases of choanal polypi arising in the maxillary antrum, but also by Dr. Syme, whose article on "Choanal Polypi" in a recent issue of the *Journal of Laryngology*[1] deals with this matter; and also by Kubo, whose contribution on "Solitary Choanal Polypi" was published in 1913. All agree that these growths should be dealt with by radical operation. Transillumination for diagnostic purposes is of no value in these cases, for the antrum on the affected side is as translucent or even more so than on the normal side, whereas to X-rays the affected antrum appears opaque. This contrast of transillumination and X-rays has been investigated by Mr. Hett and Dr. Finzi. The combination of these two tests is of great diagnostic value in the localization of antral polypi.

[1] *Journ. Laryng., Rhin. and Otol.*, 1916, xxxi, p. 515.

(*February* 2, 1917.)

Laryngeal Mirror used by Manuel Garcia, the Discoverer of Auto-laryngoscopy; also the Apparatus used by him to demonstrate the Physiology of the Vocal Cords.

By Irwin Moore, M.B.

These interesting relics of the early days of laryngology are lent by his son and pupil, Professor Gustave Garcia, to whom they were given (by his father) many years ago; they have not been shown before in public. The exhibitor feels that they may prove of some interest to members of this Section. In the centre of the frame is the original pencil sketch of Manuel Garcia, at the age of 70, by his sister, Mme. Pauline Viardot, the famous operatic singer.

A short history of this remarkable man would appear not to be out of place here.

Manuel Garcia, singer, teacher of singing, and composer, was born at Madrid on March 17, 1805. He was the son of Manuel del Populo Garcia, the famous tenor, composer, actor, and conductor, who was born at Seville on January 22, 1775, and died in Paris on June 21, 1832. His mother was Jacquina Briones, the leading Spanish actress of her time.

In 1848 Manuel Garcia came to London and practised as a teacher of singing. In 1825 he commenced his scientific inquiry into the conformation of the vocal organs, which resulted in the application of the laryngoscope and his presentation to the French Institute of a "Mémoire sur la Voix humaine," which was "crowned" by the Academy. In 1847 he published his "Traité complet de l'Art du Chant." In 1855 he addressed a paper entitled "Physiological Observations on the Human Voice" to the Royal Society. These brilliant researches on the anatomy and physiology of the larynx were published in the *Proceedings* of the Society.[1]

To Manuel Garcia belongs the credit of having been the first to employ a laryngeal mirror for physiological purposes; the first who

[1] "Observations on the Human Voice," *Philosoph. Mag. and Journ. of Sci.*, x, and *Gaz. Hebdom. de Med. et de Chir.*, November 16, 1855.

succeeded in obtaining a view of his own larynx by means of a dentist's mirror. By this means he was able accurately to describe the parts he saw in the mirror and their respective motions. How he at length obtained the desired object he describes himself. Standing with his back to the sun, he held a looking glass in his left hand before his face, the sun's rays were thus reflected by the glass into his open mouth. Then he introduced a dentist's mirror—previously warmed—into the back of his mouth and by placing it at a proper angle, he was able to see the reflection of his larynx in the looking-glass. Auto-laryngoscopy was discovered; it was an indisputable fact. His feelings —when he saw for the first time how the vocal cords acted whilst he was emitting sounds—were described to his son Gustave Garcia as follows: "I was most fortunate, for I succeeded almost at once in seeing with my own eyes what I had conceived so long; it gave me such a turn that I felt on the point of collapsing."

It will be fresh in the memory of most laryngologists present to-day that Manuel Garcia celebrated his centenary on March 17, 1905, when he was entertained at a banquet by the Laryngological Society of London and presented with his portrait painted by Mr. Sargent His late Majesty, King Edward VII, conferred on him the decoration of Commander of the Royal Victorian Order, while the Royal Order of Alphonso XII was also conferred on this veteran. He reached the ripe age of 101, and died in London on July 1, 1906.

DISCUSSION.

Dr. DONELAN: I do not like to criticize a family relic of this kind, but it should not pass into the official record of our proceedings, that this is the mirror originally used by Garcia. This type of mirror was not designed by Morell Mackenzie until many years after Garcia had used a dental mirror for his experiments. Garcia always liked to use a Mackenzie mirror, and I have personally given him mirrors on behalf of Sir Morell Mackenzie on at least two occasions. The mirror shown may be one of them.

Dr. IRWIN MOORE (in reply): It was my intention to alter the title to "Laryngeal Mirror," &c.[1] There is no question that this is a mirror with which he used to demonstrate to his pupils the action of the vocal cords, and is not the one that he used when he first saw his own vocal cords. It will be observed that in the manuscript reference is made to the first mirror which he used as being a dentist's mirror.

[1] The original title given to this note was "An Original Mirror used by Manuel Garcia," &c., &c.

(*February* 2, 1917.)

Hæmatoma (?) of the Posterior End of the Inferior Turbinate.

By F. A. Rose, F.R.C.S.

In August, 1916, the patient (a medical man) complained of a blood-stained discharge from the back of his nose for three days, following stuffiness of one nostril of considerable duration. A smooth, round swelling filled three-quarters of the right choana. It looked like a blood-clot, but all attempts to wash it away were futile, and finally it was snared. It will be seen that the swelling consists of delicate connective tissue infiltrated with blood. There have been no symptoms since removal.

Has anyone present met with a similar case?

DISCUSSION.

Dr. Pegler: I know of no previous record of a hæmatoma of the posterior end of an inferior turbinal. Mr. Somerville Hastings once exhibited sections of a discrete nasal angioma of the usual type, but the present is a pure hæmatoma.

Mr. Frank Rose (in reply): The patient was between 35 and 40 years of age. I have not seen or read of a similar case.

(*February* 2, 1917.)

Specimen from a Case of Fatal Hæmorrhage from Gunshot Wound involving the Superior Thyroid Artery.

By H. Lawson Whale, Captain R.A.M.C., F.R.C.S.

(Shown by Dr. Dundas Grant.)

Private G. H., admitted November 1, 1916. His temperature was 103·6° F., pulse 128, respiration 36, and the bases of both lungs were dull. A rifle bullet had traversed his larynx in the coronal plane, entering on the left side opposite the greater cornu of the hyoid, which was fractured at its junction with the body of this bone, and emerging at the level of the right aryepiglottic fold.

On November 3, a fit of coughing caused profuse intralaryngeal hæmorrhage, which was unaffected by digital pressure applied to the carotid artery; and in a few minutes, before further measures could be adopted, he died from asphyxiation.

Autopsy.—The lower lobes of both lungs were solid with pneumonia. The trachea and bronchi were full of frothy serum and short broken casts of blood-clot. The course of the bullet corresponded to the clinical appearances. In its transit, it had fractured the left greater cornu of the hyoid at its junction with the body, and, traversing the anterior part of the supraglottic space, had deeply grooved the base of the epiglottis. The right superior thyroid artery, at the highest point reached by the upward loop at its origin, had been cleanly severed by the bullet.

(*February* 2, 1917.)

Carcinoma of the Floor of the Mouth treated by Diathermy.

By F. A. Rose, F.R.C.S.

G. M., aged 56, was seen by Mr. Tilley, who diagnosed carcinoma of the floor of the mouth in July, 1915.

August, 1915: Diathermy was applied by Mr. Harmer.

November, 1915: A second application was made by the exhibitor.

At the present time there is no ulceration, and the patient is perfectly comfortable except for a feeling of tightness caused by the scar tissue.

A microscopic specimen is also shown.

DISCUSSION.

Mr. Herbert Tilley: The result in this case is excellent. There was a hard ulcer, now there is a soft scar. Such a case gives us hope that in these early stages of malignant disease one may get a good result by a simpler form of treatment than the usual operations which are necessary for dealing with cancer of the floor of the mouth.

Mr. Frank Rose (in reply): I think Mr. Tilley is somewhat optimistic in his remarks. The patient appears well at present, but how long he will remain so I do not know. It is fifteen months since the last operation was done.

(*February* 2, 1917.)

Case of Laryngeal Cyst.

By Cyril Horsford, F.R.C.S.

Patient, a male, aged 46, came to the Throat Department of the Royal Hospital for Diseases of the Chest in August, 1915, with acute œdema of the left arytænoid and aryepiglottic fold. There was loss of voice and dyspnœa. The swelling was scarified. At his next visit, the following week, all œdema had completely disappeared and there was no evidence of disease which might explain the condition. The urine contained a slight trace of albumin. There were no tubercle bacilli in the sputum.

In August, 1916, the patient returned with a large cystic swelling involving the left ventricular band and left aryepiglottic fold and extending into the left pyriform fossa—an appearance exactly similar to the present condition. His dyspnœa was pronounced, and to relieve his distress the cyst was punctured with a cautery point. A large amount of blood-stained gelatinous fluid escaped. Until puncture was done it was impossible to punch out a portion of its wall, owing to its slippery surface and toughness. Although a large portion of its wall was removed, the cyst quickly refilled, and on two occasions has burst, with relief to the patient. There has been no material change during the past six months. Report of section cut by Dr. Bach: "Exact nature is obscure, but the material appears to be of an adenomatous nature, with a malignant tendency." A section is shown under the microscope. The urine is now free from albumin, and contains no casts. Wassermann reaction negative.

Suggestions as to treatment are invited.

DISCUSSION.

Mr. Herbert Tilley: Two years ago I had to deal with a case which presented the same appearance as in Mr. Horsford's case. My patient nearly suffocated on two occasions, and one night he saved his life by putting his finger down his throat and bursting the cyst. It recurred and became distended again, when I opened it, and cut out a considerable quantity of the wall with laryngeal forceps; the symptoms disappeared, and a week later there was nothing to be seen of the tumour. However, in six weeks' time it was filling again, and one night he again nearly choked. In the mid-line of his neck, in

the thyro-hyoid region, he had a swelling, which increased in size and became septic. We drained it for some time, but the fistula would not heal, and Mr. Trotter therefore took the case over for me, and will describe to you what he found. I think in Mr. Horsford's case the laryngeal cyst is probably an extension from the thyro-hyoid region, especially as Mr. Horsford's notes say that some gelatinous fluid escaped when the cyst was opened by him.

Mr. WILFRED TROTTER: When I first saw the man to whom Mr. Tilley refers, he had a chronic sinus in the front of his neck, over the thyroid cartilage, and I found it led down to and passed behind the ala of the thyroid, in the region of the thyro-hyoid interval; hence I thought it was a thyro-glossal cyst. But instead of taking its way towards the middle line under the hyoid bone, it turned laterally and passed into the thyro-hyoid interval at the side, and went up, but chiefly downwards. We removed the ala of the thyroid until the cyst was traced to the level of the vocal cord. Internally to the cyst was the very thin mucous membrane of the larynx, and on the other side was the ala of the thyroid cartilage. I should not like to offer an opinion as to what the cyst was pathologically, but that was its anatomical distribution. It is clear that the swellings, which appeared first inside and then outside, communicated, and that the internal extension in the region of the upper opening had shrunk on account of inflammation. My advice would be to attack Mr. Horsford's case from the outside after removal of the ala of the thyroid.

Mr. CYRIL HORSFORD (in reply): I am much interested in Mr. Tilley's case which he described, owing to its similarity to mine. I saw my patient sixteen months ago when he had what I took to be acute œdema of half his larynx: but it was not of globular outline, and it seemed to occupy various laryngeal structures. I thought that if there was a cyst wall it must be very thin. But this wall is so tough that I had the greatest difficulty in punching out a portion. Within a week it had re-united and filled, and some further infection took place. It has been in its present condition seven months.

(*February* 2, 1917.)

Sarcoma of Ethmoid and Superior Maxilla in a Man aged 39.

By H. J. BANKS DAVIS, M.B.

THE disease first showed itself in the form of a swelling over the left antrum, with bulging and œdema of the palate. In February, 1916, the antrum, which contained pus, was opened, and as the growth was obviously malignant, after a preliminary laryngotomy, the left maxilla was partially resected, and a suitable obturator was worn by the patient

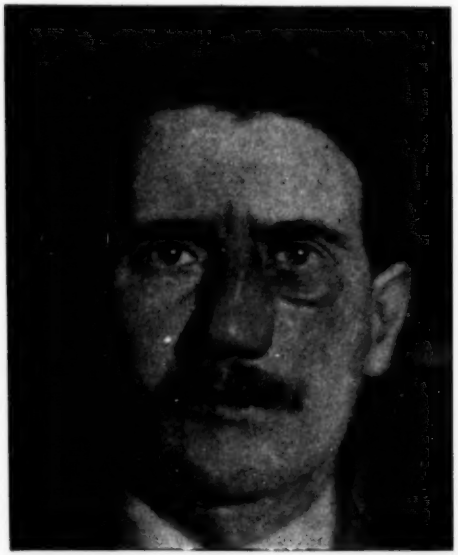


FIG. 1.

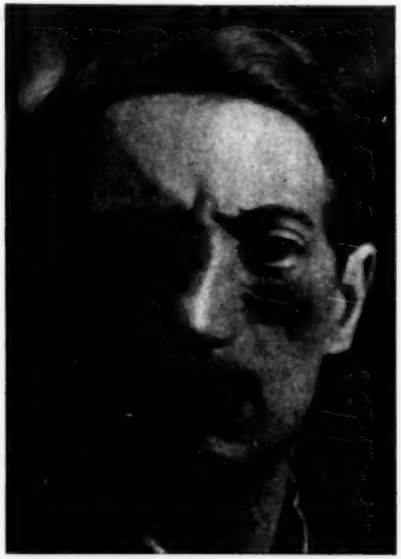


FIG. 2.



FIG. 3.

for months. He had X-ray treatment from Dr. Morton for several months, and a photograph (fig. 1) shows his appearance when treatment was discontinued.

The second photograph (fig. 2) shows his condition in November last, when he returned saying he was blind in the left eye. I opened up the face again, curetted away masses of bone, but the patient refused to have the orbit cleared. Under treatment at the Radium Institute he improved very much, and was able to follow his profession.

Postscript.—A few days after his exhibition before the Section he was seized with severe pain accompanied by hæmorrhage. He then became blind in the other eye.

The last photograph (fig. 3) shows his pitiable appearance a week before he died, after a third severe operation.

Radium treatment seemed powerless to arrest the rapidity of the growth in the later stages. It was a round-celled sarcoma.

CORRIGENDUM, p. 22.—Dr. DAN MCKENZIE's remarks should read as follows: "I suggest diathermy in the treatment of these adhesions. Has anyone tried to graft the interior of the nose in these cases?"

Section of Laryngology.

President—Mr. T. Mark Hovell, F.R.C.S.Ed.

(*March* 2, 1917.)

Case of Laryngeal Stridor due to Chronic Osteo-arthritis, relieved by Dilatation.

By J. Dundas Grant, M.D.

The patient, a woman, aged over 60, had suffered with discomfort in her throat for over fourteen years, and in the early part of 1912 developed a hard cough and noises in respiration observed mainly during sleep. The stridor was extremely marked, and there was complete absence of abduction, the vocal processes remaining in complete contact, there being only a narrow elliptical slit between the cords through which respiration was carried on. At first sight the case seemed to be one of bilateral paralysis of the abductors. Fourteen years previously she states she had an ulcer in her throat, close to the vocal cords, which was cauterized twice a week for six weeks with a brush. The possibility of cicatricial contraction as the result of cauterization was, therefore, considered. She, however, presented distinct signs of chronic osteo-arthritis, and it seemed that this condition was present in the crico-arytænoid joints. A weak spray of cocaine seemed to diminish the severity of the attacks of stridor, but she obtained no marked relief until dilatation by means of Schrötter's tubes was carried out. It cannot be said that even now there is any material movement of the arytænoids, but the elliptical breathing space is certainly larger and the improvement in the respiration is extremely marked.

DISCUSSION.

Mr. TILLEY: I was interested in this case because, on April 1, last year, a colonel, aged 77, bowed down with chronic rheumatism in all his limbs, came to me. The story was that in 1881 he had noticed he had difficulty in breathing. In 1884 he was obliged to retire from the Service on account of constant attacks of breathlessness, and one night he was nearly suffocated by extra severe dyspnœa. In April of last year his stridor was extreme, and there was bilateral adduction of both cords and I thought his condition serious. Radiography did not reveal anything in the chest in the way of new growth or aneurysm: indeed one would hardly expect either of these after such a long history. What was the nature of this adduction of the cords? In view of the general condition of his joints it was imagined he might have fixation of both crico-arytænoid joints. On December 28 last, I was asked to see him at his home. He was practically moribund and died three hours after I left. The "notch" of the thyroid was level with the manubrium sterni, râles in the lung were pronounced and extensive, and he was cyanosed. The laryngeal aspect of the case seems very similar to the one Dr. Grant is now showing. In the absence of a post-mortem examination, it was impossible to say whether there was any lesion pressing on either recurrent laryngeal nerve. If we exclude new growth and aneurysm in such a case, we are left with the possibility of a condition such as is illustrated by the present case.

Sir STCLAIR THOMSON: In this, as in similar cases, the last diagnosis we should give is fixation of the arytænoid joint, because I believe it is one of the rarest occurrences in laryngology, and absolutely proved cases of it are few and far between. This case seems to me more like cicatricial stenosis, with the interarytænoid space gone; for the cords are almost joined to one another. It is like a condition I have seen in a number of cases of tuberculosis, in which spontaneous healing takes place at the interarytænoid region, and the scar tissue draws the posterior ends of the cords so close together that in some cases I have had to do tracheotomy. On watching the present patient's arytænoids carefully when she is asked to phonate, the arytænoid is seen to swing on its base, particularly on one side. So I ask whether all other conditions have been excluded. I do not suppose this is tubercular, but it may be an old syphilitic case, and perhaps the Wassermann test may help us. Passing from that aspect, I would ask: "Why spare tracheotomy in such a case?" It seems a pity considering that tracheotomy is such a simple and safe procedure, and can be done without a general anæsthetic. This patient has had her discomfort fourteen years, and I do not see why she should not enjoy life, and I see no objection to her having a tracheotomy tube, especially as she may be able to discard it at a future date. Meantime it would also afford rest to the larynx. If the trouble is mechanical, she could wear an O'Dwyer's tube, which will produce successful dilatation.

Dr. W. HILL: A prolonged case of abductor paralysis is liable to end in immobilization, partial or complete, of the crico-arytænoid joint. Why that takes place I am not prepared to say, but I assured myself it was present in one case. In that case, one vocal cord was completely paralysed, as the result of an operation for goitre many years before. The arytænoid was fixed in a bad position: the cornicula pointed towards the middle line, whereas in Dr. Grant's case the vocal processes are in approximation. In my case, I was able, with my finger, to feel the arytænoid on the paralysed side, and it was as immobile as if it were ankylosed to the cricoid cartilage. I thought I should be able easily to take away that cartilage with forceps by the direct method, but it was so fixed that it would have needed to be cut out, and I did not do it. At a later date I did laryngofissure, for something else, and I then felt the joint again, and pointed out to those present that the joint was immobile. The deformity present there was the opposite to that in Dr. Grant's case. Conceivably this is a post-paralytic contracture of the muscles of the joint, which has become so fixed that the arytænoids do not move. [Drawing and demonstration.] I recommend Dr. Grant to try the following method: Put down a direct vision tube, and then put down the metal end of a pencil protector—of course without the rubber—on to the cornicula, and then see if the arytænoid will oscillate. In my case, the arytænoid on the immobile side would not move, but the other side was freely movable.

Dr. F. de HAVILLAND HALL: After the discussion we have had on this case, I hope that if Dr. Grant intends to continue the use of Schrötter's tubes, he will have the great patience which such treatment will demand. I remember that in my early and enthusiastic days I had experience of dilatation of the larynx, and I found it most disappointing. If, in the case of the out-patient department of the hospital, the patient failed to come at the due date, the condition, when next he came, was found to have relapsed in a most discouraging manner. I have come to regard the means as almost hopeless, especially in the case of out-patients.

Mr. CYRIL HORSFORD: Two or three days ago I saw a case of typical double abductor paralysis, for which I could find no cause: there were no signs of growth in the mediastinum or the neck, nor anything to throw light on the cause of the condition. The stridor, though obvious, was not alarming. I advised her doctor that, before it got too late, tracheotomy should be done, at the same time pointing out that she would be able to use her voice well. I stated what I believe is Semon's law, that eventually, when the paralysis becomes complete, the cords recede until they occupy the cadaveric position. I ask whether that is so. If so, it does not appear to confirm Dr. Hill's theory that the cords become fixed in an awkward position. If the later stage is adductor, not abductor, paralysis, one would expect to be able to promise, after some time, that the tube could be removed. That is a strong recommendation for the early insertion of a tube. I shall try to induce my patient to come and show herself next time. It has been going on for four years.

Dr. DONELAN: About ten years ago I showed the case of a lady who had caught cold and got fixation of the left arytænoid. The case was diagnosed by Sir Henry Butlin and Sir Felix Semon and others as rheumatic fixation of the arytænoid cartilage. I mention it because of what Mr. Cyril Horsford has just said as to the position of the cord. The cord lay in the cadaveric position, and had done so from the first. I was glad to have my impression that it might be a syphilitic condition in this case anticipated by Sir StClair Thomson. It looks very much like a case of old syphilitic stenosis I saw recently in which there was fixation by similar bands of adhesion.

Dr. DAN MCKENZIE: I do not know whether Dr. Grant has examined the case by hypo-pharyngoscopy. I have seen the method described as rough, but I do not find it upsets the patient at all, and it is easily applied. Arguing on the grounds of probability, one would say that osteo-arthritis of the crico-arytænoid articulation would be unlikely to result in such a position of both arytænoid cartilages as to bring both cords simultaneously into the position of adduction. I think it would be useful to have the larynx skiagraphed. I remember one case in particular in which the cord lay in abduction, and it was a simple matter to examine the patient by hypo-pharyngoscopy, and one then saw a great swelling behind the arytænoid of the affected cord. It persisted many years, and I concluded there was a chronic change in the bone.

Dr. DUNDAS GRANT (in reply): I thank members for their remarks. I think, with Mr. Tilley, that the two conditions are associated: it could scarcely be a mere coincidence, though there is much in what Sir StClair Thomson says as to the possibility. The appearance is such as would be produced by such a cicatricial contraction as he and I have seen in tuberculosis: I saw it also in one case in which applications of trichloracetic acid had been too zealously made for the destruction of tuberculous nodules in the inter-arytænoid space. That patient had very distressing stridor, which, however, settled down later. I think the possibility of bilateral paralysis of abductors may be left out of account. The reason which Dr. Hill adduced is incontrovertible. The patient has been very greatly benefited by the introduction of these tubes, even at longer intervals, and she has never gone back to the condition she was in before. I have not practised hypo-pharyngoscopy in this case, but I looked carefully for distortion of the joints. I think Sir StClair Thomson found there was none. I admit it is a weak spot in the diagnosis of arthritis, because in a paper which Sir Felix Semon published a number of years ago, he said swelling of the joint was one of the characteristic features. X-rays have not yet been used in the case, but I think they might be with advantage. I think the suggestion as to tracheotomy is a most appealing one, and probably that will be the ultimate solution.

(*March* 2, 1917.)

Case of Symmetrical Fibromata on the Vocal Cords, removed simultaneously by means of the Exhibitor's Forceps.

By J. DUNDAS GRANT, M.D.

THE patient, a fish-hawker, aged 36, suffering from extreme hoarseness of eleven months' duration, came to Brompton Hospital and was referred to the throat department. There were elongated sessile fibromata covering the middle of the edges of both vocal cords. The case seemed to invite the use of the exhibitor's so-called "safety" intralaryngeal forceps, and at one introduction the fibromata were cut off with absolute completeness on the left side and almost absolute on the right; the voice was at once completely restored; the small recurring tags on the right side were destroyed by means of the galvano-cautery.

DISCUSSION.

Dr. DUNDAS GRANT: There has been a recurrence since then, as the man has been using his voice, so the case is not yet a cured one.

Dr. DAN MCKENZIE: I cannot refrain from once again speaking of the great usefulness of Dr. Grant's laryngeal forceps. Some time ago I tried to remove a little growth from the cord by the indirect method, and I used his forceps. I was delighted at the ease with which the fibroma was produced.

Dr. WILLIAM HILL: I have used Grant's forceps for the direct method. I have had them made straight for the purpose. I think they are about the best forceps one can use for small growths projecting from the cords.

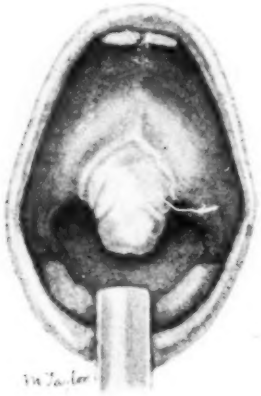
Mr. CYRIL HORSFORD: I have not used Grant's forceps for growths on the vocal cord. I think this is a good opportunity to bring up one point for discussion—namely, as to the advisability of using the galvano-cautery for the destruction of simple growths on the edge of the vocal cords. It is done by many laryngologists, but I regard it as a most dangerous operation, and from the artistic point of view, especially in the case of singers, the result is fatal as regards the voice. I think it would be valuable to hear a few experiences and views as to the suitability or advisability of the method.

(*March* 2, 1917.)

Lupoid Tuberculosis of the Pharynx in a Boy, aged 8, affecting the Soft Palate and Uvula.[1]

By Irwin Moore, M.B.

Patient, who was first seen on January 16, complained of a swelling in the neck under the chin, which was first noticed eight months ago. On examination, a semi-translucent swelling affecting the uvula was



Lupoid tuberculosis of the pharynx affecting the soft palate and uvula.

seen extending over the greater part of the soft palate, which, on palpation, felt solid, rigid, and leathery. The tonsils were not enlarged. Some adenoids which were present have since been removed, and were found to be tuberculous. The submental glands were enlarged and matted together, also enlarged discrete glands were present on both sides of the neck and in the inguinal region. There were no signs of pulmonary disease. With the exception of ulceration of the mouth when he was a year old, patient has been healthy, and has always had a good appetite.

[1] The original title given to this case was "Lymphadenoma (Hodgkin's disease)," but in view of the opinions expressed, and a later examination of the lungs showing slight active tuberculosis, it has been changed to "Lupoid Tuberculosis."

Patient has been treated for six weeks with potass. iodide, and the submental glands have considerably decreased in size but the condition of the pharynx is unchanged.

BLOOD REPORT (DR. EASTES' LABORATORY) ON FEBRUARY 16.

Number of red corpuscles	... ...	4,390,000 per cubic millimetre.
Number of white corpuscles	... ...	19,500 ,, ,,
Amount of hæmoglobin	... ...	70 per cent. of the normal.
Colour index	... ...	0·83.

Differential Count of 400 White Corpuscles.

Small lymphocytes	27 per cent.	40 per cent.
Large lymphocytes	13 ,,	
Polymorphonuclears	57 ,,	
Eosinophiles	3 ,,	
Mast cells	0 ,,	
	100	

No poikilocytosis was observed. There is occasional "stippling" of the red disks.

Histological report on adenoid tissue removed from nasopharynx: "The lesion is undoubtedly tuberculous."

DISCUSSION.

Dr. JOBSON HORNE, commenting on the description of the case printed on the agenda paper as "Lymphadenoma (Hodgkin's disease)," said: Before labelling the case as one of Hodgkin's disease we should remember that the term covers a large number of conditions, and that the growth removed from the nasopharynx is, in the opinion of the histologist, tuberculous. I could, had I known, have brought some specimens to compare with the one exhibited. Many years ago I did research work on the subject,[1] and from the same body I was able to demonstrate in the glands typical Hodgkin's disease, lymphosarcoma, and tuberculosis. I think we need to be a little more precise in this case.

Dr. DAN MCKENZIE: There is the fact that the submental glands are enlarged and matted together, which is not in favour of lymphadenoma, in which the glands remain discrete, there being no peri-adenitis to mat them together. Dr. Horne's opinion that the lesion is tuberculous is interesting, because I have heard him say that when multi-nucleated giant cells are found,

[1] "The Larynx—a Site of Infection in Certain Diseases of the Lymphatic Glands known as Lymphadenoma, Lymphosarcoma, Tuberculous Adenitis, with a Note on Primary Tuberculosis of the Organ," *Journ. of Laryng., Rhinol., and Otol.*, December, 1901.

one should be careful before calling it tuberculous. Lymphadenoma begins invariably in the cervical glands. I think it commonly affects the tonsil as well as the tonsillar gland, and this gives us the idea that whatever does produce lymphadenoma obtains entrance by the tonsil. Removal of the tonsils in lymphadenoma, even in an early case, however, has no effect on the course of the disease.

Dr. JOBSON HORNE: At a future meeting I shall be pleased to demonstrate specimens which illustrate the points I have raised. Another point is the part played by the larynx in Hodgkin's disease. In some cases I found ulceration in the larynx, and that, and not the tonsil, was the site of the infection.

Mr. FRANK ROSE: I agree with Dr. Jobson Horne's remarks as to the difficulty in establishing the diagnosis of Hodgkin's disease, or lymphadenoma. My experience is that in the post-mortem room a considerable proportion of the cases turn out to be something else, mostly tubercle. Before accepting that diagnosis in this case I think we require more convincing evidence.

(*March* 2, 1917.)

A Series of Twenty-four Foreign Bodies removed from the Air and Food Passages, with Indications as to the Lessons to be derived from some of the Exhibitor's Experiences.

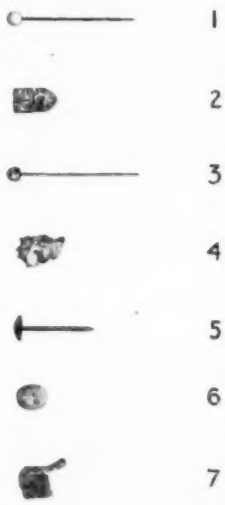
By HERBERT TILLEY, F.R.C.S.

THE point accentuated in certain specimens in this series is the one raised by Dr. Irwin Moore—viz., that it is inadvisable to use blind instrumentation for the removal of foreign bodies from the air and food passages.

In one of the patients I was asked to remove a coin from the upper part of the œsophagus of a hospital patient, but before I arrived there an enthusiastic house-surgeon tried to get the coin out with a coin-catcher. When I employed œsophagoscopy I could not find the coin in the gullet, and immediately afterwards a skiagram showed it was in the stomach; it had been pushed down by the ineffectual attempt to remove it with the coin-catcher. It was duly passed *per anum*.

Another case occurred in a soldier (fig. 7), in whom a portion of shrapnel casing lodged in the larynx, attention having been directed to

it by hoarseness. I saw something black in the left ventricular band, and decided to try to remove it by the direct method under general anæsthesia. It slipped away from the forceps and disappeared. As the patient was struggling most of the time, I obtained permission to bring an expert anæsthetist the following week, and we found the foreign



Bronchoscopy. Foreign bodies removed from the air and food passages by the direct method (bronchoscopy and œsophagoscopy). (1) Shawl-pin removed from left bronchus of adult female after two weeks' impaction; (2) metal pencil cap from left secondary bronchus, patient aged 9; (3) pin removed from left secondary bronchus of female, aged 23, after six weeks' impaction (vide *Lancet*, November 7, 1908); (4) portion of mutton bone removed from right bronchus of female after ten days' impaction (vide *Lancet*, April 22, 1911); (5) nail from right bronchus of female, three weeks' impaction; (6) pea from right bronchus of old man (*vide* present communication); (7) portion of shrapnel casing from left bronchus (*vide* present communication).

body in the left bronchus, where it had been located by the X-rays in the interval. The lesson to be learnt is that for the removal of foreign bodies from the larynx the patient should be in the

Trendelenburg posture, so that gravity does not aid the foreign body in gaining a deeper position. Also, one should have bronchoscopic instruments handy, in case the body slips from the larynx to a deeper region.

In another case (fig. 6), which was shown at the International Medical Congress in London, a pea had lodged in the right bronchus of an old man. Since it would have been futile to try to crush the pea and bring it out, we passed a bronchoscope on to it, and inserted a plug of wool soaked in paraffin, until it was close on the foreign body, and then by sudden withdrawal of the improvised piston the intruder was brought into the tube and readily extracted.

One of the three safety-pins (figs. 1, 3, 4) was removed last Christmas Day morning. It had been swallowed forty-eight hours previously. The matron and the house-surgeon of a hospital considered the patient's story to be fanciful. As the symptoms persisted, a skiagram was taken and the pin located. I succeeded in removing it quite easily with a Mosher's œsophagoscope. The moral of this case is that the history given by a patient concerning a foreign body should not be pooh-poohed or neglected. It is always safer to have a skiagram taken.

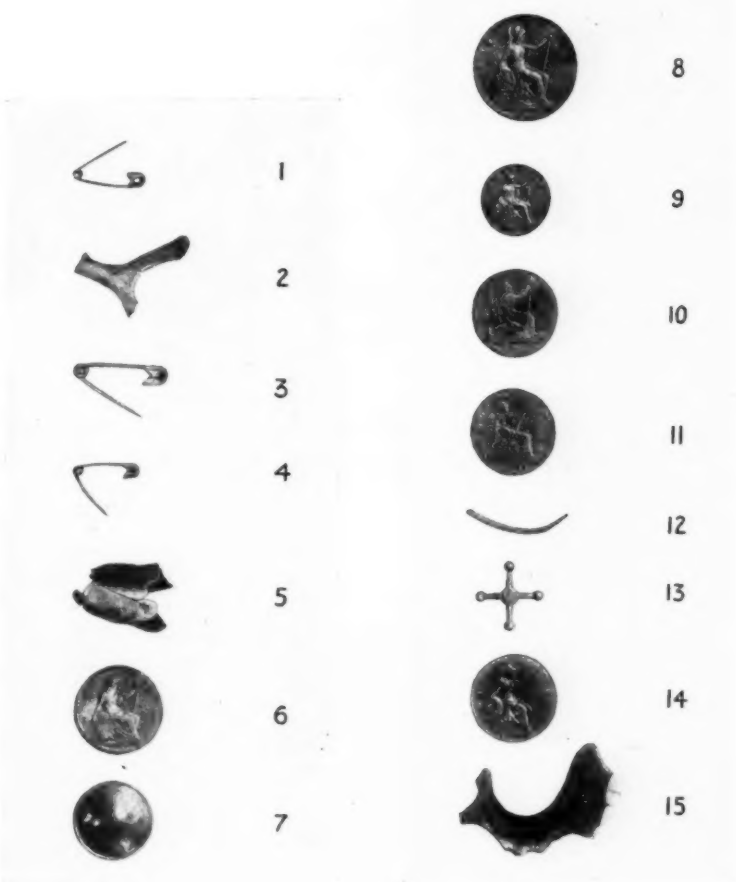
The turkey bone (fig. 2) was removed from the gullet of the wife of a medical man. Her husband tried on three occasions to push it further down with a probang. The result was some ulceration of the œsophagus. We removed it easily by the direct method (Mosher's distal lighted œsophagoscope).

This specimen (fig. 5) is from my youngest case of a foreign body. The patient was a baby aged 4 days. The nurse had given the child a "comforter" to suck, the inside of which was filled with cotton wool. The child detached the comforter from the shield and swallowed it; the secretions of the gullet made the wool swell and the œsophagus was obstructed, so that nursing at the breast was impossible. We removed the foreign body by means of a small bronchoscope.

In another instance a message came to my house from University College Hospital that a child had swallowed a foreign body (fig. 13). I told them to put the patient under an anæsthetic, and I would be round in ten minutes. I removed the foreign body and was back at my house within thirty minutes of leaving it.

The last specimen (fig. 15) was from a fatal case. A man was sent into the hospital forty-eight hours after having swallowed a denture during sleep. Before admission, ineffectual attempts had been made by means of a probang to push it into the stomach. The lower pharyngeal

regions were obstructed by œdematous swellings, the breath was foul, and the whole area was bathed in purulent secretion. We found our way into the œsophagus, and saw the denture lying in a greyish-green surrounding. We took hold of it with forceps and removed it without



Œsophagoscopy. (1, 3, 4) Safety pins removed from gullet (*vide* present communication); (2) turkey bone (*vide* present communication); (5) teat of "comforter" from gullet of baby, 4 days old (*vide* present communication); (6, 8, 9, 10, 11, and 14) coins—a penny (8), halfpennies (6, 10, 11, and 14), farthing (9); (7) coachman's button; (12) portion of rabbit's rib, impacted one week; (15) denture swallowed during sleep, blind instrumentation caused death (*vide* present communication).

difficulty, but the patient died forty-eight hours afterwards. Post mortem, three perforations were found in the side of the œsophagus close to the foreign body.

I show these foreign bodies and skiagrams in order to help in establishing the rule that at the present moment when endoscopes are so numerous, and there are so many experts to use them, it is unjustifiable to attempt the removal of foreign bodies in the air or food passages by blind instrumentation.

(*March* 2, 1917.)

Pin in Bronchiole of Posterior Lobe of Right Lung.

By Hunter Tod, F.R.C.S.

(Read by Mr. Norman Patterson.)

Girl, aged 12, admitted to the London Hospital on November 29, 1916. The mother stated that on the previous evening she fell off a chair whilst holding a long pin between her teeth. The girl complained of no symptoms, but as her mother could not find the pin she brought the child to the hospital to be X-rayed.

The pin appeared to be low down in the right bronchus with the head lying downwards. On the day of admission I passed a bronchoscope (medium size) along the right bronchus until it could be pushed down no further; the distance measuring 13½ in. from the teeth. A good view of the openings into the bronchioles could be obtained, but the pin could not be seen.

Two days later another attempt was made under the guidance of an X-ray screen. A photograph was taken when the bronchoscope had been inserted at a distance of 10 in. from the teeth. It shows it to be in the right position, although some way from the pin.

A third attempt was made a week later (December 8) also under the guidance of an X-ray screen. When the patient was lying on her back it seemed as if the bronchoscope had almost reached the pin, and that a pair of forceps passed along it would pass over the pin; but on turning the patient on her side it was found that the bronchoscope and forceps were well in front of the pin. It seems, therefore, that the pin is situated in the lower posterior bronchiole.

The patient suffered no inconvenience as a result of these examinations. She was kept in the hospital until January 12, over five weeks after the last examination.

An X-ray photograph, taken two days ago, shows the present position of the pin, which does not seem to have moved. The patient is still in the best of health and has no cough nor expectoration.

The points on which I would like the opinion of members of the Section are the following:—

(1) Should a further attempt be made to extract the pin through the bronchoscope or should it be left *in situ*?

(2) Is there more danger in leaving it *in situ* and risking an abscess of the lung than in making further attempts at extraction?

(3) Is this a case for surgical interference and for direct removal of the pin from the lung by an intrathoracic operation?

My own opinion is that nothing further should be done because as the patient is still in the best of health it is probable that the pin has become encysted and may give rise to no further trouble.

The surgical view that foreign bodies in the lung always lead to an abscess is not necessarily correct, because if a patient keeps well in spite of having inhaled a foreign body, unless the fact is known and the case is X-rayed, as in this particular case, such a condition may never be suspected.

DISCUSSION.

Dr. IRWIN MOORE: Mr. Hunter Tod's case relates to one of the few foreign bodies in the lungs which are out of reach of the bronchoscope. I understand that this pin has not yet been located by the bronchoscope, though it has been demonstrated by the X-rays. It is evidently situated in a posterior branch of an inferior lobe bronchus, and the only endoscopic tube we have that can be used in a child of this age measures 13 in., and this would not pass along the smaller bronchi nor reach the pin. Mr. Somerville Hastings had a similar case at the Middlesex Hospital in 1912, where a child aged 4½ aspirated a 2 in. shawl-pin into the lung. Many attempts at removal with the bronchoscope, even through a low tracheotomy opening, failed. A month later Mr. Kellock[1] performed pneumonotomy and successfully removed the pin. It was situated ¾ in. above the diaphragmatic surface of the lung. There was no associated empyema, and the child made a complete recovery. Chevalier

[1] Thomas H. Kellock, "A Case of Pneumonotomy for Foreign Body," *Proc. Roy. Soc. Med.*, 1912, vi (Clin. Sect.), p. 64.

Jackson, in his latest book on "Per-oral Endoscopy," refers to a few exceptional cases of failure which he has had, as beyond the limitations of bronchoscopy, and they were foreign bodies in a lower lobe bronchus, and out of reach. He considers that in such cases, if the foreign body is not extracted, an abscess always forms, sooner or later, and he records a case in which a pearl collar button remained in the left bronchus for twenty-six years before abscess formation occurred.

Mr. NORMAN PATTERSON: I think the pin in Mr. Tod's case might be got at by doing a tracheotomy, and carrying out inferior bronchoscopy.

Dr. D. R. PATERSON (Cardiff): I agree with Mr. Norman Patterson as to the advantage of tracheotomy in these cases, because it enables one to get nearer the foreign body, and a larger tube can be used. For a really good search in the lower parts of the bronchus, tracheotomy gives great advantages. I have adopted it twice in cases of very small children. In one case I found removal to be impossible through the larynx, but after doing tracheotomy it was manipulated without the slightest difficulty.

The PRESIDENT: Has strapping the child to a board, and more or less inverting it, been tried? It could not do any harm.

Sir STCLAIR THOMSON: These are most interesting cases, and there are many lessons to be drawn from them. One would like to study the histories in detail. One lesson is the use of suction, which would be very useful in cases of bodies impacted in a secondary bronchus. The air behind it is absorbed, and in getting it out there is the negative pressure on the opposite side. A foreign body seems to slip out of our forceps, and in some cases we think it catches at the end of the bronchoscope or œsophagoscope: yet we know that if we go at it again and remove the whole *en masse*, we are much more successful. So now, when I feel I have seized the foreign body, I remove the whole thing *en bloc*, and in doing so, I think the suction made by withdrawing the tube helps us very much. I had a case in which I saw a pin projecting from a secondary bronchus. I got hold of it; there was a gush of mucus and I lost sight of it. I let go, and withdrew the tube, which was full of mucus. I wiped it, and went in again, but there was no pin to be found! I took out the tube to have a rest, and when I turned up the lights the pin was seen on the floor. It had come out with the first gush of mucus. We know that to break up peas in the lung is a deadly proceeding. With regard to Mr. Tod's case, has anyone used the electro-magnet for such a case? I have seen surgeons use it, and they said their object was to get the pin simply to shift its position. This might help to get the pin from the secondary bronchus to the primary bronchus.

Dr. W. HILL: Iglauer has used the electro-magnet, and found it useful for metallic foreign bodies which were non-impacted, but when they were

impacted it did not shift them. So in America it fell into disrepute on account of failure in just those cases where it was required. With regard to the method of suction, I have shown a piece of chestnut, a portion of which was just projecting from a secondary bronchus. I could touch it with a sharp hook, but it would not come out. The patient was aged 13 months. I deliberately applied suction. I passed a glass tube down through the bronchoscope and connected the tube with a suction bottle. I got the piece out by this means, but on looking again into the bronchoscope I saw a second piece, which had dropped off the tube. At that moment the child gave an inspiration, and the second piece of chestnut disappeared from the endoscope, and went down the right bronchus. On following it up without cocainization, there was a spasm and cough, and the piece was ejected against my spectacles. With regard to Mr. Tod's case, I suppose most of us will advise him to leave it alone, but I do not know whether it is good advice. If the patient gets bronchiectasis, it should be more easy to find.

Mr. TILLEY (in reply): In answer to Dr. Paterson, I removed the safety-pins through Mosher's tube, with a distal light. In all endoscopic work I prefer distal illumination to the proximal light afforded by Brünings's instrument.

Mr. H. J. BANKS DAVIS: Three or four years ago, it occurred to me that using such an instrument as a magnet might enable one to remove these pins and needles. Several attempts to magnetize the fine gripping forceps were made for me by an electrical engineer, but, owing to the length of the instrument, it was found not possible to magnetize the tip—which after all is the most important part—but I do not see why this difficulty should not be overcome.

Dr. KELSON: With regard to blind instrumentation, an interesting case has just left the hospital. It was that of a man who, just before Christmas, had been eating nuts, when a piece of one stuck in his throat. He stated that on the following day vigorous and painful instrumentation was used with a probang. On the next day he had a swelling in the neck, and eight days later he came to hospital with a big brawny swelling on the left side, and the left arytænoid and left ventricular band were much swollen. Nothing could be seen of the foreign body, though he was watched for a few days, and X-ray search made. I made an exploratory incision into his neck, and let out a lot of fœtid pus, and bare cartilage could be felt. The wound continued to discharge for some time, but after opening one or two other abscesses, it has now cleared up.

(*March* 2, 1917.)

Ethmoiditis causing Blindness.

By G. W. Dawson, F.R.C.S.I.

Patient, a woman, aged 27, was sent to me by Mr. Kenneth Campbell on February 9, 1916. He reported optic neuritis of right eye, left eye slightly affected. For six weeks she complained of pain at the back of the eyes and across the bridge of nose; a small quantity of pus was seen in each olfactory cleft.

February 26: Eye report.—Right disk indistinct and pale; left, marked papillitis; elevation, 1½ D. She was almost totally blind and could only distinguish light and darkness.

February 26: Submucous resection of septum, which was thickened and deviated high up. Both ethmoids curetted and found to be necrosed.

In two days her vision was much improved, and on March 6 the ophthalmic surgeon reported a trace of neuritis. On March 31 Mr. Kenneth Campbell reported vision in each eye $\frac{6}{6}$.

(*March* 2, 1917.)

Report of Two Cases of Mutton Bones impacted in the Œsophagus; Œsophagoscopy; Recovery.

By Irwin Moore, M.B.

Case I: The Spinous Process of a Cervical Vertebra.—In May, 1916, patient, aged 62, came to the London Throat Hospital complaining that he had swallowed a piece of mutton bone four days previously. He was unable to swallow any hard food and even soft food caused vomiting. A radiogram, taken by Dr. Finzi, after a bismuth feed, outlined the foreign body about an inch above the level of the sternal notch. Under a general anæsthetic the following day an œsophagoscopic tube of 10 mm. diameter was passed, and the piece of bone was

easily located lying across the œsophageal lumen. On passing forceps and attempting to seize the bone by its extremity in order to turn it into its narrower diameter, it was displaced and slipped 2 in. lower. While again applying the forceps through a 12 mm. tube, a paroxysm of retching occurred, due to light anæsthesia, and the bone was vomited up the œsophagoscope on to the patient's face. It measured 24 mm. (1 in.) in length, and 8 mm. ($\frac{5}{16}$ in.) in diameter.

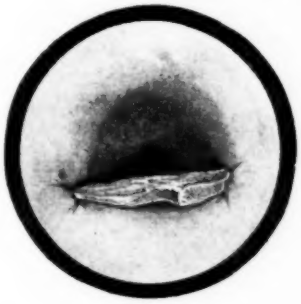


FIG. 1.

Piece of mutton bone, as seen by the œsophagoscope, impacted in the œsophagus in a man, aged 62.

Case II : The Transverse Process of a Cervical Vertebra with Bolus of Meat attached.—Patient, a housemaid, aged 40, swallowed a piece of mutton bone on December 5, 1916, which stuck in her gullet. She was sent to a general hospital, where she was told there was nothing wrong; four days afterwards she returned, and a brush-probang was passed down the œsophagus; then she was again told that nothing was there and sent home. Because she was unable to swallow anything but liquid food, most of which was regurgitated, and because her breath had become very foul, she was advised to go to the London Throat Hospital. When first seen on December 8 her breath was extremely fœtid, this suggesting that sloughing had taken place. Her temperature was subnormal (96° F.). A radiogram was at once taken and it showed a piece of bone impacted in the œsophagus at the level of the cricoid cartilage. The œsophagoscope was passed under general anæsthesia with great difficulty, owing to the patient having a short thick neck, contracted mouth, prominent teeth, and a very stiff jaw. The bone was seen and seized with forceps, but on account of its firm impaction with the surrounding œdema, ulceration,

and bleeding — which kept the field obscured — the risk of doing further damage was considered, and attempts at removal postponed. Considerable difficulty also arose during the examination from cessation of breathing four times, and artificial respiration was found necessary. The condition of the œsophageal wall was probably due to the previous ill-advised attempts to use a bougie. On re-examination, two days later, a large piece of decomposed meat was discovered and removed by forceps. Further search was postponed on account of the patient's condition, which required artificial respiration on three occasions. At midnight the patient vomited up the bone during a coughing attack. It measured 25 mm. (1 in.) transversely, and 16 mm. ($\frac{3}{4}$ in.) in length, and consisted of the transverse process and portion of the body of a vertebra, with three sharp and jagged angles. Patient made a speedy recovery, and left hospital a week later.



FIG. 2.

Piece of neck of mutton bone impacted in the œsophagus at the level of the cricoid cartilage in a woman, aged 40.

Remarks.—In connexion with the first case it is interesting to note that this is not the first instance of foreign bodies in the œsophagus or bronchi being expelled by vomiting or coughing while the œsophagoscope or bronchoscope were in position and covering the object. It may be useful in some cases to get rid of foreign bodies in this way by stimulating the laryngeal or pharyngeal reflexes during light anæsthesia. The second case was somewhat similar to one reported by Dr. D. R. Paterson, at a meeting of the Laryngological Section on December 3, 1915,[1] the measurements of the piece of bone being practically the same. In that case the patient unfortunately died from septic absorption, six days later, as the result of a lacerated œsophageal wall, probably caused

[1] *Journ. Laryngol.*, 1916, xxxi, pp. 149-150.

by the previous attempts of a surgeon to pass a bougie. These cases exemplify the serious risks to the patient's life from an impacted bone, and the danger which may follow the use of a bougie; also the importance of an examination by X-rays of all cases in which a foreign body is said to have been swallowed. The great advantages of œsophagoscopy cannot be too frequently emphasized.

(*March* 2, 1917.)

Infiltration and Ulceration of Vocal Cords; Malignant?

By W. H. JEWELL, M.D.

E. H. W., MALE, aged 53, became hoarse eighteen months ago, and has continued so. He has been a heavy smoker and drinker for thirty years. He has never had any pain, expectoration, difficulty in swallowing, or hæmorrhage. His septum is deflected. Wassermann reaction negative. The anterior extremities of the cords adjoining the anterior commissure have a yellowish glazed appearance, and behind this the left cord is swollen and abraded for the greater part of its extent. The margins of the cords do not meet posteriorly on phonation.

DISCUSSION.

Dr. JOBSON HORNE: The disease, I think, is malignant. It may be more extensive than the mirror would show. The case should be closely watched, and the position of affairs stated to the man, so that he may decide whether he will have the part involved removed as a precautionary measure, or leave it alone. The patient, I understand, has been taking iodide of potassium with some slight benefit.

Mr. CLAYTON FOX: I think it is hypertrophic laryngitis. There is a similar thickening of the opposite cord, more anteriorly, and the man has suffered from marked nasal obstruction, and is working in dusty surroundings. The cord in question is quite mobile, and if it were what it is said to be, with that infiltration, there would, surely, have been some sluggishness in the cord by this time. I have seen a similar case recently which was diagnosed as laryngitis tuberosa. There was fibroid thickening, but the cord has now cleared up entirely under treatment.

Mr. FRANK ROSE: I think there is severe laryngitis in this case, but the localized lesion on the left cord is, to my mind, highly suggestive of malignant disease. I should watch the lesion very closely, and if there is evidence of increase in size, I would recommend that the cord be removed.

Note. — Dr. Irwin Moore's epidiascopic demonstration of skiagrams showing some interesting points in the differential diagnosis and localization of diverticula and strictures could not take place for want of time. In consequence the reports of his two cases of pharyngeal pouches, together with the discussion, have had to be omitted.

Section of Laryngology.

President—Mr. T. Mark Hovell, F.R.C.S.Ed.

(*May* 4, 1917.)

Retainers for Tracheotomy Tubes.

By T. Mark Hovell, F.R.C.S.Ed. (President).

As several members of this Section have asked me for particulars with regard to the tapes with an elastic insertion for retaining a tracheotomy tube in position, as devised and used by Sir Morell Mackenzie and in use at the Throat Hospital, Golden Square, for the last forty years or more and to which I referred in my Introductory Address, I exhibit two specimens.

(*May* 4, 1917.)

Remarks on Treatment of Irritative Coughing.

By T. Mark Hovell, F.R.C.S.Ed. (President).

I have practically discontinued the use of trichloracetic acid for the treatment of irritative cough because I found that, in many cases, it produced a considerable amount of irritation which sometimes continued for several days, and I now employ instead a solution of perchloride of iron, 4 dr. to 1 oz., in equal parts of glycerine and water; but I always write the prescription as 240 gr., as otherwise the chemist is liable to dispense the liquor, which, as you know, contains much more free acid, which is not required and is better omitted.

I may mention also that Messrs. Hall and King, chemists, of Folkestone, have prepared for me, with the juice of garlic, an acetic syrup, an elixir, and an oxymel, all in the proportion of 1 in 5, so that now garlic may be administered by the mouth and thus save the more troublesome method, to which I referred in my Introductory Address, of cutting the cloves into thin slices and wearing them under the soles of the feet between two pairs of socks. These preparations are not disagreeable, and with the addition of honey or treacle they will be readily taken by most children if they dislike the preparation in its crude form. The dose of the juice of garlic as prepared by Martindale is 10 to 30 minims.

Although garlic will destroy the micro-organism of whooping-cough it must not be forgotten that, for the cure of the cough, an astringent must be applied to the lingual tonsil.

(*May* 4, 1917.)

Case of Adherent Soft Palate.

By G. W. Dawson, F.R.C.S.I.

Patient, a male, aged 23. At the age of 12 he had a sore throat which lasted two years, and for which he was treated at the London Hospital. However, when he was aged 16 the nasopharynx became closed, and he states that he was operated on four times and wore a tube in his nose for six weeks. When I saw him in October, 1916, the post-nasal space was completely shut off from the pharynx, and he was rapidly going deaf. In November I made an opening in the adherent palate, which was very thick, and introduced a rubber tube, which he has worn ever since, with great benefit to his hearing and general comfort. Wassermann's reaction was strongly positive.

DISCUSSION.

Mr. Herbert Tilley: This patient will be relieved, but only so long as the tube remains in position, and I should like to know how long Mr. Dawson proposes to leave it there, and how long he thinks it will be before the edges are covered with epithelium. If his method fails, perhaps he will feel inclined to adopt the method I described some years ago, when I showed two cases some months after operation. It entails dividing the adhesions between the palate

and the posterior wall of the pharynx, and inserting a silver-wire suture from before backwards at the junction of the soft and hard palate, letting it pass over the posterior free surface of the soft palate, to re-enter it on the posterior aspect of the free margin of the soft palate. The two strands are brought forwards and fixed over one of the incisor teeth. This is done on each side of the mid-line, and the wires are allowed to stay in a week, or until they cut out. This is a modification of a method described by Mr. W. G. Spencer many years ago at the old Laryngological Society.

The PRESIDENT: There is one method of preventing adhesions which is not generally known. About seventeen years ago, a young lady had her soft palate very seriously torn at an operation for removal of adenoids. When I saw her, extensive adhesions had formed between the soft palate and the posterior wall of the nasopharynx, leaving only a small aperture. Sir Frederic Eve, who performed the operation for me, had a prepuce which he had removed immediately before the operation commenced, which he split and stitched to the edge of the soft palate and base of the posterior pillars. The result has been extremely satisfactory. I saw the patient a year ago, and a free opening still remains. I agree with Mr. Tilley that directly the tube is removed from Mr. Dawson's patient, the opening will close; but if it were to be operated upon as Mr. Tilley suggests and then some mucous membrane stitched on, the result might possibly be good.

Dr. JOBSON HORNE: The note that the Wassermann reaction was strongly positive is the all-important factor in this case. I have had excellent results in two traumatic cases such as the President described, but we know the tendency for plastic adhesions to form in syphilitic cases.

Dr. DAN MCKENZIE: I should like to advise members against operating on these cases unless they are sure that the activity of the original malady has ceased. I say that because I have, by operating, set up again an amount of ulceration which ultimately left the patient decidedly worse than he was before.

(*May* 4, 1917.)

Case of Nasopharyngeal Fibroma.

By G. W. DAWSON, F.R.C.S.I.

PATIENT, a boy, aged 14½, complained of nasal obstruction for six months. Swelling appeared at the right anterior naris two months before operation. There was slight swelling of the right cheek. He was in no pain, but was very drowsy. On October 20, 1916, after a

preliminary laryngotomy, the growth was removed. It was found very dense and tough, attached to the roof and right side of nasopharynx. The antral wall was eroded and admitted a finger.

DISCUSSION.

Dr. IRWIN MOORE: Were there attacks of hæmorrhage before the operation? The growth is either recurring, or has not been completely removed. Might it not be a fibroma originating in the nose from the ethmoid or the antrum, with adhesions in the post-nasal space?

Dr. PEGLER: Mr. Dawson asked me to look at his section, and I conclude at once it is not a true nasopharyngeal fibroma, but a diffuse angio-fibroma, which has taken to growing into the nasopharynx. A true fibroma is not very vascular: the difference between the two types is very considerable from the point of view of vascularity. Pure nasal fibromata are very rare.

Dr. JOBSON HORNE: I agree with Dr. Pegler. The angiomatous element is the more important of the two. I agree also that, in most cases, these spring from the basisphenoid.

Mr. DAWSON (in reply): My first case had been operated upon four times, and when the man came under my observation he was distressed by the amount of deafness, which was rapidly becoming worse. That decided me to reopen, and see what would happen. Immediately it was opened he recovered his hearing, and expressed himself as feeling generally better. I found the scar tissue more than ½ in. thick, so that one could not manipulate it; so I think it would have been very difficult to carry out the wire treatment. If the tube is left out a few days, closure occurs: I leave it in because the patient prefers it so, and is then more comfortable. In my second case the fibroma protruded from the front of the nose, and bulged the palate downwards. It was attached to the roof and side of the nasopharynx, and its broad attachment was very tough, so that it was difficult to make any impression on it. With a raspatory and strong forceps I removed it piecemeal, as best I could. I could not get it all away because the boy collapsed towards the end of the operation. The growth caused an opening in the antral wall; one could introduce the finger past the tumour, and the opening felt as if it were eroded. I have no doubt it was due to pressure. He only had several slight hæmorrhages before. During the operation hæmorrhage was severe.

(*May* 4, 1917.)

Case of Right Optic Neuritis caused by Suppuration in the Right Posterior Ethmoidal Cells and Sphenoidal Sinus.

By E. D. D. Davis, F.R.C.S.

A. B., a fishmonger, aged 49, was sent to the hospital by Mr. McMullen, who found right exophthalmos, with restriction of movements of the right eye in all directions. Right optic neuritis and loss of sight. Vision, $\frac{6}{36}$. The loss of sight was of four weeks' duration, and exophthalmos two weeks. The left eye was normal.

On December 29, 1916, I examined the nose and I found tenderness of the floor of the right frontal sinus. No tenderness on pressure of the eyeball, or during movement of the eye. The nose showed polypoid middle turbinals, with pus in the right olfactory cleft: the signs of right nasal sinus suppuration were obvious. There was no suppuration of the left side. The maxillary and frontal sinuses were dark on both sides. The right maxillary was syringed out with a Killian trocar and contained no pus. X-ray: Small right frontal sinus; no evidence of cause of exophthalmos.

January 2, 1917: About six weeks after onset of defective sight, both middle turbinals were removed, the right ethmoidal cells and sphenoid were opened. The fronto-nasal duct was enlarged and a right intra-nasal antral operation was performed. Pus was *only* found in the posterior ethmoidal cells and sphenoid. The maxillary sinus was normal.

February 2: Mr. McMullen reported the eye to be practically normal, vision $\frac{6}{12}$, and that the exophthalmos and optic neuritis had disappeared.

A chart of the field of vision is shown.

DISCUSSION.

Mr. Mollison: It is not entirely proved that Mr. Davis's operation restored the patient's sight. I have seen these cases of blindness coming on for no obvious reason, and have removed the middle turbinal once or twice, simply in order to feel that I had left nothing undone, and in a month or so the patient recovered. But we cannot say that because the recovery followed

an operation, such as the opening of the posterior ethmoidal cells, that these cells were the only cause of the blindness.

Dr. DAN MCKENZIE: Although what Mr. Mollison says about retro-bulbar neuritis is true, we know that the condition clears up spontaneously. Yet here there was definite purulent disease of the nose and the nasal sinuses; and at this time of day I do not think one can doubt that optic neuritis is induced by suppuration of these sinuses.

Mr. DAWSON: I had a very similar case in February, 1916, in a woman. A little pus was observable in each olfactory cleft, but there were very few symptoms otherwise. She had a slightly deflected septum, particularly at the upper part, which rather obscured the ethmoid region above. With the right eye she could only distinguish the difference between light and darkness, and the other eye was little better. I did a submucous resection, and found the ethmoid soft immediately above. I scraped both sides, and in two days she had recovered her sight. Mr. Kenneth Campbell, who sent the case, and saw her thirty days after the operation, reported that vision returned to $\frac{6}{6}$ in each eye. I should like to hear an explanation as to why the recovery of sight was so rapid.

Dr. P. WATSON-WILLIAMS: This is a good example of a kind of case which probably most of us have encountered. There are one or two points to be emphasized in connexion with sinus infection in relation to optic neuritis. The connexion between the two was recognized as far back as the days of Berger, and he introduced the term "canalicular neuritis." I think one of the reasons we have failed to make the advance we might have done, lies in the fact that if there is no gross evidence of suppuration, we are apt to negative the nose as a possible cause of the optic symptoms. We may be unlucky in catching the secretion as it escapes from the sphenoidal sinus: it may be seen at one time, and a few minutes later the evidence may disappear. Given an infective sinusitis, I think it is when the leucocytosis is poorly marked and the secretion is mainly clear mucus and is not at all abundant, that infective conditions of the sphenoidal sinus leading to canalicular neuritis are most apt to occur. The reason is that polynuclears inhibit the organisms to such an extent that when there is much suppuration there is less likely to be toxic absorption, just as when a post-mortem wound does not suppurate there is more likely to be a systemic infection. So we must examine very carefully, and not be guided by the presence or absence of suppuration. We must be particularly suspicious when there is a nasal field contraction or relative scotoma, for then the case is much more likely to be due to sinus disease. When later the manifestations of suppuration are more pronounced, it may be too late to do anything.

Mr. CLAYTON FOX: It is very important, in these cases, to pay particular attention to the condition of the optic disk, especially in the early stages of the neuritis. The first change seen is in the periphery of the disk, and that causes enlargement of the blind spot. That is a very important matter, because it constitutes direct evidence of the periphery of the nerve being involved.

(*May* 4, 1917.)

Case of Left Optic Atrophy caused by Suppuration in the Posterior Ethmoidal Cells, Sphenoidal and Maxillary Sinuses of the Left Side.

By E. D. D. Davis, F.R.C.S.

T. S., a warehouseman, aged 57, complained of loss of sight of left eye for ten weeks, and long-standing nasal obstruction. He had nasal polypi for years, which were removed at intervals. The sight of the right eye was becoming slightly misty.

He was seen by Mr. McMullen on June 20, 1916, who found left optic atrophy, the pupil reacting sluggishly to light. No pain on pressure on the globe or on movement of the eye. He could only see hand movements in the upper nasal half of the field of vision. Right eye, vision $\frac{6}{6}$ partly.

Mr. McMullen sent the patient to me for an examination of the nose. Both sides of the nose were full of polypi, with suppuration on the left side. The left frontal sinus and maxillary antrum were dark. Both clear on right. Operation advised as soon as possible.

July 19, 1916: Middle turbinals and polypi removed. Both sphenoids and ethmoidal cells opened. Both maxillary and left frontal sinus opened by intranasal operation. The left sphenoid, posterior ethmoidal cells, and left maxillary antrum, contained pus and polypi.

Three days after operation, the patient volunteered the statement that he could see better with the left eye. He was seen by Mr. McMullen on October 20, three months after operation; the sight of the left eye had improved, the pupil reacted to light, and the periphery of the field of vision was normal, but there was a central scotoma.

A chart of the field of vision is shown.

Mr. E. D. D. Davis (replying on both cases): During the last eighteen months, ophthalmic surgeons have sent me sixteen cases of blindness which they called either optic neuritis, or retro-bulbar neuritis. In only two was nasal disease detected, the two now shown. The nasal disease—polypi and suppuration—was obvious, and the sight improved after operation. Early

operation gives a better chance of recovery, because cases due to nasal suppuration go on to atrophy, and then whatever is done does not improve the sight. Two cases were instances of syphilitic neuritis, which were treated in the ordinary way with benefit. In one case there was double optic atrophy of doubtful origin. One sphenoid had been operated upon, and I operated upon the other, but found nothing and there was no improvement. Of the other class, so-called retro-bulbar neuritis, there were eleven cases, and in these I examined the nose on several occasions, exploring the sphenoids with the Eustachian catheter. I also explored the maxillary antra, but none of these examinations yielded any result. Wassermann reactions, urine, nervous system and X-ray examinations, and searches for tuberculosis were all negative. Two patients were suspected of tuberculosis, but no evidence was obtained. These cases usually occur in young women under 40, and the degree of visual defect varies. It is usually of sudden onset, and unilateral. In a month or six weeks the sight improves and the patient is much better, but a recurrence is apt to follow. Ultimately there is recovery. What is the cause of that type I do not know: the ophthalmic surgeon suggests there is an intermittent catarrh of the ethmoid or sphenoid, and that the sight improves with the disappearance of the catarrh, more particularly as the onset of retro-bulbar neuritis is sometimes accompanied by a history of an influenzal cold. I had charts of the fields of vision made, so that we might know what fibres of the optic nerve were affected. The outside fibres supply the macular region and, if affected, produce central scotoma. Affection of the nasal side would produce a central scotoma, while the central fibres of the trunk of the nerve go to the periphery of the field. But the charts were of little value: in both the cases which had nasal disease there was a central scotoma and contraction of the field. The retro-bulbar neuritis cases also had a central scotoma and contraction of the field. Two of the patients with retro-bulbar neuritis had slight enlargement of the middle turbinal and the condition of the nose was doubtful, so I removed the middle turbinals and opened the sphenoids and ethmoids with a negative result and no improvement in the sight. I hope members will be able to find something to account for this condition.

(*May* 4, 1917.)

Case of Acute Osteomyelitis of Frontal Bone secondary to Acute Frontal Sinusitis; Operation; Recovery.

By W. M. Mollison, M.C.

W. B., male, aged 15, attended the Throat and Ear Department at Guy's Hospital on February 22, 1917, on account of pain and swelling on the forehead. He gave the following history: "For several years

attacks of epistaxis from the left side of the nose; for some weeks (?) frontal headaches. Ten days ago had 'influenza' and suffered from severe frontal headaches and had discharge from the left side of the nose. This morning the left eye began to swell."

Examination showed swelling over the forehead, most marked over the left frontal sinus; the skin was shiny but not red; there was great tenderness over the whole of the swelling; the left eye was practically closed on account of œdema of the lids; there was some œdema of the right eyelids, but this eye could be opened well. There was creamy pus in the left middle meatus. Temperature 100·4° F. The patient's general condition was good. This examination took place at 4 p.m. and the boy was admitted forthwith.

Operation was performed at 9 p.m.; by this time the œdema had spread very distinctly, both eyes were quite closed by œdema of the lids. The usual incision was made over the left frontal sinus and the sinus opened and pus found; while this was being done the cut edges of the bone were noticed to be oozing minute beads of pus and to be somewhat avascular. It was now realized that extensive removal of bone was indicated; further skin incisions were made—one over the right frontal sinus, which joined the original incision at its lower end across the bridge of the nose; from the centre of this incision across the nose a vertical incision was carried up over the forehead; the flaps thus marked out were reflected up and outwards. Bone was now removed till a healthy cut surface was reached; the frontal bone required removal from one external angular process to the other, and for more than 2 in. in a vertical direction from the fronto-nasal junction; the left ethmoidal cells were found to contain pus and were removed, also the supraorbital crests and a small amount of the orbital roofs. The cut edge of the bone was then mopped with pure carbolic acid.

The patient made a good recovery, and about four weeks later the flaps of skin were replaced and sutured.

DISCUSSION.

Mr. TILLEY: I congratulate Mr. Mollison on having saved this boy's life, because osteomyelitis of the frontal bone is nearly always fatal. Those who wish to inform themselves on the subject cannot do better than consult Dr. Dan McKenzie's article in the *Journal of Laryngology*, 1913. During the last six months I have been asked to see two cases presenting that condition: one not as a result of operation, but a complication of an acute, unrecognized frontal sinus suppuration of influenzal origin. The following were the main

features in the case: Intense œdema of the left upper eyelid, chemosis of conjunctiva and some proptosis. Œdema over the supra-orbital region which extended to the neighbourhood of the parotid gland and over the ascending ramus of the jaw. Deep pressure over the region of the frontal sinus caused much pain. Pus present in the left middle meatus. The frontal sinus was opened by an incision just below the level of the eyebrow. Pus and bubbles of air escaped. The whole anterior wall was removed and the inflamed mucous membrane wiped away. Small beads of pus could be seen in the diploë bordering the edges of the sinus. Pus continued to discharge freely from the wound for six weeks and it was irrigated twice daily. Meantime the non-inflammatory œdema of the skin on the left frontal bone continued to spread until it reached beyond the hair-line. The whole of the left frontal bone was then exposed and found to be extensively necrosed in many areas, while in others the bone was inflamed. In removing the necrosed portions, adherent dura mater and cortical substance of the brain came away. In spite of such desperate conditions, the patient has recovered completely although a metastatic abscess in the left breast and an attack of acute facial erysipelas after her return home gave us considerable anxiety. In the second case I adopted an incision which avoids the occurrence of the median vertical scar in the forehead. A transverse incision is made in the scalp beyond the hair-line, extending outwards and downwards to the temporal fossa; from the lower end of this a second incision is made forwards to the external angular process of the frontal bone where it joins the outer limit of the original incision in the eyebrow (for opening the frontal sinus). The small incision in the temporal fossa healed without leaving any visible scar.

Dr. DAN MCKENZIE: I also would like to congratulate Mr. Mollison on the very prompt and efficient manner in which he tackled the case, for to that was due the saving of the boy's life. Even when there is an abscess in the soft parts, one's mind is occupied about the frontal sinus, and if, on getting through the bone we happen to find it diseased, it then requires considerable moral courage to extend the operation sufficiently to get beyond its limits if they are wide. I advise caution in regard to the prognosis of these cases. This boy looks well now, but, unfortunately, in this disease one cannot be sure, until at least six months have passed, that the condition will not return. There have been cases in which the surgeon thought he had removed every bit of disease, and the patient remained well for a considerable time, but at the end of it there was a little puffy swelling in the neighbourhood, and it developed into a recurrence. But we must not be too despondent about these cases, because there have been cases in which prompt and thorough removal of diseased bone has resulted in permanent cure. And if there be no recurrence, in course of time the bone removed is replaced, so that in this case the forehead will be as good as before, as the frontal eminences will be restored. That, however, does not apply to the supra-orbital margins, I fear. My paper[1]

[1] *Journ. of Laryngol.*, January, February, March, 1913.

showed that the cases which had come on after sinus operation did not recover: the only cases which recovered were some of those which had been dealt with surgically in a very thorough manner, and in which the osteomyelitis had set in spontaneously.

Dr. P. WATSON-WILLIAMS: What were the organisms in this case? If these were determined, it might be an indication for treatment which might tend to keep off recurrences. I should also like to know how much bone was removed. The success in this case is certainly a matter for congratulation.

Dr. JOBSON HORNE: Dr. Dan McKenzie said that osteomyelitis is very fatal when it follows operation upon any sinus. Does he include in that the antrum? [Dr. MCKENZIE: Yes]. I should have thought that osteomyelitis following upon operation on the antrum of Highmore would be very rare, and if it did occur, that the patient would very likely recover. I agree as to the grave outlook in these cases generally, in fact I believe external operations on the frontal sinus are more fatal than we think.

Dr. IRWIN MOORE: Mr. Norman Patterson has asked me to show the photograph of a very interesting case of osteomyelitis which started after an operation on the frontal sinus. He operated upon the case twelve times, the whole of the frontal bone being eventually removed. The patient, an Australian soldier, has now rejoined his regiment. Mr. Patterson hopes to speak more fully on it when he returns from France.

Mr. O'MALLEY: I recently had a case which turned out eventually to be syphilitic, in which there was an infection by the ordinary micro-organisms. There was a history of the right frontal sinus having been opened, and of redness and tenderness over it, with swelling extending to the outer angle of the eye. A few days afterwards I laid it open, and tried drainage and ordinary treatment, but with no effect. There was no appreciable rise of temperature. In a few days the œdema had extended to the outer angle of the other eye and across the forehead, so that both eyes were closed with œdema. Therefore I laid him open from one malar bone to the other, and found that the walls of the frontal sinus had gone, though only one had been operated upon, and the bone had been replaced by granulation tissue, the latter having also replaced the supra-orbital ridges, and the fronto-malar joint was in a similar condition. After clearing it out thoroughly and dressing the wound, I thought I would have the Wassermann test done. It was positive. Improvement followed soon after the iodide and mercury mixture had been started, and after injection with "606" there was a marvellous clearing up. There may be a syphilitic factor in some of the cases one hears of. The present case seems to have been an ordinary acute osteomyelitis.

Mr. E. D. D. DAVIS: I have seen two cases of osteomyelitis following a radical operation for antral suppuration. In the operation I did, the antrum was vigorously curetted and the patient died of osteomyelitis and meningitis

some weeks later, in spite of a second operation. In another case seen by me, curetting of the ethmoid produced osteomyelitis and death. I have only seen one case of osteomyelitis following a frontal sinus operation. The frontal sinus had been attacked on three occasions by a surgeon, and I did a fourth radical operation establishing free drainage into the nose. The patient developed chronic osteomyelitis of the frontal bone and, I believe, died eighteen months later.

Mr. H. J. BANKS DAVIS: It would be interesting to know, in cases of osteomyelitis following the antral operation, whether the operation done was the intranasal one alone or the complete operation. I do not agree with the present tendency to do the intranasal operation—it does not suffice, because often one finds the antrum filled with polypi, which cannot be properly cleared out by operating through the nose only. This does not apply to acute cases, where polypi have not had time to form or the disease to become chronic.

The PRESIDENT: I am strongly of opinion that the antrum should be cleared out by the external operation, and then an opening made through the inner wall for dressing and washing out. I agree with Mr. Banks Davis that septic matter is very liable to be left behind when only the intranasal operation is done.

Mr. CLAYTON FOX: I should like to ask what the patient dies from in these cases of osteomyelitis in connexion with the jaw. The term osteomyelitis was applied a few years ago only to cases in which there was inflammation of the marrow of the bone. It now seems to be used if any part of the maxilla becomes inflamed. The nature of the operation is important. If the radical operation is accompanied by scraping out, there is more likely to be infection. But these cases of osteomyelitis can be separated from those in which there is extensive inflammation of the diploë between the two tables of the skull bones. Osteomyelitis arising from operation on the maxillary sinus is only a matter of infection which may occur even when only a portion of soft tissue is removed. There must be some inflammation after operation on any portion of tissue, and the bone concerned must be infected for the time being, though perhaps not sufficiently to produce general symptoms, such as pyæmia.

Mr. MOLLISON (in reply): I regret to say that the nature of the micro-organism is not known. The operation was done late at night; a swab was taken but seems to have been lost. With regard to the amount of bone removed, I took away both outer and inner tables, exposing the dura well on to the forehead almost as high as the upper limit of the vertical scar. I took away bone from the external angular process on one side, by a curved incision made to that on the other side, also the supra-orbital ridges and a portion of the roof of both orbits. I was not convinced I got beyond the disease at the right external angular process. There are two classes of cases of osteomyelitis of the frontal bone. One is typified by this case of

mine. The other class contains those in which the disease came on subsequently to the performance of a frontal sinus operation. These classes are to be separated from one another from the point of view of prognosis. I have no doubt that extensive operation at once is the only treatment: it is of no use to do the operation in portions. I have had three fatal cases in which osteomyelitis has come on subsequent to operations on the antrum or frontal sinus. I am sure the initial mistake of not doing enough at first is fatal. A little point of importance in this case was the treatment of the bare edge of the bone: I painted it with pure carbolic acid, in the hope that any organisms left in the wound would thus be prevented from getting into the bone surface exposed. I am interested in hearing Dr. McKenzie say that the bone will be replaced in course of time.

(*May* 4, 1917.)

Case of Carcinoma of Maxillary Sinus, Three and a Third Years after Operation, with no Recurrence.

By W. M. MOLLISON, M.C.

THE patient, E. C., female, aged 50, was shown to the Section in 1914. Original operation, January, 1914; removal of carcinomatous glands, April, 1914. Recently a gland was felt in the submaxillary region, and it was thought to be the seat of recurrence. It was removed in January, 1917, but microscopical examination showed it to be merely inflammatory.

Mr. HARMER: The notes do not state whether this carcinoma was of an epitheliomatous nature or of the columnar-celled variety. If it was an epithelioma it is a remarkable recovery; if columnar-celled there is nothing unusual in the history of the case.

(*May* 4, 1917.)

Skiagram of the Skull of a Patient suffering from Tumour of the Pituitary Body, with Brief Notes of Operation and Result.

By HERBERT TILLEY, F.R.C.S.

J. D. K., MALE, admitted on March 6, 1917. He gave a history of severe headache on the left side of some three and a half years' duration. About the middle of May, 1916, he noticed marked dimness

of left eye. August 21, 1916, an ophthalmic surgeon in New York found a "large scotoma (temporal) of left eye. The fundus of the same eye has small arteries and large veins." Mr. Cecil Snell, of Sheffield, saw the patient in October, confirmed the above findings, and found the right eye healthy, but on February 21, 1917, he discovered that the patient had bilateral hemianopia, "the right optic disk is slightly pale, the left one markedly so. There has been no optic neuritis." "He is now quite unable to read." Wassermann reaction negative. No improvement under iodide of potash.

On March 6, 1917, I saw the patient for the first time. When resting, his pulse was 60. Examination of nose revealed no abnormality beyond absence of left middle turbinal, which had been removed last year. I referred him to Dr. Finzi, who provided me with the radiographs exhibited.

March 22: Operation. Moure's lateral rhinotomy on left side; submucous resection of the posterior third of septum, removal of anterior wall and septum of the sphenoidal sinus, followed by removal of postero-superior wall of sinus and portion of tumour. A loose wad of gauze was inserted into sinus and the external wound was sutured. Great care was taken not to wound the easily visible lateral walls of the sinus and yet next morning the right upper and lower eyelids were intensely ecchymosed, but not swollen.

The patient made an excellent recovery, and the pulse, which was 54 on the evening before the operation, rose to from 65 to 72 during the time he was in the nursing home.

April 12: The patient can now read a little without any difficulty.

Remarks.—The exact nature of the tumour was not determined, and the ultimate issue of the operation cannot yet be stated, although there is marked improvement in his pulse-rate and the patient is delighted with the improvement in his sight. One of the main reasons for bringing forward the case is to emphasize the easy access to the sphenoidal sinus regions which was provided by Moure's lateral rhinotomy.

Dr. DAN MCKENZIE: We have not had many opportunities of seeing cavernous sinus thrombosis, but I suggest that in future it will be possible to make a surgical attack on the cavernous sinus by means of this operation. If I have such a case I shall certainly attempt to get at it through the nose, by the route so successfully adopted in this case by Mr. Tilley.

(*May* 4, 1917.)

Skiagrams illustrating Spasmodic Stricture of the Thoracic Œsophagus.

By Dan McKenzie, M.D.

The skiagrams were taken by Dr. Robert Knox.

The first (*see* figure), taken during the phase of active spasm, shows the bismuth mixture arrested about the level of the ninth rib. The shadow shows, by the typical pointed lower border, that there is no dilatation above the stricture.

The second shows the thorax in an interval between the attacks, and, the œsophagus being normal, the bismuth mixture is passing without any "hold-up."

The patient is a lady, aged 25. Occasional difficulty in swallowing was first experienced in 1910, and in 1911 the attacks became frequent and pronounced. She married in 1912 and has two children.

When first seen the attacks were very troublesome, coming on at irregular and unexpected times, and varying in degree, although, as a rule, the obstruction was so great as to prevent even soft solids being swallowed, and sometimes liquids were also arrested and obstruction was absolute. The attacks lasted from a few seconds to several days, and the patient was always aware of their onset by feeling a "grip" in her throat. The relaxation of the spasm was also manifested by a feeling of relief. During the intervals between the spasm the patient could eat anything. She was seriously emaciated when first I saw her. Since treatment was begun the attacks have practically ceased; none has lasted more than a few seconds, and these have only occurred once or twice in a month. The treatment consisted in the administration of bromide, and during an attack, of atropin $\frac{1}{150}$ gr. sublingually. The improvement, however, set in immediately after the first X-ray examination.

Mr. Dawson: I recently had a case which might have been one of spasmodic stricture of the œsophagus—a young woman who for a long time had had difficulty in swallowing. I passed the œsophagoscope, and she said she was much better, and she remained better for a month. Then she returned with a slight recurrence, and the passage of the œsophagoscope relieved her again.



Skiagram taken by Dr. R. Knox during phase of active spasm. Bismuth mixture arrested at level of ninth rib.

(*May* 4, 1917.)

Retro-pharyngeal Abscess; Injury to the Cervical Sympathetic —an Unusual Complication following Operation by the External Route.

By Irwin Moore, M.B.

This patient, a female, aged 25, was shown at the meeting of this Section on April 7, 1916,[1] two weeks after operation. The abscess was



Fig. 1.

Retro-pharyngeal abscess due to breaking down of a tubercular gland—in a female aged 25, operated on by the external route.

opened by careful dissection along the posterior border of the right sternomastoid, ½ oz. of pus evacuated, the abscess wall, which was found to be much thickened, was curetted, and a drainage tube inserted. Examination of the pus showed tubercle bacilli. The wound healed in six weeks and there has been no recurrence. After the operation the

[1] See *Proc. Roy. Soc. Med.*, 1916, ix (Sect. Laryngol.), p. 114.

patient unfortunately suffered from paresis of the right cervical sympathetic manifested by:—

Pseudo-ptosis—slight drooping of the upper lid causing narrowing of the palpebral fissure, due to paralysis of the unstriped muscle fibres of Müller.

Enophthalmos—sunken appearance of the eyeball, which looked smaller than its fellow, and resembled a badly fitting artificial eye.



FIG. 2.

Retro-pharyngeal abscess operated on by the external route. Injury to the cervical sympathetic. Shows pseudo-ptosis, enophthalmos, and contracted pupil. Recovery.

Contraction of the pupil with absence of dilatation on shading the eye.

Paresis of the upper portion of the right trapezius muscle was also present—shown by the fact that the patient was unable to shrug her

shoulder when pressure was made from above. (Cervical plexus from third and fourth nerves.)

The case is again shown because there was considerable doubt as to whether the paresis would improve, and to draw attention to the possibility of such an occurrence with its accompanying disfigurement, which is ignored in surgical text-books. Treatment by faradism was carried out. Fortunately the symptoms have now cleared up, as may be seen by a comparison with the photograph taken two weeks after operation.

DISCUSSION.

Mr. H. J. BANKS DAVIS: In these cases I am now in the habit of opening through the mouth, though I know it is supposed to be a wrong procedure. Sometimes there is great difficulty by the neck route. I remember operating on a child aged 8 with retro-pharyngeal abscess and secondary œdema of the neck. Landmarks were obliterated, and the incision exposed the anterior border of the sternomastoid instead of the posterior, and in opening the abscess the greatest difficulty was experienced, the neck wound being of enormous depth. I now employ the buccal route with sinus forceps, and I have not seen trouble following. As students, we were taught it was fatal to open through the mouth, though I believe that this is the method in vogue at Great Ormond Street Children's Hospital.

Mr. TILLEY: I uphold what Mr. Banks Davis says and I do not think it is a wrong method. At a large Fever Hospital in London they make no difficulty about opening retro-pharyngeal abscesses from inside the mouth. No anæsthetic is given, and the patient's head is held as if for an examination for adenoids. The surgeon opens the abscess with his index finger and, after evacuating the pus, the patient is returned to bed, and made to lie for two or three days with the head hanging backwards over a pillow, so that no pus can be inhaled into the lower air passages. The cases seem to do very well. The external route should, of course, always be employed when the retro-pharyngeal abscess is due to caries of the cervical spine.

Dr. IRWIN MOORE (in reply): The last two speakers referred to retro-pharyngeal abscesses in children, and there is no doubt that in these cases opening from the mouth is the correct way. In the case I have shown there was a question as to whether it should be done through the mouth or through the neck. This patient's age was 25, and her child had recently been operated upon for tuberculous glands in the neck. I thought this abscess might be found to be tubercular, and therefore it would be better to drain it from the outside to prevent the discharge re-infecting her. The pus from the abscess contained tubercle bacilli. The sac of the abscess was so much thickened that the question of dissecting it out was raised, but I did not attempt it. The wound healed up in six weeks, and the thickening and swelling have now disappeared.

(*May* 4, 1917.)

Syphilitic Disease (Gumma) of the Larynx, treated by Tracheotomy and Galyl Injections, complicated by Arsenical Poisoning; Recovery.

By Irwin Moore, M.B.

This patient, a female, aged 39, was shown at the meeting of the Section on December 3, 1915, and April 7, 1916—a complete history appearing in the *Proceedings*[1] of the latter date. She complained of hoarseness for two months. There was very marked swelling of the left aryepiglottic fold and ventricular band, and the left vocal cord was fixed in the cadaveric position. Perichondritis of the larynx was also present. There was no history of syphilis and the possibility of malignant disease was at first considered.

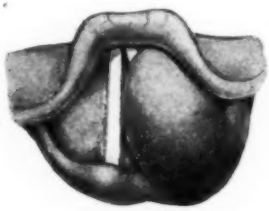


Fig. 1.

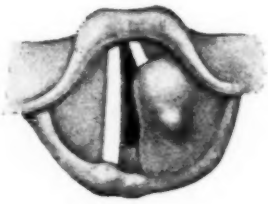


Fig. 2.

Fig. 1.—Gumma of larynx treated by tracheotomy and galyl injections (April 6, 1916).

Fig. 2.—Gumma of larynx treated by galyl injections. Shows improved condition of larynx on May 4, 1917.

On account of progressive stridor, tracheotomy was performed on March 1, 1916. Three days later the Wassermann reaction was found to be positive. An intravenous injection of galyl was given on March 22 and a second on May 2. Following this injection the patient suffered severely for five days from arsenical poisoning, manifested by the following symptoms: Intense headaches and pains all over the body, severe paroxysms of colic, vomiting, watery diarrhœa, muscular tremors, suppression of urine, diminution of vision, and temporary attacks of

[1] *Proc. Roy. Soc. Med.*, 1916, ix, pp. 117, 118.

blindness with dilatation of pupils, paresis of extensor muscles of forearms and dropped wrist, great tenderness on pressure of muscles of forearms. She lay curled up in bed resenting interference, drowsy and at times comatose. As a result of the galyl injections, she soon improved, and the tracheotomy tube was removed on July 7—four months after insertion.

A comparison of the state of the larynx on April 6, 1916, and the present condition (as seen by the drawings exhibited) shows very great improvement. The swelling has considerably diminished, the glottic space is more open. The left vocal cord is now fully exposed, but fixation is still complete. Breathing is quite free and there has been no recurrence of stridor. The patient is greatly improved in health.

Slight discharge still continues from the tracheotomy wound; this is probably caused by infiltration of the thyroid isthmus—caseous nodules having been observed during the tracheotomy.

The special interest of this case lies in the fact that a well-defined, circumscribed gumma of the larynx is uncommon and is generally followed by ulceration, but this did not occur, probably as a result of the galyl treatment.

DISCUSSION.

Dr. JOBSON HORNE: Is it worth while to run the risk of the symptoms that follow this treatment when excellent results are obtained by simply using iodide of potassium and mercury?

The PRESIDENT: Not long ago, I was asked to see a case of severe ulceration of the throat. The patient was under a surgeon in London and a general practitioner in the country. After seeing the case, I wrote and suggested the patient should have 10-gr. doses of iodide of potassium and—what is often omitted, though well known to our forefathers—opium, and another old-fashioned remedy, sarsaparilla. After taking it about two days, the patient came to London to have his first injection of galyl. After that, he continued to take the medicine, and when he came up for another galyl injection, the surgeon said he had never seen such a wonderful result from one injection. Of course the improvement was due to the iodide of potassium *plus* the opium and sarsaparilla. The value of opium in the treatment of severe syphilitic ulceration appears to be somewhat forgotten by the present generation.

Dr. DONELAN: A suppository method of injecting salvarsan has been introduced by Allen and Hanbury. As it takes six suppositories to equal one dose of salvarsan, the treatment can be conveniently varied. In one case where we repeated the series of six, as the patient had been taking arsenic as a corrective of the iodide of potassium, he developed an intense arsenical rash.

These suppositories have yielded excellent effects. I do not think tertiary manifestations can be adequately treated without mercury. There is often a tendency to use iodide of potassium alone.

The PRESIDENT: I have known 1-gr. doses of iodide of potassium act beneficially when larger doses produced unpleasant symptoms.

Dr. IRWIN MOORE (in reply): When I first saw this patient I treated her with potassium iodide and liquor hydrarg. perchlor., but it did not suit her, and she got œdema of the larynx. She had a large circumscribed gumma, causing stridor, for which tracheotomy was necessary. Recently I have tried to get the thickening, which remains, further reduced by again giving potassium iodide and mercury, but patient cannot tolerate potassium iodide except in 2½-gr. doses. At the time I used the galyl there were some samples on the market which were not good in some way—the powder would not dissolve properly—and as a result of my calling attention to it, the agents in London investigated the matter. The instructions supplied by the makers, in their circular, to use a concentrated solution of 10 c.c., is very dangerous, and I have no doubt was the cause of the poisoning in this case. It should not be injected in this strength, but diluted up to at least 20 c.c. with normal saline.

Postscript.—The discharging sinus has now healed as a result of treatment by chromic acid.

(*May* 4, 1917.)

Case for Diagnosis.

By A. L. MACLEOD, M.B.

W. J. H., CLERK, male, aged 57, first seen on February 23, 1917, complaining of hoarseness which had lasted for a year. There was no pain, no cough, no dysphagia. He had had very little treatment. There was some deviation of the septum, both vocal cords were inflamed, and both ventricular bands swollen. After appropriate treatment he improved, but there remained a superficial ulceration of the right vocal cord and part of the anterior portion of the left.

On March 26 he saw Sir StClair Thomson, who wished to be certain that his case was not tubercular. The tuberculosis officer (Leicester) reported on April 14 that he could detect no definite signs of phthisis. Three films from the sputum were examined, but no tubercle bacilli were found. Morning and evening temperatures were taken without showing anything definite.

Mr. H. J. BANKS DAVIS: I think it is either chronic tuberculosis or pachydermia laryngis. It most resembles the latter.

(*May* 4, 1917.)

Case of Nasal Fibroma (of the Right Fossa) with Specimen and Microscopic Section.

By L. H. PEGLER, M.D.

THE patient, a man aged 60, is exhibited one month after removal of the growth by forceps and snare intranasally; the stump being left for the present to show its place of origin from the lower border and septal aspects of the middle turbinate, and also the firm, non-œdematous character of its tissue. The tumour was visible for some time as a smooth, round body projecting slightly below the right anterior naris; the macroscopic specimen includes this part of the lobulated mass intact, much shrunken by spirit; it was not very vascular, and the bleeding was quite moderate. The microscopic section shows the fibrous character of the tumour and especially the abundance of the glandular element. A further report will be supplied later. Few of these nasal fibromata have been shown here, perhaps not one since Dr. Jobson Horne's case in December, 1896.

DISCUSSION.

Dr. JOBSON HORNE: As Dr. Pegler has referred to what I did at the end of the last century, it is only right to explain that, in the light of recent work done by Dr. Pegler, this case would have been labelled "bleeding polypus." It contained more angiomatous than fibrous tissue, and the symptoms were those of bleeding polypus. Therefore, while I was one of the first to show bleeding polypus before this Section, Dr. Pegler is probably the first to show a pure nasal fibroma.

Dr. PEGLER (in reply): There are two points of interest. Anyone examining the scar or stump may gain some information from it. The stump which remains, though it has not been touched since the first operation, is different from that which one finds after operating on fibro-angioma. This will be seen on comparing my case with Mr. Dawson's. One notes its redness and firmness and I may add—that it has not altered since the third or fourth day after the operation. It was essentially a dry, non-vascular growth. What Dr. McKenzie saw and took for vessels are cross-sections of the muciparous ducts. Where there are ducts there must be glands, and another part of the section shows them. The other large spaces are fissures or intrusions of epithelium such as one finds in many nasal growths, especially of the turbinals. They are lined with epithelium and very abundant. Mr. Shattock found these specimens so interesting that he furnished me with a special report, which I shall publish in the *Proceedings* later.

(*May* 4, 1917.)

Case of Aphonia.

By G. C. Cathcart, M.D.

Patient, a female, aged 25, was under the care of Dr. Bryan from January, 1914, till the end of July of the same year, and was treated for general weakness, and during April and May for nervous breakdown, laryngitis, and aphonia (one week).

In January, 1915, she again developed laryngitis and aphonia, and was treated in the usual way for six or seven weeks. She was sent to the London Throat Hospital in March. I applied electricity, and her voice lasted till the end of May. She then lost it for a week, but it resumed its normal condition during a Zeppelin raid on the night of June 4.

At the end of March, 1916, her voice had again disappeared. Dr. Bryan applied electricity nearly every night until the end of November, when he made an appointment with me to see her. She was examined by the X-rays for tuberculosis with a negative result. I then applied electricity, and her voice lasted for ten days; after that Dr. Bryan applied electricity, which had no effect. He then sent her to me and I made another application of electricity, with the result that the voice resumed its normal condition. She went to Eastbourne for a month and spent three weeks in London.

In 1917 she obtained work at the War Office, and in another few days she again lost her voice. Between February and the end of March I treated her with the faradic current three times, but her voice only lasted for a few days after each application. The following are the results of last week's treatment by electricity to date (April, 1917): First night, voice lasted three hours, normal condition; second night, voice lasted one and three-quarter hours, normal condition; third night, voice lasted two and a half hours, normal condition; fourth and fifth nights, voice normal at bedtime but failed by morning; sixth night, voice lasted half an hour.

I should be glad to receive suggestions for treatment.

Mr. H. D. Banks Davis: It is a case of functional aphonia, the bowing of the cords being typical.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

EDITED BY

J. Y. W. MacALISTER

UNDER THE DIRECTION OF

THE EDITORIAL COMMITTEE

VOLUME THE TENTH

SESSION 1916-17

SECTION OF MEDICINE



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1917

Section of Medicine.

OFFICERS FOR THE SESSION 1916-17.

President—

ARCHIBALD E. GARROD, C.M.G., M.D., F.R.S.

Vice-Presidents—

H. P. HAWKINS, M.D.
H. D. ROLLESTON, C.B., M.D.

Hon. Secretaries—

CHARLES R. BOX, M.D.
W. CECIL BOSANQUET, M.D.

Other Members of Council—

J. CHARLTON BRISCOE, M.D.
JOHN FAWCETT, M.D.
DAVID FORSYTH, M.D.
A. J. HALL, M.D.
P. HORTON-SMITH HARTLEY, C.V.O., M.D.
ROBERT HUTCHISON, M.D.
A. G. PHEAR, M.D.
H. BATTY SHAW, M.D.
F. PARKES WEBER, M.D.
F. J. WETHERED, M.D.

Representative on Library Committee—

G. NEWTON PITT, M.D.

Representative on Editorial Committee—

CHARLES R. BOX, M.D.

SECTION OF MEDICINE.

CONTENTS.

November 28, 1916.

H. Batty Shaw, M.D., and A. W. Woo. PAGE

Six Cases of Œsophagectasia 1

F. Parkes Weber, M.D.

Acquired Syphilis of the Lungs 23

June 12, 1917.

Aldo Castellani, M.D., M.R.C.P., Lieutenant-Colonel, Roy. Ital. Navy.

Tropical Diseases in the Balkans 81

The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

LONDON:
JOHN BALE, SONS AND DANIELSSON, LTD.,
OXFORD HOUSE,
83-91, GREAT TITCHFIELD STREET, OXFORD STREET, W. 1.

Section of Medicine.

President—Colonel A. E. Garrod, A.M.S., F.R.S.

(*November* 28, 1916.)

(Chairman—Dr. F. Parkes Weber.)

Six Cases of Œsophagectasia.

By H. Batty Shaw, M.D., and A. W. Woo.

Probably most museums of pathological anatomy contain specimens showing dilatation of the œsophagus, which involves the greater part of the tube, and is not due to the existence of a pouch. But though the condition is well known to the pathological anatomist, until recent years the clinician has been at a loss how to detect it, and has generally only discovered it at the post-mortem examination. The advent of the use of X-rays and bismuth meals, and of the œsophagoscope has rendered it possible for this condition to be demonstrated before death. Dr. F. Parkes Weber is the first observer of recent years to publish a case of this kind which was completely diagnosed before death.[1] Major A F. Hurst has more lately described a case discovered before death by means of X-rays, and has used the term "Achalasia of the Cardia"—perferring this term to that of "Cardiospasm."[2]

We have met with five cases, three of which were fatal, and we add a sixth which came under the care of Dr. Sidney Martin in 1892. With his permission we publish the notes of his case, because it presented similarly baffling symptoms at the bedside.

[1] *Proc. Roy. Soc. Med.*, 1914, vii (Clin. Sect.), p. 147.

[2] *Proc. Roy. Soc. Med.*, 1915, viii (Clin. Sect.), p. 22.

Case I.—E. L., governess, aged 35, was admitted into University College Hospital under Dr. Martin's care, on September 17, 1892, and died on October 22, in the same year. She was admitted because she had had attacks of great difficulty in breathing for about six weeks. Her story was that she had really been ailing for a little over two years, and her illness had begun whilst abroad, with pain in the upper abdomen, accompanied by vomiting, which relieved the pain. There was no hæmatemesis, and she was treated, she said, for "congestion of the stomach." Similar attacks occurred periodically during the ensuing year; but in October, 1891, the pain became persistent, vomiting again giving her relief. These symptoms became intensified until six weeks before admission, when a new symptom developed —namely, attacks of dyspnœa which came on night and day, especially at night and on lying down: the attack would last about half an hour, and, as above stated, it was this symptom which caused her to seek admission. With regard to the pain and vomiting, the latter was lost altogether and the pain was much reduced if her diet consisted only of milk.

Physical Examination.—The patient was a spare woman, and she showed marked inspiratory stridor on occasion, especially after exertion. There was frequent cough, sometimes "brassy" in character: the expectoration was mucoid and watery. The heart and lungs were found to be normal, and repeated examination of the larynx failed to show any abnormality. She had great difficulty in taking solid food: her sleep was disturbed by a cough, and whilst in hospital this grew worse, her temperature rose a little, and she developed dullness at the right base behind. During the night of October 14 she had a very severe attack of dyspnœa, and stridor of an inspiratory type was again noticed. The use of amyl-nitrite and of morphia failed to give relief, whereas chloroform was effective. She did not become blue during the attack.

The cough persisted and the sputum became muco-purulent. On the evening of October 20 the epigastric pain became very severe and a return of her dyspnœa took place. The stridor was mainly inspiratory, but also expiratory. The patient became very livid, and chloroform, pushed to deep anæsthesia, gave no relief to her breathlessness. She then became unconscious (after recovery from the chloroform anæsthesia) and remained so for forty hours until her death; only on one or two brief occasions was there any slight return to consciousness; during this period the pulse failed occasionally, and the veins of the neck became very turgid. During the forty hours there were many attacks of dyspnœa, one or two of which were very severe; in the intervals the breathing was quiet, and the patient's colour was good. Finally the pulse failed, and death from exhaustion ensued. The vomiting *seemed* to be a true vomiting and not a mere regurgitation of food, and took place some time after taking food. On one occasion only had it been noticed that food taken some hours before was returned unaltered. Nothing abnormal was found in the larynx on repeated examination. *Vomiting was usually brought on by an attack of coughing.*

Post-mortem Examination.—The lungs appeared emphysematous. The right lung was adherent by old adhesions at the apex, the lower part showed recent adhesions, and there was a small pleural effusion at the right base. There were patches of broncho-pneumonia in the lower lobe; the left lung was congested in the lower lobe. The heart was normal, but a soft cystic swelling bulged into the back of the pericardial space. On raising the right lung a sausage-shaped tumour was seen lying between the spinal column behind and the trachea and pericardium in front. It bulged into the right chest, and proved to be the œsophagus greatly dilated; it was filled with undigested food, and no stricture could be made out. *The finger could be passed through the cardiac opening of the stomach into the œsophagus, and no resistance was met with in doing this.* The trachea showed no constriction, but a submucous extravasation of blood was seen on the posterior surface near the bifurcation; the cartilages appeared to be a little softened, as though from pressure; the brain, heart, kidneys and liver were normal.

Report on the Œsophagus by Mr. T. W. P. Lawrence.—An œsophagus measuring 24 cm. in length, with the cardiac end of the stomach. At the upper end the internal circumference measures 4·5 cm., at the cardia 5 cm. Between these points, involving the whole œsophagus, there is a fusiform dilatation, which reaches its maximum width at about an inch below the middle, where the internal circumference is 13 cm. The wall of the tube is slightly increased in thickness at the lower end (about 4 mm.) and becomes gradually somewhat thinner towards the upper part. In the main part of the dilatation the rugæ are smoothed out, but are still present at the upper and lower portions of the tube, and in the latter situation the mucous membrane is swollen or altered in appearance so as to resemble somewhat the mammillated mucous membrane of chronic gastritis, and in many places it shows with a lens a finely papillary surface. The epithelium in the most dilated part of the tube is irregularly thickened, and in places destroyed, leaving small areas of ulceration in which the muscular coat is laid bare. No ulcers or cicatrices are present in the lower part of the œsophagus, or cardiac end of the stomach. The submucosa appears to be everywhere loose and free from induration. At the lower end of the specimen on its external aspect, there is a small group of slightly enlarged lymphatic glands, one of which contains caseous foci; these are connected with the stomach rather than with the œsophagus, though at one spot they slightly overlap the extreme lower end of the latter.

Microscopical Examination.—(*a*) Chronic catarrhal inflammation: This is most marked at the lower part of the œsophagus. The epithelium is thickened, shows a tendency to surface desquamation, and the papillæ are increased in size, forming bead-like projections above the general surface. There is some induration of the areolar layer between the epithelium and the muscularis mucosa, with leucocytic exudation. The submucous tissue and the areolar tissue between the circular and longitudinal muscle layers are also in places slightly indurated. In the smooth area of the more dilated part of the

œsophagus these changes are less marked, and the papillary hypertrophy is absent. (*b*) Muscular hypertrophy: Judging from the thickness of the circular muscle fibres, this appears to be slightly hypertrophied in the lower half of the œsophagus. (*c*) Muscular atrophy: Many of the bundles in the circular layer of muscle fibres are very slender and obviously atrophic. This condition is fairly uniform in the more dilated part of the tube; but only occurs in localized patches at the lower end. (*d*) Definite staining for fat with Scharlach R. and Sudan III is not obtainable.

Case II.—J. H., an accountant, aged 55, was admitted to University College Hospital on May 21, 1914. He complained of the following symptoms: (*a*) general weakness for a month, much worse one week; (*b*) difficulty in keeping his food down, particularly solid food, on and off for eighteen years; it had become much worse in the last six weeks, when his food had to be returned immediately after taking it; (*c*) pain in the chest on and off, not related to the actual taking of food, most marked during the preceding month, and much worse the last week; and (*d*) during the last two weeks he had developed shortness of breath and cough. His symptoms began eighteen years previously, when after having had dinner he had a fit of coughing and fell down unconscious. He recovered and felt quite well, but three days later he found he could not swallow properly. He went to Guy's Hospital and was under the care of Mr. Charters Symonds. Thence he was sent to St. Bartholomew's Hospital and remembered he was "fed with a tube." He had had pneumonia at the age of 17, typhoid fever at the age of 23, and influenza and bronchitis at the age of 51. There was no history of syphilitic infection, nor was there any history of a similar sort of "dyspepsia" in his family. He was moderate in the consumption of alcohol and in the use of tobacco.

Physical Examination.—On admission he was found to be wasted, was rather short of breath, had some pain in the upper abdomen which was bad enough to disturb his sleep very much, and he had some cough and brought up grey expectoration. He was tender in the upper part of the abdomen, to the left of the middle line. There were no abnormal physical signs in the heart, the blood-pressure registered 94 to 116 mm., the pulse was regular, 100 to a minute, and the temperature was 99° F. The lungs were emphysematous and rhonchi were heard on both sides.

X-Ray Examination by Mr. R. Higham Cooper (*May* 25, 1914).—A bismuth emulsion appeared to pass straight into the stomach, and there was no sign of an œsophageal pouch. The plate, however, showed a considerable increase in width of the shadow usually seen at the base of the heart.

An œsophageal bougie, the size of the little finger, was passed by Mr. Herbert Tilley without any difficulty, and *apparently* into the stomach, and the patient said he had felt "relieved by this, as if something had been pushed on." A rather small-bore œsophageal tube was then passed for 21 in. This caused immediate discomfort and retching, and when withdrawn a piece of

carrot, eaten twenty-four hours before and practically unaltered, was found in the eye of the tube. Œsophagoscopy showed a dilated œsophagus, the walls of which were folded, presenting depressions and crevices which caused confusion when an attempt was made to pass a bougie through the œsophagoscope into the stomach. The attempt to do this failed entirely. Analysis of the vomited matter showed no *free* hydrochloric acid, but some lactic acid. The total acidity was 0·073 HCl grm. per cent., the proteid HCl was 0·036 HCl grm. per cent., and the lactic acid 0·045 HCl grm. per cent. There was a trace of albumose present and there was only a small amount of digestive power (Dr. F. H. Teale). The vomit varied in amount from 1 oz. to as much as 38 oz. Attempts were made by liquid dietary and rectal feeding to maintain the patient's strength, but he wasted rapidly and died on June 1, 1914, in an attack of angina pectoris.

At the post-mortem examination the patient was found to have a dilated œsophagus and calcification of the coronary arteries; beyond this no other abnormality was found.

Report by Mr. Lawrence.—An œsophagus measuring 28 cm. in length, with the cardiac end of the stomach and the bifurcation of the trachea. From above down the œsophagus presents the following peculiarities: (*a*) For 6 cm. as far down as the bifurcation of the trachea a slight cylindrical dilatation and marked hypertrophy. (*b*) For 17 cm. below the bifurcation a large fusiform dilatation with a maximum internal circumference of 16 cm. at a little below its middle; marked hypertrophy of the walls in the upper third of this part, with a maximum thickness of 6 mm. at its highest part, the wall gradually thinning to 2 mm., somewhat below the middle and again increasing at the lower end of the dilatation. (*c*) For 4 cm. below the fusiform dilatation a slight cylindrical dilatation, with marked hypertrophy (7 mm.). (*d*) The lowest 1·5 c.m. of the tube normal. The rugæ are smoothed out in the more dilated portion, and the mucous membrane shows patchy thickenings of the epithelium, which in places has become detached, leaving shallow circular and irregular ulcers. There are no ulcers nor cicatrices in the lower part of the œsophagus or cardiac end of the stomach. No enlarged glands, induration or other changes are present round the lower extremity of the œsophagus.

Microscopical Examination.—(*a*) Chronic catarrhal inflammation: The epithelial layer is narrow generally, owing apparently to desquamation; in the most dilated portion the epithelium is absent in places. Between the rugæ at the lower end the papillæ are hypertrophied, forming small projections on the surface. The areolar tissue between the muscularis mucosæ and the epithelium and the adjacent part of the mucous layer is infiltrated with leucocytes and is slightly indurated. In the lower part of the œsophagus there is some fibrosis of the layer of longitudinal muscle and of the areolar tissue between the two muscle layers. (*b*) There is marked hypertrophy of both muscle layers, except at the most dilated part of the œsophagus. (*c*) There is marked atrophy of the bundles of circular fibres in the region of greater dilatation.

The following deductions have been made by Mr. Lawrence on these two specimens: (1) That the inflammatory changes are simply due to irritation by retained food. (2) That the hypertrophy, even when it extends to the upper part of the tube, must be due to obstruction at or near the lower end. (3) That the atrophy is due to stretching of the walls owing to dilatation by food retention, and that it may be preceded by hypertrophy. (4) That in Dr. Martin's case there never was much hypertrophy, and that therefore hypertrophy is quite a secondary process in these cases: in fact all the changes seem to be secondary.

Case III.—A. B., aged 50, housekeeper, had been anæmic as a young woman, and liable to faint. At the age of 30 she hurt herself by swallowing a rather large fish-bone. She first noticed a trace of dyspepsia at the age of 33, when she began to observe that swallowing effervescing drinks like ginger-beer caused her great discomfort in the chest. Eleven years ago a definite pain developed behind the sternum, which was relieved by bringing up wind. Cold food, solid or liquid, always caused the greatest degree of this pain. Solid foods generally seemed "to stick" at the epigastrium, and caused her to return the food, or at any rate if she made herself sick she was much relieved of the pain; butter and fat foods also seemed to increase this pain; the pain did not necessarily come on as the result of taking food; it was accompanied by a feeling of distension of the abdomen. During the night mouthfuls of food, not very acid, would be brought up, and sometimes be expelled through the nostrils. It was about this same time that she first noticed shortness of breath after taking solid food, and relief came only by her bringing back her food. When seen by one of us at this stage naturally some form of obstruction was feared. She did not, however, seek further advice for eighteen months, and then as she was well nourished, and her symptoms had not intensified since she had kept to a liquid or a very light diet, a tentative diagnosis of dyspepsia was made; her teeth were put in order and a stomachic mixture containing bismuth was prescribed; this latter apparently relieved the slight symptoms that persisted despite the light character of her diet. In the course of time she got tired of the diet, which consisted of milk, custards, bread and milk, and scrambled eggs, and came under the care of Dr. J. C. Williams, who was struck with the persistence of the symptom that food apparently "stuck in the chest," and suggested the use of a bismuth meal and X-ray examination. This was carried out by Mr. R. Higham Cooper on September 30, 1914. He found that "on examination immediately after the meal no shadow of the stomach could be made out. All the bismuth meal appeared to be in the œsophagus, which showed enormous general dilatation, with a small process below and to the left, evidently corresponding to the cardia. Half an hour after the meal small portions of opaque matter were seen ejected from the œsophagus at the end of each forced inspiration. One and a half hours after the meal was taken there was a mass with rounded lower border, visible below and to the left of the umbilicus, evidently representing the greater curvature of

the stomach. Above this was a long spindle-shaped mass in the position occupied by the ejected particles mentioned in the previous examination. Four and a half hours after the meal was taken the shadows in the stomach had disappeared; but that in the œsophagus was practically unchanged. Further examination could not be made, as the patient became sick and faint. The condition appeared to be one of almost complete obstruction of the cardia, with consequent dilatation of the œsophagus. On pressure being applied to the œsophagus by inspiratory movement, the fluid parts of the meal seemed to be squeezed through the cardiac orifice. . . . ."

This patient is alive still, is very well nourished, and so long as she adheres to a dietary such as has been indicated above, she is free from all but the slightest symptoms. Owing to the absence of Mr. Cooper in France we are unable to reproduce his pictures, and Dr. Stanley Melville has radiographed her again, and reports as follows: "Opaque porridge passed without difficulty as far as the lower end of the œsophagus. At this point further quantities of the meal were seen to collect above the constriction, the œsophagus dilating to accommodate the food. There was some tendency for the food to regurgitate, and what appeared to be reverse peristaltic waves were noted in the œsophagus. The appearance of the œsophagus at the conclusion of the meal bore the customary shape of cardiospasm, the sharp 'sickle-shaped' outline to the right of the cardiac shadow being well marked. In this case, the usual thin line of food as it slowly passed the obstruction was not very evident, but small quantities of the meal were seen at intervals to enter the stomach."

Case IV.—F. P., aged 24, accountant's secretary, came under the care of Dr. J. A. Belcher, at the beginning of this year, for cough and hæmoptysis, which he found was due to pulmonary tuberculosis. She had also noticed for the previous three years that when she took solid food it appeared to stick in her chest behind the breast-bone, and on occasions the food would come up, apparently little altered. Liquid food did not cause any trouble; sometimes the food brought up had been taken a day or so before. She had lost nearly 2 st. in weigh in three years, and was short of breath, two symptoms, however, as easily attributable to her lung condition as to that of the œsophagus. With careful dieting the regurgitation of the food became much less frequent, and if she ate slowly and thoroughly masticated her food she could retain fish, chicken, and even meat, though this was not always the case. It was distinctly noticed that sometimes she had a difficulty in retaining food, whereas at others, on the same diet, she had none. Dr. Stanley Melville carried out an X-ray examination on March 17, 1916, using a bismuth meal, and reports as follows: "The opaque meal of the consistency of porridge passed without delay or obstruction until the lower limit of the œsophagus was reached. At this point the food was arrested, further quantities of the meal accumulating at the expense of the œsophagus, which continued to dilate until a large mass, with a well defined and characteristic curve to the right, was seen. This was distinguishable from the right border of the heart. After

an interval of about ten minutes, a thin, somewhat long, and sinuous streak was seen to be entering the stomach. There was very marked delay in the passage of the food into the stomach, and at the end of an hour only a portion of the meal had passed the constriction."

On April 7, 1916, the patient was again placed under X-ray examination. Considerable improvement was observed in the transit of the bismuth meal; for at the end of fifteen minutes the greater part of the bismuth had reached the stomach. Dr. Dundas Grant was able to carry out an œsophagoscopic examination and to demonstrate the folds and recesses met with in the walls of what was evidently a dilated œsophagus.

The radiographic study of the two last cases was interesting in that besides giving the true diagnosis, an interesting phenomenon was noticed when the patient took a deep breath. A pencil-like shadow of bismuth could be seen, indicating the narrow lower end of the œsophagus and the cardiac orifice of the stomach. This would remain unaltered for some time, and so far as a radioscopic examination was concerned no food was taken into the stomach, showing the obstruction was probably complete, even for such a weighty salt as the one of bismuth used. However, this obstruction could be overcome in both cases when the patient took a deep breath or coughed; for then a fainter shadow of the bismuth meal could be seen, indicating that some bismuth escaped on such occasions into the stomach. The obstruction was not absolute.

Case V.—Through the courtesy of Dr. Arthur Latham and Mr. G. F. Darwall-Smith we are able to publish the details of a case which came under the observation of one of us (A. W. Woo) at the General Lying-in Hospital, York Koad, S.E. The patient subsequently died at St. George's Hospital. Mrs. E. T., aged 33, the mother of three children, was admitted to the hospital on January 28, 1915. She was thirty-two weeks pregnant; as she had frequent vomiting, which could not be controlled, she was admitted as a case of "pernicious vomiting of pregnancy." The following points in the history of the case were elicited, part being obtained from the husband, because the patient herself had developed defective memory. It was found that she had never had any serious illness before, but that she had always to swallow slowly and that the difficulty in getting the food down had for the last ten years been increased, so that even slow swallowing did not obviate it. During these ten years the food would occasionally be actually brought back as if she had vomited it, and there was an accompanying sensation of nausea. This return of food, it was found on close inquiry, only followed immediately on the ingestion of food; that is to say, it did not occur during periods of fasting. In November, 1914, the patient had a very severe attack of coughing, which seemed to intensify the habit of bringing up her food after swallowing it, so much so

that from this date until her admission to hospital, it was a constant feature, and for four days before admission she had been quite unable to retain any food given by the mouth. The attack of coughing which began this phase of severe vomiting was not an isolated attack. She was liable to similar attacks, and even had had attacks of "asthma." When she swallowed food she noticed that it "seemed to stick" at a point referred to the lower end of the sternum.

The clinical study of this case belongs to the patient's stay under Mr. Darwall-Smith's care at the General Lying-in Hospital, and under Dr. Arthur Latham's care at St. George's Hospital. On admission to the former hospital she was found to have a distressed facial aspect, to be emaciated, was suffering from bronchitis and was slightly deaf (the deafness having come on two days previously): the temperature was sub-normal and the pulse-rate 120 per minute throughout her stay. No abnormality was found in the urine. The bowels were constipated; there were some scars on the right side of the neck suggesting former glandular disease; her nervous symptoms were such as to suggest neurasthenia. Various remedies were tried to check the vomiting, and the simplest of dietaries, consisting of albumen water or peptonized or "citrated" milk in small quantities, was adopted. Despite these efforts the food was rejected after being swallowed, very often being associated with a bout of coughing, or the bringing-up of phlegm from the pharynx. Thinking over the case at the present date, it seems to us very possible that the very act of coughing, or straining to bring up phelgm, was responsible for the rejection of food, and also probable that the mere taking of the food provoked the cough. On February 6, 1915, that is nine days after admission, it was considered desirable that labour should be artificially induced, and this was carried out, the child being alive and weighing 77½ oz. The effect upon the vomiting was nil. The bronchitis rapidly got less under treatment with expectorants, &c. On February 22, other symptoms were suddenly added. The deafness increased, the patient became dizzy, her memory was more impaired, and she became somewhat drowsy and delirious. Her sight began to fail, so that though she could appreciate light, she was unable to recognize the faces of those about her. Ophthalmoscopic examination showed the presence of swelling of the disks and of retinal hæmorrhages. Nystagmus was present and marked: the knee-jerks could not be obtained. On February 25 an attempt was made to give relief by washing out the stomach. The stomach-tube passed in easily for a distance of 15 in., measured from the incisor teeth; but it was impossible to pass it beyond that distance. The passage of the tube caused the patient to retch, and actually to eject food she had swallowed; the return of the food was not through the tube, but outside it, and escaped by the mouth and nostrils. The patient's condition had become very grave, and it was considered desirable to transfer her to St. George's Hospital. This took place on February 27, 1915. When admitted she was not unconscious, but drowsy, she was wasted and pale, and had a little bronchitis; temperature 96° F., pulse 86, respiration 20. The pupils were found to be equal and they reacted to light; bilateral optic neuritis and flame-shaped retinal hæmorrhages were present. The knee-jerks

were diminished, and the calves were somewhat tender. A faint trace of albumin now appeared in the urine. The symptoms did not improve and the patient died on February 28, 1915.

We are indebted to Dr. R. Salusbury Trevor for the following post-mortem report: "There was well marked lordosis and old scars on the right side of the neck, the lungs were œdematous and collapsed at their bases; no indications of tuberculosis were seen, but there was much bronchitis. The heart weighed 6 oz., showing typical brown atrophy; the mitral valve was thickened, but competent; the aorta was small, but natural; there was no atheroma; small petechial hæmorrhages were found at the base of the right ventricle; the heart was not a 'renal' heart. Nothing abnormal was found in the abdomen; the right kidney weighed 4 oz., and looked healthy, except for slight injection of the pelvic blood-vessels; the left kidney weighed 2 oz., it was much furrowed as if by old infarcts; but on section they had not that character, the cortex being normal in colour and plentiful. Alimentary canal: The only point of interest was the condition of the œsophagus, which showed fusiform dilatation from the cricoid cartilage to the cardia, suggesting a second stomach. The length was 10 in., the maximum width 4½ in.; it contained about a pint of milk. The muscle was hypertrophied; the mucosa was ulcerated, small white epithelial islands being left in patches on an ulcerated surface. The stomach was small and catarrhal. The uterus was enlarged, and showed on the fundus and posterior walls a shaggy mass of placental tissue and clot; there was no endometritis. The brain showed no abnormality. Flame-shaped hæmorrhages and optic neuritis were demonstrated in the right fundus (confirmed microscopically). There was well marked lordosis of the spine. The right vagus was dissected out and appeared to be normal.

"*Microscopical Examination.*—This showed marked inflammation of the epithelial lining of the œsophagus, and there was small-celled infiltration between the muscle fibres. The circular muscular coat was much fibrosed, and in portions of the œsophageal wall which corresponded to dark rings, seen by naked eye on the mucosa, the blood-vessels were greatly dilated, even producing an angiomatous appearance. The furrows noticed in the left kidney corresponded to areas of fibrosis which appeared to originate around the blood vessels."

Case VI.—Mrs. R. T., aged 51, was admitted to University College Hospital on May 29, 1916, for pain after taking food, vomiting and wasting. She had been quite well up to four years before admission, when she began to have attacks of chest pain and vomiting. The attacks, when severe, lasted about two weeks, but at the beginning, each attack was followed by an interval of about two months, during which the patient was quite well. Gradually the attacks became much more frequent, and for the last five months, the interval between the attacks was only about three weeks. The pain was felt behind the middle of the chest and in the upper part of the stomach; it was dull in character and came on about twenty minutes after a meal, particularly, but

not exclusively, if the meal was a solid one. In another ten minutes, vomiting would occur and then the pain was relieved: the site of the pain was constant and it did not radiate in any direction. Vomiting was a constant sequel to the pain; the vomited matter varied in quantity, the severe attacks amounting to over one pint, the vomiting was easy and the vomited matter was free from blood. There was no flatulence. No history of any antecedent illness was obtained. On admission, the patient was found to be wasted (5 st. 8 lb. 11 oz.), and anæmic. She was somewhat tender on pressure in the epigastrium and the right rectus was somewhat rigid; on distending the stomach with carbon dioxide, it was found to be dropped, the brown margin reaching well below the umbilicus. She was given a fish diet on admission and seemed to be able to swallow it comfortably. Examination of the blood showed secondary anæmia; the examination of a test meal obtained on June 1, showed no free hydrochloric acid, although the total acidity was 0·219 grm. HCl per cent., consisting of protein hydrochloric acid 0·1095 per cent., and of lactic acid. Albumose was present and the digestive power was 30 per cent. (Dr. F. H. Teale). During her stay in the ward, the patient had irregular fever, reaching on occasions as much as 102° F. Gradually pain and vomiting recurred, pulse and respiration ratio increased in frequency, and death took place on June 30. A radioscopic study of a bismuth meal had been made, but so far as could be seen, there was nothing pointing to delay in the passage of food along the stomach or intestine.

Report by Mr. Lawrence. — "The œsophagus is 23·5 cm. in length. Its upper and cardiac orifices are of normal size; between these the tube is dilated throughout, its internal measurements being: At 2 cm. below cricoid, 6 cm. circumference; at 5 cm. below cricoid, 8 cm. circumference; at 11 cm. below cricoid, 7·5 cm. circumference; at 17 cm. below cricoid, 13·5 cm. circumference. Longitudinal rugæ are generally absent from the mucous membrane, except at the upper parts; in the middle third there are scattered superficial erosions of small size; in the lower third the mucous membrane is thickened and opaque and the submucous tissue indurated. At the lower end, just above the cardia, is an oval ulcer about 1 in. in length, which has part of its edge thickened, indurated and overhanging. The muscle of the upper half of the œsophagus is not appreciably hypertrophied; in the lower half it is distinctly but slightly hypertrophied, except at and for a short distance above the cardiac orifice, where it is normal. The lower tracheal and bronchial glands are much enlarged and caseous, and are adherent to the œsophagus in places. Just above the level of the aorta a softened gland has perforated the œsophagus by an aperture measuring 2 mm., and lower down, at about the middle of the œsophagus, is a larger ulcerated aperture (about 1 cm. in diameter)."

Remarks.

(1) It certainly looks as if this condition is not so very rare.

(2) The symptomatology is very variable. Dr. Martin's patient (Case VI) died comatose, and had symptoms of dyspnœa, which to a certain extent simulated asthma. The second patient also had shortness of breath, and at any rate at the beginning of his illness he had lost consciousness. Our third and fourth cases had some similitudes to cases of dyspepsia, one to ordinary acid flatulent dyspepsia, and the other to the dyspepsia of pulmonary tuberculosis. The fifth case simulated the toxic vomiting of pregnancy, and also had a history of attacks of cough and of asthma. The sixth case simulated carcinoma of the stomach.

(3) *Causation.*—Such cases in the past have been frequently referred to as examples of cardiospasm, by which is meant that quite independent of any organic lesion spasm occurs in the circular muscular fibres round the cardiac orifice of the stomach (cardiospasm). The obstruction caused by this phenomenon leads to dilatation of the œsophagus and to a variable amount of hypertrophy of the muscle of the tube, food being retained in the œsophagus, irritating its walls, leading to chronic inflammation, and assisting in bringing about dilatation. This might be looked upon as a sort of *positive* cardiospasm, but, unfortunately, there is no proof histologically that there is hypertrophy of the muscles of the cardia such as might be expected if spasm had occurred. Dr. H. D. Rolleston[1] has advanced the view that possibly the action of the longitudinal fibres is at fault, in that they do not help to dilate the cardia at the moment when the œsophagus is ready to propel food into the stomach, so that the circular fibres remain unopposed in action, producing, as it were, a *negative* cardiospasm. This view has been adopted very largely by Major Hurst, and he has introduced the term "achalasia of the cardia" to describe these cases, by which he means that when the peristaltic wave passing along the œsophagus reaches the cardia the relaxation which should occur then does not take place. He does not, however, commit himself to any explanation why this relaxation does not take place, other than the one suggested by Dr. Rolleston, who, as we have seen, hypothecates faulty action of the longitudinal muscular fibres.

Dr. F. Parkes Weber has put forward the ingenious suggestion that the neuro-muscular inco-ordination causing the faulty action of the

[1] *Trans. Path. Soc. Lond.*, 1896, xlvii, p. 37.

longitudinal muscular fibres was akin to heart block, and, if this were the case, it is easy to see that the block might not only be due to structural changes in the longitudinal muscle fibres, but to functional ones which would not reveal histological change.

Accepting Dr. Rolleston's view, it is easy to see that we need not necessarily in each case find that the post-mortem evidence of muscular hypertrophy is identical. Major Hurst asserts that in none of these cases is hypertrophy of the cardiac sphincter observed after death, but hypertrophy of the œsophagus as a whole was met with in our third case, except at the most dilated part, Mr. Lawrence finding hypertrophy in both muscle layers, and hypertrophy was seen in the fifth case by Dr. Salusbury Trevor. In our first case Mr. Lawrence found slight hypertrophy of the circular muscular fibres in the lower half of the œsophagus. From these facts and from accounts given by others it is clear that in some cases hypertrophy is the rule; in others atrophy is the more marked feature, whereas in others, again, there is hypertrophy of one part of the muscle layer and atrophy of another. So far as a consideration of the literature shows, the œsophagus in these cases in no way simulates the condition of the stomach wall in the condition met with in hypertrophy of the pylorus in children, where *all* the muscular coat of the stomach is hypertrophied, and that near the pylorus especially.

A second theory may be advanced for the explanation of this condition. The researches of Braune and of von Goubaroff have shown that a valvular mechanism may exist at the cardia, owing to the fact that when the œsophagus pierces the diaphragm it turns somewhat abruptly to the left side to open into the stomach, and it is probable that when the stomach is distended this bend is increased to such an extent as to obstruct the return of the contents of the stomach into the œsophagus. Obviously in such a condition there may be an obstruction to the passage of food from the œsophagus into the stomach. We think that this would provide an explanation for the occurrence of these cases, and we think that after childhood and early youth has passed, this valvular obstruction may begin to be operative to the extent of producing symptoms for the first time in the individual's life. This is in harmony with the well-known clinical fact that visceroptosis does not, as a rule, produce symptoms until early adult life is reached. In every one of the cases which we have recorded the symptoms were absent in childhood and youth, and first were noticed at the ages of 33, 37, 33, 21, 23, and 47. Another fact which supports this theory is that occasionally during

life the obstruction at the cardia *disappears*, the bismuth meal at one time meeting with obstruction, and at another time apparently passing on without any delay. This would explain why, in our second case, Mr. R. Higham Cooper found that the bismuth meal passed straight into the stomach, and yet at the post-mortem examination the classical condition of œsophagectasia was present. Further, in Case IV, Dr. Stanley Melville was able to prove by bismuth meals that there was obstruction on March 17 of this year, and that on April 7 it had quite passed away; the disappearance of the obstruction in this case is also confirmed by the fact that the patient can at the present time swallow solid food without difficulty, and has no symptoms whatever, although, as we have seen, she has persistent œsophagectasia. We feel, therefore, that there is a good deal to be said for this mechanical kinking at the cardia as an explanation of the development of the dilatation of the œsophagus in these cases, a theory which does away with the need to postulate "spasm" of circular or longitudinal muscular fibres, for which it is impossible to find an explanation.

(4) A few words remain to be said with regard to the symptomatology and other features of these cases.

(*a*) They must not be confused with cases of hysterical spasm of the œsophagus, in which neither food nor bougie can be passed. Hysterical manifestations were remarkable by their absence in our cases. It will be noted that five out of the six patients were women.

(*b*) That death may occur directly or indirectly about the age of 30, as in two of these cases, or may be postponed to later life, when death may occur from some quite independent cause, angina pectoris in our second case, at the age of 55, and at a still more advanced age from pneumonia in Sir Samuel Wilks's case.[1]

(*c*) The outstanding features which should attract the physician's attention to these cases are as follows:—

When food does not seem to pass down the œsophagus properly, it "sticks" behind the breast-bone, and may even cause pain in the epigastrium; *further, this has been noticed, frequently for a few or many years.* Relief is obtained by making the food come back, the patient securing this by tickling the fauces, attempting the act of vomiting, or by coughing, or by taking a deep breath. It will be found that the taking of a meal is followed by a feeling of difficulty of breathing, actually described by some patients as a feeling of stifling or of asthma,

[1] *Trans. Path. Soc. Lond.*, 1866, xvii, p. 138.

which is only relieved by again bringing up the food. In some cases a condition of persistent cough is provoked by taking a meal, as if the dilatation of the œsophagus irritated the lung, and caused a reflex cough, the combination of cough with attacks of respiratory embarrassment giving rise in these cases to the opinion that the patient is suffering from true bronchial asthma. When studying the transit of a bismuth meal by means of X-rays in cases of so-called "dyspepsia," it is very necessary to observe the shadow in the œsophagus as well as in the stomach and intestine; two at least of the above cases were missed from want of this precaution.

These patients soon learn that the best way to obviate symptoms is to masticate very thoroughly, and to spend some extra time over their meals, avoiding all "bolting." If this plan fails, they generally betake themselves to liquid diet, with which they can deal comfortably.

(*d*) We are entirely at a loss to explain why Dr. Martin's patient died unconscious, having been in that condition for something like forty hours; possibly it was due to acidosis. That it was not an exceptional feature is shown by the fact that our second patient lost consciousness at the onset of the malady, and our third patient was, when a young woman, liable to faint; this may, however, have been simply due to "anæmia," from which she suffered as a girl, for she has not had such a symptom during recent years. Our fifth patient also showed loss of consciousness; but there were other possible causes for it besides acidosis; there can be little doubt that her death was really due either to the toxic condition which produces pernicious vomiting in pregnancy, or it was due to uræmia, or to both.

(*e*) In conclusion, we would call attention to the extreme ease, in the absence of a careful history, of confusing these cases with dyspepsia, and even with bronchitis and asthma, for in none of them, except the fourth, did the patient volunteer the information which led us to suspect the condition which we have described.

We do not feel any hesitation in using the term "Œsophagectasia" for these cases. The name describes what is an obvious fact, and does not commit us to any of the various theories which have been advanced and as yet not proved.

DISCUSSION.

Professor S. G. SHATTOCK: None would deny that dilatation of the œsophagus *may* result from a cardiac kink, but such a condition would, speaking pathologically, have little interest—not more than the hydronephrosis that results from a kink of the ureter. The condition in question, however, has a pathogenesis of its own; it is idiopathic. Certain things can be, I think, excluded in its causation, and its ætiology thus indicated by way of exclusion.

(1) That it is not due to vagal palsy is clear from the fact that the muscular wall of the dilated part is not atrophic nor degenerate; it maintains its relative thickness like that of the intestine above the seat of a chronic obstruction.

(2) If the condition of the cardia be compared with that of the pyloric canal in "hypertrophic stenosis," a notable difference at once appears in the thickening of the circular muscular wall of the pyloric canal, which is as invariably found as it is invariably absent at the cardia in cases of dilated œsophagus. The overgrowth of the musculature of the pyloric canal is best explained, perhaps, by regarding it as due to spasm, possibly brought about by a hyperæsthetic state of the mucosa at this spot, whereby it responds abnormally to the mechanical stimulus of the passage of the gastric contents. This difference is one reason for not viewing dilatation of the œsophagus as due to cardiac spasm or hypertonic contraction.

(3) With respect to the theory of kink, the evidence would require that the parts should be displayed *in situ* after death; this has not yet been done. Moreover, a kink of many years' duration would presumably become permanent by a process of "adaptive atrophy"—the process whereby any part becomes organically shortened on the side of relaxation, if kept for any length of time in a distorted position. In the Museum of St. Thomas's Hospital there is an excellent example of dilated œsophagus which is instructive since it is not so widely distended as is the case in Dr. Batty Shaw's specimens. In this the cardiac portion of the stomach is preserved: there is no deviation whatever from the direct line. The parts came from a patient who had suffered for thirteen years from œsophageal obstruction, and who ultimately died of inanition.

(4) As regards a "block" theory, I had considered this possibility in a paper published a few years ago on "Idiopathic Dilatation" of the urinary bladder, and I then came to the conclusion that it did not admit of being applied. If the intestine is divided transversely the nerve impulse ceases to travel across the gap; here there is a "block." But nothing of this kind results under similar circumstances in the œsophagus; the impulse is transmitted across the interval by means of the vagus, and, in the case of the dilated œsophagus, no organic lesion of the vagus such as would result in a block has been demonstrated.

One is thus led to the hypothesis that the dilatation is due to an inco-

ordination of the nervous impulses transmitted by the vagus during deglutition, which impulses should normally cause contraction of the tube and an active dilatation of the cardia; if the cardia dilates at all, the dilatation does not synchronize with the contraction of the tube above, and it remains closed, so far as practical purposes go, by the normal action of the sympathetic. If this is so, one still requires to know why the inco-ordination fails; and whether, in some cases, there may not be a fault on the side of the mucosa, which fails to furnish a proper afferent stimulus.

Finally whether, as an extreme measure, it would be justifiable to excise the cardia and make an end-to-end junction between the œsophagus and stomach, may be a suggestion worth consideration.

Major A. F. HURST, R.A.M.C.: I do not like the term "œsophagectasia" proposed by Dr. Batty Shaw and Dr. Woo, as the dilatation is secondary and not primary. In one of my cases, which I had the good fortune to see as early as the second day on which obstruction occurred, dilatation had not yet had time to develop. As Dr. Batty Shaw has apparently only read the remarks I made when showing a case before the Clinical Section in May, 1914, I should like to explain more fully my views, which were published in the *Quarterly Journal of Medicine*, 1915, viii, p. 300. In May, 1914, I had not seen the paper published in 1896 by Dr. H. D. Rolleston, in which he expressed views somewhat similar to mine, although I believe that the circular muscle fibres at the cardia are alone concerned, and that there is no evidence of any abnormality in the behaviour of the longitudinal fibres. Every time a peristaltic wave passing down a muscular tube reaches a sphincter, the sphincter relaxes in order to allow the contents of the tube to proceed. If for any reason this normal relaxation is absent, the contents of the tube accumulate behind the sphincter. This absence of the normal relaxation is the condition which I have called achalasia; its occurrence at the pylorus, ileo-cæcal sphincter, pelvi-rectal flexure and anus accounts for a number of pathological conditions. Achalasia of the cardia causes food to accumulate in the œsophagus, which gradually becomes dilated; it is not surprising that the efforts made by its muscular coat to overcome the obstruction offered by the closed cardia lead in time to hypertrophy, when the extremely powerful character of the peristaltic waves is seen with the X-rays. The stagnation of food in the œsophagus leads to catarrh of the mucous membrane, so that the dilatation, muscular hypertrophy, and changes in the mucous membrane are all secondary to the achalasia of the cardia. The cardiospasm theory is untenable because of the absence of hypertrophy of the cardia, which would certainly develop in course of time, as the condition may persist for as many as twenty years; moreover, a soft rubber tube, closed at its lower end and filled with mercury, drops into the stomach without meeting any resistance at the cardia, such as would occur if spasm were present, and it can be readily withdrawn without being gripped in the way the finger is gripped when withdrawn from the rectum in cases of anal spasm, or the mercury tube is gripped when true œsophageal spasm is present,

as a result, for example, of ulceration. With regard to Dr. Batty Shaw's theory of a kink at the cardia, no anatomical or other evidence has been brought forward in its support. The inconstancy of the obstruction in the early stages is much more in accordance with some upset in the neuromuscular mechanism of deglutition than with an anatomical peculiarity which, if present once, would be present always. Moreover, the obstruction manifests itself already when the first mouthful of a meal is swallowed, whereas, according to the kink theory, it should not occur until the stomach is distended. I agree with Dr. Batty Shaw that the condition is not uncommon, as I saw as many as five cases in private and two at Guy's Hospital between 1911 and 1915. Achalasia is, I believe, also the cause of the comparatively common condition known as water-brash, in which faintly alkaline watery fluid, consisting of saliva, sometimes mixed with œsophageal secretion, is brought up. It occurs most commonly in conditions like duodenal ulcer, which are associated with hyperacidity; at the end of a meal, when the acidity of the stomach has reached its maximum, excessive secretion of saliva is reflexly called forth; the saliva is swallowed, but accumulates in the œsophagus, as the cardia remains closed owing to the presence of excess of acid in the stomach. It is impossible to explain in any other way how alkaline fluid, unmixed with food, can be brought up in large quantities when the stomach is full of food mixed with acid juice, which may subsequently be vomited, as the alkaline and acid fluids cannot possibly come from the same viscus. Finally, I should like to add a few words about treatment, as I believe that the condition can always be relieved without the aid of any operation. In five of my patients the mercury-tube was used, and led to more or less complete recovery, though it is still occasionally passed by some of them. In the very early case already referred to permanent recovery followed the passage of an ordinary bougie on a single occasion, and in the remaining case, one of twenty years' standing, the dilatation was so great that the end of the mercury tube was seen with the X-rays to coil round in the pouch formed by the lower end of the greatly dilated œsophagus so that it never reached the cardia, which was on a slightly higher level; I believe that improvement subsequently followed dilatation with an instrument passed over string, which had been swallowed by the patient.

Dr. A. W. Woo: The treatment of this condition (œsophagectasia) has interested me very much. In looking over the reports of cases by writers on the subject, I find that considerable success has attended their methods. Lockwood, for instance, in 1906 described a case in which the treatment consisted in dilating the cardia by means of an instrument which consisted of an ordinary œsophagus tube with a distensible rubber bag around the last 4 in. or 5 in. of the stomach end.[1] This proved quite successful. In the *Annals of Surgery*, 1906,[2] Sippy, of Chicago, reported several cases in which the symptoms were completely removed by dilating the cardia. In the

[1] *Brit. Med. Journ.*, 1906, i, p. 1367.

[2] *Ann. Surg.*, 1906, xliii, p. 858.

treatment of these cases the instrument used by him consisted of a rubber bag 3½ in. long, enclosed in a silk bag, which limited the distension of the rubber bag to 15 cm. circumference. When in a collapsed condition the diameter of this apparatus was half that of an ordinary stomach tube, the apparatus was introduced and held in position by a bougie, then inflated: one or two applications were found to be sufficient and no anæsthetic was required. Another method of treatment by dilatation was that adopted by Major Hurst, who used rubber tubes filled with mercury without the bag. He met with considerable success in a number of cases. Some writers have thought that resting the œsophagus would have the desired result. Gastrostomy was accordingly performed in a few instances; the results obtained, however, were not satisfactory. On carefully going into the symptoms of these cases I find I am able to classify them as follows: (1) Those of slight degree of obstruction not warranting operative measures; (2) Those of severer nature, but admitting a small-sized bougie; (3) Those in which the obstruction is sufficient to prevent the passage of an instrument. As regards the treatment I have formed the opinion that, in the first class of case, all that is required is careful dieting and attention to general health. I think the success of this method of treatment in these slighter cases has been demonstrated in many instances. In the second class of case I think systematic and careful dilatation should prove successful. In the third class of case I do not hesitate to suggest that such cases should be treated first by gastrostomy, then by dilatation with more suitable apparatus.

Mr. HERBERT TILLEY: In spite of the forcible arguments which have been advanced by previous speakers against the "spasmodic" theory, I still feel reluctant to give it up entirely, because I have been so impressed with the degree of spasm which the œsophagus is capable of exerting. For example, if a small foreign body lodges in the upper third of the gullet, it may produce such a degree of spasm that neither fluid nor solid can be swallowed. A similar result may be produced by a small superficial ulcer at the lower end of that viscus. Then again, we have those cases of phreno-spasm (so-called cardio-spasm) of functional origin with dilatation of the œsophagus in which the attacks of dysphagia may be cured by one, two or more divulsions of the hiatal or phrenic portion of the œsophagus. Lastly, there is the argument of practical experience in the use of the œsophagoscope. In the majority of these cases of œsophageal dilatation, when the distal end of the tube reaches the hiatal constriction of the gullet, this is seen to be closed, but it gives way to a little gentle pressure and the tube enters the œsophageal portion of the cardiac portion of the stomach. The resistance is practically absent under general anæsthesia, but it is very definite when œsophagoscopy is practised without any anæsthetic—local or general. I submit that spasm appears to be an explanation of these facts. And this leads one to another point of practical importance, for I wish to protest against any inference being drawn from the blind passage of bougies in any affection of the gullet. In any case of difficulty of swallowing which cannot be explained by the laryngoscope,

the œsophagoscope should be passed and every inch of that viscus should be inspected by the naked eye. It is the only way in which we can determine, once and for all, the nature of the lesion which is present. Such a method of examination would only be contra-indicated in the presence of an aneurysm or an extremely feeble condition of the patient from other causes. Radiography must go hand in hand with œsophagoscopy, but the physician who relies on radiography or fluoroscopy to the exclusion of œsophagoscopy will not rarely make an altogether wrong diagnosis. With regard to treatment, the only method of curing the dysphagia is by mechanical divulsion of the hiatal portion of the œsophagus, and instruments are now made by means of which this can be done under the guidance of direct vision through the œsophagoscope. I purposely speak of curing the symptom rather than the lesion, because once a pathological dilatation of the gullet has been produced it always remains. In conclusion, and for the consideration of those who have this afternoon advanced different theories as to the causation of "œsophagectasia," I would like to say that a case has been recorded in an infant, aged two days, by Dr. Chevalier Jackson, Pittsburg, U.S.A. It was cured by the passage of a small œsophagoscope (*vide* "Peroral Endoscopy," p. 507).

Dr. C. R. Box: The skiagram which I show is from a patient with œsophageal dilatation at present under my care. It was taken with the mercury tube *in situ* and confirms the advisability of passing the tube, for the first time at all events, under the guidance of the fluorescent screen. The sharply defined shadow of the tube is seen to deviate rather abruptly to the right of the heart shadow and on approaching the lower limit of this to turn almost at a right angle and lie parallel with the surface of the diaphragm. The tube has not penetrated the cardia at all and there is no doubt that previous passages were equally futile. The œsophagus in this case is evidently elongated to a considerable degree as indicated by its flexuous course. The distance the tube passes may be no index to the position of its lower end which, in this patient, impinges on a small œsophageal cul-de-sac to the left of the cardia.

Dr. F. Parkes Weber: In regard to the question of "water-brash" or "pyrosis," it may be noted that some persons suffer from a recurrent œsophageal "pseudo-vomiting" of saliva, which was mixed with blood in the kind of cases (especially women) described in 1893, by Nové-Josserand, under the term, "hæmosialemesis" (vomiting of saliva and blood).[1] Functional obstruction at

[1] Cf. A. Mathieu, "Pseudo-vomiting," *Med. Press*, Lond., 1912, cxliv, p. 483. In young men with gastroptosis copious salivation and "œsophageal pseudo-vomiting" of saliva, without any heartburn, may sometimes be induced by violent exercise (for instance, a game of football) after a considerable midday meal. These disagreeable symptoms which I refer to subside when the recumbent position is assumed, and therefore I suppose that they are the expression of a reflex disturbance due to the "dragging" of a full stomach in cases of enteroptosis. Cf. F. Parkes Weber, "Reflex Salivation from Abdominal Disorders," *Edin. Med. Journ.*, 1900, N.S., vii, p. 348.

the cardiac end of the stomach of the kind under our to-day's consideration may sometimes be a very grave disease indeed—a disease, in fact, which sooner or later may lead to death by inanition. Such were the deaths formerly attributed to "spasmodic stricture of the œsophagus," when the patients during life were supposed to be suffering from cancerous stricture of the œsophagus, though at the necropsy no cancer nor cicatricial stricture could be discovered. In very bad cases, I think, an attempt at some radical operative method of treatment, as suggested by Mr. Shattock, would be justified. But perhaps the surgeon might be able to enlarge the channel by a plastic operation on the cardia, analogous to the operation of "pyloroplasty" on the pylorus—that is to say, he might be able to make a longitudinal incision through the cardia, then open out the wound, and sew it up transversely.

Dr. DUNDAS GRANT: I associate myself entirely with Mr. Tilley in his protest against the blind use of œsophageal bougies in cases of possible dilatation of the œsophagus due to cardio-spasm, and in his plea for the use of the œsophagoscope. The X-ray examination is of primary importance in diagnosis, and under the guidance of the X-rays I was able in Dr. Batty Shaw's case to pass a bougie through the constriction. According to subsequent reports, the patient's swallowing was considerably improved. Guisez, of Paris, and Starck, of Heidelberg, both describe appearances seen by the œsophagoscope which they consider characteristic of cardio-spasm as distinguished from other forms of mechanical closure of the cardiac—or, perhaps, more properly the diaphragmatic—narrowing, namely, symmetrical cushiony swellings on each side of or around the orifice. The chief point in diagnosis is no doubt the exclusion of malignant disease, and, along with the history, these œsophageal appearances may assist in the diagnosis. I am particularly interested in Major Hurst's remarks about water-brash, which I have always considered to be caused by an accumulation of pharyngeal secretions above a constriction in the œsophagus. When I was a practitioner of general medicine, I saw a considerable number of cases of this condition which failed to answer to other treatment, but in which I was able to give extraordinary relief by the administration of soda and belladonna, and in one case the patient allowed me to pass an ordinary soft rubber stomach-tube, which also seemed to afford relief. Probably this condition is due to mild intermittent cardio-spasm.

Dr. BATTY SHAW (reply): In reply to the criticism that the term "œsophagectasia" is a very cumbersome one, I think the criticism is a little unfair. In the first case it is little more cumbersome than the word "œsophagoscopy," which has been introduced to describe the operation of looking down the œsophagoscope; and, secondly, I think it is in harmony with the term "gastrectasia," used to describe a kindred condition occurring in the stomach, and restricted by some to describe the condition in which the stomach is permanently dilated; and yet there is no explanation, or there are only purely theoretical explanations, to account for the condition. I cannot but think that after the discussion on this paper, it will be obvious to those who have no theories to advance that the true explanation is not yet forthcoming. Mr. Shattock's

cogent argument in favour of the use of an alternate expression, "idiopathic dilatation of the œsophagus," to describe these cases is to my mind unanswerable; but I still think that for mere brevity's sake "œsophagectasia" is a reasonable alternative. Now, turning to Major Arthur Hurst's explanation of the cause, I feel that the arguments that he put forward to show that the explanation of this condition is that, for some reason or another, the physiological relaxation of the cardia fails to take place when a bolus of food is propelled down the œsophagus by its peristaltic action, may also be used as a proof that the obstruction is due to an anatomical kink at the cardia. This is notably the case with regard to the fact that when a bougie is passed through the obstruction, it is not grasped as by an active spasm, but is withdrawn quite easily. I regret very much, however, that we have not referred in our paper to Major Hurst's further contribution on this subject in the *Quarterly Journal of Medicine*.[1] Even after perusal of this paper, however, I think the reader will feel that the root difficulty in accepting his explanation is that he gives no explanation why the cardia does not relax. Our explanation does at least find a cause, an anatomical one; but we admit that we have not substantiated the possibility by anatomical demonstration. I cannot follow Mr. Shattock's argument that if this kink existed, evidence would be found at a post-mortem—at any rate, in the way in which post-mortems have been hitherto carried out. Nevertheless, it is a fair criticism that the burden of the actual proof of the existence of this kink rests with us, and we must endeavour in the future, now that these cases are shown to be capable of diagnosis during life, to carry out an adequate necropsy to prove our point. Mr. Shattock's suggestion that there is a "peccant" spot at the cardia which is hypersensitive, and is the cause of the trouble, also needs anatomical substantiation of a most thorough character before we could ask a surgeon to excise the area for the purposes of a cure. The discussion has turned on the pathogenesis of this condition; but there is a point in our paper which has unfortunately missed criticism, and that is, that in two cases the analysis of the test meals showed the presence of combined protein hydrochloric acid. We have discussed this point with Dr. F. H. Teale, and he stands by his observations, and gives as an explanation that the tube used to abstract the meal really did enter the stomach. But we have heard this evening how difficult it is in these cases to pass a tube or bougie through the cardia, and I have seen this same difficulty even when it was attempted to be passed by means of an œsophagoscope. Dr. Box has shown us, by means of an X-ray picture, how easy it is to conclude wrongly that an instrument has passed successfully, judging by the length of the instrument used; for his picture shows that it simply curled round, and did not enter the stomach. This difficulty merely shows a further reason for reading our paper. We have already seen that an X-ray examination in two cases, when not properly carried out, fails to detect these cases; and now we see that the chemical analysis of a test meal in two cases may give the impression that we are dealing with stomach disorder, when all the time the trouble is œsophageal.

[1] *Quart. Journ. Med.*, 1915, viii, p. 300.

(*November* 28, 1916.)

Acquired Syphilis of the Lungs.

By F. Parkes Weber, M.D.

Case I.—The patient, J. S., aged 59, a seaman, was admitted to hospital on January 3, 1916, suffering from bronchitic and asthma-like symptoms, with frequent moderate pyrexia (up to about 100° F. in the evenings), suggesting pulmonary tuberculosis. There were signs of emphysema of the lungs and some inspiratory crepitation at the base of the left lung, but there were no signs of localized tuberculous lesions anywhere, and the mucopurulent sputum was repeatedly examined for tubercle bacilli, always with negative result. Röntgen skiagrams of the lungs showed (Dr. James Metcalfe) enlargement of the hilus glands. A point that attracted my attention was that the patient presented very marked and typical so-called "syphilitic leucodermia" of the neck, and on examination it was found that his blood serum (Dr. H. Schmidt) gave a positive Wassermann reaction for syphilis. An expectorant mixture was prescribed containing $7\frac{1}{2}$ gr. of potassium iodide (three times daily) and this was taken from January 6 to August 24; and on five occasions small doses of salvarsan were injected intravenously. When he left the hospital on September 29, 1916, though his bronchitic symptoms were not cured, he seemed on the whole better, and he had gained $3\frac{1}{2}$ kilograms in body weight.

I regard the case as one of chronic tertiary syphilitic disease of the lungs, with peribronchitic fibrotic changes. The syphilitic leucodermia was of some diagnostic importance, and it is interesting that this really valuable sign of previous syphilis is supposed to occur relatively rarely in male patients.[1]

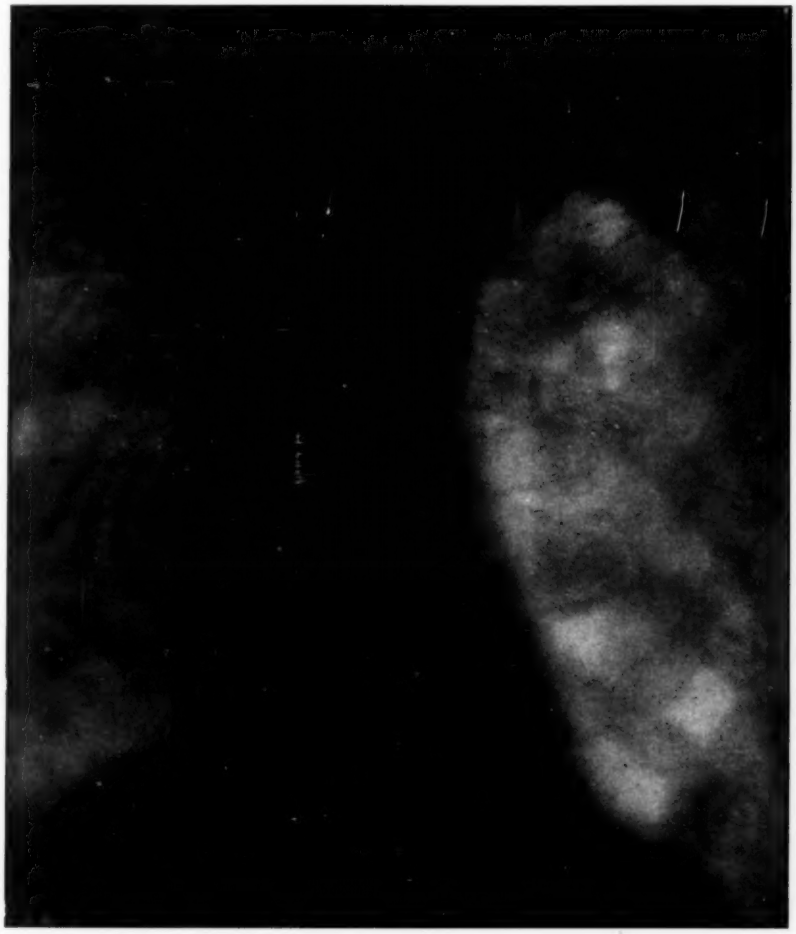
Case II.—K. N., aged 56, a waiter, admitted to hospital on February 29, 1916, for signs suggesting fairly advanced pulmonary tuberculosis. There was much nummular mucopurulent sputum, and on auscultation of the thorax crepitation was heard over the left front and over the upper parts of both lungs. The left lung was most involved, and on another occasion I noted a good deal of crepitation over the back of that lung, especially in the interscapular region. There was not much dullness to percussion. I thought that

[1] Cf. G. Pernet, *Proc. Roy. Soc. Med.*, 1911, iv, *Sect. Derm.*, p. 103. Cf. also the list of cases given by A. Brandweiner in his monograph, "Leucoderma Syphiliticum." Leipzig and Vienna, 1907.

the case was one of advanced pulmonary tuberculosis; but there was practically never any pyrexia; repeated examination of the sputum failed to reveal the presence of tubercle bacilli; and the patient's blood serum (Dr. H. Schmidt) gave a positive Wassermann reaction for syphilis. Moreover, there was abductor paresis of the left vocal cord, which is a rarer complication of intrathoracic tuberculosis than of malignant disease or syphilis. The patient said that he had suffered from pleurisy fifteen years ago and had lately had copious expectoration. He was subject to occasional epileptic attacks. There were no signs of any disease in the abdominal viscera. Under treatment in the hospital by iodide of potassium (15 gr. three times daily) from admission (in February) to April 4, and afterwards with cod liver oil and extract of malt, the patient's health improved greatly. When he left the hospital on June 26 he had gained 4 kg. in body-weight, but there was still much mucopurulent expectoration. I have no doubt that the case was one of tertiary syphilitic disease of the lungs and of the mediastinal lymphatic glands, the latter giving rise also to paralysis of the left recurrent laryngeal nerve. There was probably also a good deal of bronchiectatic dilatation secondary to the chronic syphilitic disease.

The paralysis of the left recurrent laryngeal nerve in this case was certainly not due to aneurysm, but the appearance in a Röntgen skiagram of the thorax, taken early in March, 1916 (*see* figure), was, according to Dr. James Metcalfe, somewhat suggestive of a mediastinal growth. He reported that in the great vessel area above the heart there was a dense shadow, which was nearly as wide as the whole cardiac shadow, and that there was considerable irregular opacity in the upper part of both lungs, but especially the right lung. A skiagram taken later on (May, 1916) unfortunately did not show the details very distinctly, and the two skiagrams could therefore not be compared in regard to the question of improvement.

Case III.—R. M., aged 42, a seaman, was admitted to hospital on March 9, 1916, complaining that during the last eighteen months he had been subject to pains in the lower left region of the thorax and spasmodic attacks of coughing. Some of the attacks of coughing were accompanied by much mucopurulent expectoration, and occasionally sputa streaked with blood were coughed up. There was a history of syphilis twenty-four years ago, and the Wassermann reaction (Dr. H. Schmidt, March, 1916) was found to be weakly positive. In the hospital I noted crepitations and creaky dryish sounds localized to the area of the lower left side of the thorax external to the heart. No other signs of disease were discovered. The brachial systolic blood-pressure was 120 mm. Hg. The urine was free from sugar and albumin. There was no fever. The sputum was frequently examined for tubercle bacilli, always with negative result. An expectorant mixture was ordered.



Röntgen skiagram of thorax from Case II (March, 1916).

Potassium iodide was tried but was discontinued owing to its producing conjunctival irritation and swelling in the fauces and face.[1] Six small intravenous injections of salvarsan were given, at intervals of two weeks or so. About the middle of May by ordinary examination of the lungs I could find no abnormal physical signs, excepting a few inspiratory crepitations after coughing, about the lower angle of the left scapula; but a Röntgen skiagram showed (Dr. J. Metcalfe) much hilus lymphatic gland enlargement and peribronchial thickening. When patient left the hospital on June 13, 1916, nothing wrong could be found with him by ordinary examination. On November 2, 1916, he was, however, readmitted with his old trouble, violent paroxysms of coughing accompanied by mucopurulent expectoration. There was slight pyrexia (up to about 100° F.) on the first two days following his readmission. By ordinary examination I could find nothing abnormal excepting some dry bronchitic and "squeaky" sounds at the base (behind and at the side) of the left lung. A Röntgen skiagram showed (Dr. J. Metcalfe) "increased shadowing in the great vessel area and much bronchial gland enlargement at the hilus on both sides." No tubercle bacilli could be found in the sputum.

The disease in this case seems to be chronic syphilitic peribronchitis, chiefly localized to the base of the left lung, perhaps accompanied by some bronchiectatic dilatations and local pleuritic adhesion.

Case IV.—P. L., aged 55, a strongly built man, was admitted to hospital on August 15, 1915, with moderate fever and signs which I supposed to indicate pneumonia in the lower part of the right lung. (Pulse on admission 94, and respiration 36, per minute.) The patient said that he had had "rheumatism" in February, 1915, and that the present illness commenced with shivering and vomiting on August 12. In the hospital the fever gradually disappeared by August 21. There was another period of fever from August 26 to 31 inclusive, and there was again a little fever about September 5, but none afterwards. On August 28 some tender swollen lymphatic glands were felt in the right axilla. (It should be noted in this connexion that the patient had a scar from a recently healed burn on the back of the middle finger of the right hand.) Later on the tenderness was less, but the swelling increased and the glands were enlarged, not only in the right axilla, but above and below the right clavicle. On September 3, there was still a little impairment of resonance in the right infra-scapular region, and a slight murmur was heard accompanying the first sound at the cardiac apex. The abdominal viscera were apparently free from disease, and no albumin nor sugar were present in the urine. The brachial systolic blood-pressure (August 18, 1915), was 135 mm. Hg. A blood count (September 4) showed very slight excess of red

[1] With this idiosyncrasy towards iodides may be compared the swelling of the parotid or submaxillary salivary glands (or of both parotid and submaxillary glands), and the thyroidal disturbance, which occasionally constitute unusual (and sometimes misleading) manifestations of iodism.

cells. The blood serum (Dr. H. Schmidt, August 31) gave a positive Wassermann reaction for syphilis (and so did his wife's blood serum, when she was under hospital treatment in April, 1916). He admitted having had a chancre about eighteen years ago. On September 3, treatment with potassium iodide (15 gr. three times daily), and Zittmann's decoction was ordered, and on September 29, a course of mercurial inunction was started. The glandular swelling disappeared by about the middle of October.

I think that in this case the pulmonary trouble was possibly, but not certainly, syphilitic. He was afterwards under treatment for frontal sinus disease, and except for that and some "rheumatic" pain in one shoulder, he seemed quite well. But the Wassermann reaction was still positive in November, 1915, not long before he left the hospital.

Case V.—B. S., aged 46, a fairly well-nourished married woman, was admitted to hospital many years ago,[1] with pulmonary symptoms. About a year previously she had been treated in the out-patient department for secondary syphilis. For about four weeks before admission she had been suffering from a dry cough, worse at night, with hardly any expectoration. No history of any previous thoracic disease was obtained. On admission there was a localized patch of bronchial breathing in the left lower axillary region, over which dullness to percussion was noted. There was also some impairment of resonance over the greater part of the infra-scapular region on the same side. Pulse, 64 per minute, regular. There was no fever. A localized gummatous affection of the pleura and lung, with a little pleuritic effusion, might have accounted for the symptoms. The patient was treated with potassium iodide and given full diet. In a few days the bronchial breathing could no longer be detected, but some crepitation or scraping sound could be heard in its place. The cough soon ceased. The patient left the hospital before the abnormal signs had quite disappeared. The urine, when tested, contained no albumin and no sugar. The temperature was never found to be above 99° F.

Case VI.—J. B., aged 30, a married woman, was admitted to hospital on August 2, 1913, with diffuse bronchitic signs and moderate pyrexia. The history was that for the last six months she had suffered from symptoms of bronchitis, accompanied by occasional asthma-like breathing. Eight or nine months ago she had suffered from an ulcer on the forehead, which had left a depressed scar, attached to the bone. About the same time her hard palate had become perforated, and the perforation still persisted. About twelve months ago there had also been considerable ulceration about the right ankle. She had had a miscarriage soon after her marriage (which was six years ago), and had been deserted by her husband not long afterwards. In the hospital her cough was very "brassy" and there was apparently some tracheal ulceration. Röntgen ray examination of the chest showed thickening at the hilus of both lungs, and

[1] Cf. F. Parkes Weber, *St. Bart.'s Hosp. Reports*, 1898, xxxiv, p. 305.

what seemed to be enlarged bronchial glands. The blood serum gave a positive Wassermann reaction for syphilis. No tubercle bacilli were found in the sputum (but this was, unfortunately, insufficiently examined). Expectorants and antisyphilitic treatment were tried. There was very little fever after the first six days in the hospital. About the middle of October, however, the patient became drowsy, as if from chronic carbonic acid poisoning. She seemed to lose the power of expectorating. Death occurred on October 18, 1913, and no post-mortem examination was permitted.

Remarks.

Whilst I was physician to the Mount Vernon Hospital for Diseases of the Chest I practically never saw a case which I could regard as one of pulmonary syphilis, but recently I have met with several in which I believe that that diagnosis can be made with more or less probability. Many writers on the subject have had the opportunity of making post-mortem examinations. The *chief pathological-anatomical features* are: (1) gummatous formation, with tendency to necrotic changes; and (2) the more chronic, and often widely-spread, fibrotic changes, tending to become associated with bronchiectatic dilatations. In the above-described cases I think the changes were chiefly of the fibrotic order, though gummatous formation may likewise have occurred, for instance in Case V, and perhaps in Case VI. According to T. Tanaka,[1] the histology of the pulmonary indurative changes of acquired syphilis is very similar to that of the so-called "white pneumonia" of congenital syphilis. The most characteristic features are much endarteritis obliterans, together with periarteritis and endo-phlebitis. Mesarteritis likewise occurs, and in three cases Tanaka found a noteworthy increase of the unstriped muscle tissue, which he regarded as derived from the walls of the small bronchi, and which had been till then never described as connected with pulmonary syphilis.

In regard to *clinical diagnosis* the question of there being an aortic aneurysm or mediastinal new growth may arise, as it did in Case II, especially if there is paralysis of the left recurrent laryngeal nerve. But the clinical differentiation from tuberculosis is the chief difficulty, for in tertiary syphilitic diseases of the lungs there may be pyrexia, that is to say, a so-called "tertiary syphilitic fever," such as not very rarely occurs in tertiary syphilitic disease of the liver—or else a complicating

[1] T. Tanaka, "Beitrag zur Kenntniss der Lungensyphilis beim Erwachsenen, zugleich über sogenannte Muskuläre Lungencirrhose," *Virchow's Archiv*, Berl., 1912, ccviii, p. 429.

fever, due to associated bronchitic and bronchiectatic changes with purulent discharge. Moreover, not only may there be pyrexia, but there may also be (though less in frequency and degree than in tuberculosis) night-sweats, emaciation, hæmoptysis,[1] and copious nummular muco-purulent sputum. In pulmonary syphilis, however, the lower parts of the lungs are generally chiefly involved, and the absence of tubercle bacilli from the sputum (on frequently repeated examinations), together with a history of past syphilis, evidence of tertiary syphilitic lesions elsewhere, and a positive Wassermann reaction (when obtained), ought to turn one's attention in the right direction. The presence of typical syphilitic leucodermia in Case I was, as I have already pointed out, of some diagnostic value.

In regard to pulmonary syphilis, one has to bear in mind the possible co-existence of syphilitic ulcers or stenoses in the bronchial tubes, trachea or larynx. Stengel (1903) pointed out that one of the most suspicious symptoms is intense dyspnœa of more or less spasmodic type, with a tendency to stridor and cyanosis, all of these being indicative of obstruction in the larger air passages. In some cases the respiratory symptoms are altogether much more severe than the ordinary physical signs in the lungs would lead one to suppose. There may also be, as in my Case III, recurrent severe spasmodic attacks of coughing, accompanied by more or less copious mucopurulent expectoration.

In some cases pleurisy and pleuritic effusion may be associated with pulmonary (gummatous) syphilis, as they probably were in my Case V.

The therapeutic test is, needless to say, always of extreme importance, especially when the patient is found to derive obvious benefit from the use of potassium iodide, a drug which sometimes aggravates the symptoms in pulmonary tuberculosis. Old cicatricial stenoses of the respiratory passages cannot of course be removed by antisyphilitic treatment.

It must always be remembered that pulmonary tuberculosis is not at all rare in syphilitic subjects, and vice versa, that tuberculous patients not very rarely acquire syphilis. I do not think that the two diseases have much effect (either good or bad), one on the course of the other, unless one or both of them have been sufficiently severe to produce a condition of cachexia and generally to lower the resistance of the body towards disease. Pulmonary tuberculosis, as far as I can judge, is

[1] In some cases of hæmoptysis many spirochætes have, I believe, been found in the sputum, though doubtless not the *Spirochæta pallida* of syphilis.

uninfluenced by the presence of an old quiescent syphilitic taint. Though by some an antagonism between the two diseases has been supposed to exist, it is more probable that syphilis predisposes to tuberculosis, in so far as it lowers the vitality of the organism; and it is not unlikely that syphilitic tissues (as has been maintained[1]) constitute a specially favourable soil for the growth and multiplication (within the body) of tubercle bacilli. When the tuberculosis is active and the syphilis quiescent or obsolete, the former naturally dominates the whole clinical aspect and the prognosis, and the effect of energetic antisyphilitic treatment by mercury and potassium iodide is not unlikely to be a bad one. Though salvarsan and neosalvarsan, in moderate doses, seem to do no harm in ordinary cases of pulmonary tuberculosis, yet they do no good so far as I know, although this is contrary to opinions which I think have been expressed by some writers.[2]

[1] Cf. R. W. Taylor, "Bone Syphilis, Hereditary and Acquired," *New York Med. Journ.*, 1907, lxxxv, p. 7. Cf. also, on the whole subject, Émile Sergent, "Syphilis et Tuberculose," Par., 1907. Gouget also (*Journ des practiciens*, Par., 1910, xxiv, p. 771) maintains that while tuberculosis does not predispose to syphilis, the latter certainly does to tuberculosis.

[2] For a discussion of this subject see, however, N. B. Potter, "Salvarsan in the Treatment of Double Infections, Tuberculosis and Syphilis," *Amer. Journ. Med. Sci.*, Philad., 1916, cliii, pp. 823-845.

Section of Medicine.

President—A. E. Garrod, M.D., F.R.S., Colonel A.M.S.

(*June* 12, 1917.)

(Chairman—The President of the Society (Sir Rickman Godlee, Bt., K.C.V.O., M.S.)

Tropical Diseases in the Balkans.

By Aldo Castellani, M.D., M.R.C.P., Lieutenant-Colonel.

(*Royal Italian Medical Service, Navy, Balkanic Zone.*)

The present paper is based on the work I have done in Serbia, Macedonia and other parts of the Balkanic and Adriatic zones during the last two years. It is not my intention to go fully into every tropical disease to which the allied troops are exposed in those zones, but merely to touch on a few points and features of each malady which have especially attracted my attention.

My remarks, based on my own clinical experience, though of little or no scientific importance, may perhaps be useful to medical officers joining the Salonika Army, as certain clinical points to which I refer are hardly mentioned in text-books or are given in a way which at times does not correspond with what one actually sees in practice in the Balkans.

I use, of course, the term "tropical diseases" *sensu lato*, or very few maladies indeed would remain in the domain of the tropical specialist. As a matter of fact certain diseases which are often called tropical—as for instance "typhus exanthematicus" and "Malta fever" are of more frequent occurrence in temperate zones and sub-tropical zones than in the Tropics.

Before beginning to relate my observations, I feel it my duty to place on record my great indebtedness to the military authorities of the British, Italian, French, Serbian, and Russian Armies; and I wish also to express my thanks to the Serbian Relief Fund and its Committee, and to the American Red Cross Commission, to the hospitals of which I have been attached on several occasions and where I have carried out a certain amount of my researches.

I beg to express my thanks to Surgeon-General Rho, the Chief of my Service, for permission to publish this paper.

Malaria.

The number of cases of malaria throughout the Adriatic and Balkanic zones during the years 1915-16 has been appalling, and the cases of pernicious type quite common. Malaria has been the disease which has caused by far most invaliding among the allied troops.

Unfortunately during recent years in certain quarters in Europe and even in tropical countries, there arose a tendency to consider malaria as being a not very dangerous disease, and that its importance had been somewhat exaggerated — its prevention and cure being, in theory, a very simple matter. The present experience in the Balkans has, therefore, come as a rude awakening, and has shown what a terrible scourge this disease can be.

The experience we have had in Serbia, Macedonia, and the Adriatic zone, in addition to showing the frequency and seriousness of this malady, has also shown what a protean disease it is. This, of course is nothing new to the old tropical practitioner, but it is decidedly bewildering to the young medical man just out from home. It is especially on these protean features of the malarial infection, as well as on the prognosis and treatment of the malady, that I should like to say a few words.

It may be said at once that in the Balkanic zone the classical malarial fevers, as described in books, are not frequent. The clinically atypical cases are extremely numerous, and the patient may present symptoms pointing to the most different pathological conditions; indeed there are very few internal diseases which malaria may not simulate, and it may even give rise to syndromes closely resembling acute surgical conditions.

I shall take, to begin with, the diseases of the blood.

Diseases of the Blood.

The Purpuric and the Hæmorrhagic Type of Malaria.—Near Skopolje, in Serbia, in September, 1915, an outbreak of so-called scurvy was reported as occurring in a Serbian regiment, and I was asked to investigate the condition. The patients were extremely pale, felt very weak and languid, complained of pains in the loins and limbs; in most cases the whole body was covered with petechiæ, and tense, indurated swellings due to large extravasations of blood were present in several cases. In many of the patients bleeding was taking place from the gums, which were swollen and spongy, and from the nose; in two cases there were hæmorrhages from the stomach and the intestine; in one there was hæmorrhage from the lungs and bronchi; in another from the kidneys and bladder. In none of the cases was fever noticed, but the presence of a large indurated spleen in two of the patients, and the fact that I had seen similar cases in the Tropics, made me suspect malaria, and, in fact, the blood examinations made soon after showed presence of malaria parasites, though only in about 10 per cent. of the patients. However, all of them were put on quinine, and all the symptoms improved in a remarkably rapid manner, and disappeared completely within three to six weeks. In some cases I gave calcium lactate in association with quinine, but I soon found out it was unnecessary. The diet was not changed in any way. This hæmorrhagic type of malaria must be distinguished from the ordinary types of purpura, from scorbutus, also from a peculiar hæmorrhagic affection, without jaundice, of spirochætic origin.

The Pernicious Anæmia Type of Malaria.—Of this type I have seen several cases both in the Balkans and in tropical countries. The appearance of the patient is quite different from what one sees in ordinary malarial cachexia, and there may be no history and no sign whatever pointing to a malarial infection. The patient is generally a young man or woman with the typical pallor and the lemon-yellow tint of pernicious anæmia; in some cases the liver is slightly enlarged, or, more rarely, the spleen; there is no fever, or just 99° or 100° F. at night, which is quite common also in true pernicious anæmia. The blood may be negative for malaria on repeated examinations, and may show most of the common features of pernicious anæmia, such as poikilocytosis, nucleated red cells, high colour index, relative increase of the small mononuclears instead of the large mononuclears, as one would expect in malaria.

We had one such case for several months in 1915, at one of the Uskub hospitals. The patient, a Serbian soldier, aged 21, was fairly well nourished but very pale, with a lemon-yellowish tint; he was always very languid and apathetic and stayed in bed the whole time; no fever, no enlargement of spleen or liver. Blood: 2,100,000 red blood corpuscles; marked poikilocytosis, some nucleated erythrocytes present; leucocytes, 5,000, with a large increase of the lymphocytes (55 per cent.), instead of the large mononuclears as one would expect in a malarial condition. He was considered to be a case of pernicious anæmia, and accordingly was treated with arsenic for several months, when suddenly one day he had a rigor, the temperature going up to 105° F.; the spleen became just palpable and a few subtertian rings were found in the blood. A quinine treatment by intramuscular injections was immediately started, and within two months the patient was so improved that he was discharged, and returned to his regiment.

In old individuals the profound anæmia may give rise to the suspicion of *internal cancer*. A man aged 60 was admitted to Sorovich Hospital in a condition of profound anæmia; he was greatly emaciated, with no fever, no enlargement of the spleen or liver, dyspeptic symptoms dating from several months; complete loss of appetite, pain after food, and vomiting, which he said was of a very dark colour. The doctor who had attended him very naturally had suspected cancer, especially as the usual treatment for dyspepsia, dieting, &c., had completely failed. Some time after his admission to hospital, the examination of the blood one day revealed the presence of a few Laveran's crescents. A quinine treatment caused all the symptoms to disappear within a few weeks.

Diseases of the Nervous System.

The Polyneuritic Type.—This type has been denied by many observers, but I have come across a number of cases both in the Balkans and in the Tropics. I would call attention to the type of malarial polyneuritis which simulates wet beri-beri very closely. The patient is œdematous, there is the characteristic gait, knee-jerks absent. No fever, no enlargement of the spleen and liver. In two such cases the blood showed Laveran's crescents. They both got well on quinine given in large doses by the mouth, and by intramuscular injection. One of my patients suffering from malarial polyneuritis showed peculiar mental symptoms with loss of memory for recent events—a condition

resembling Korsakov's disease, polyneuritic psychosis. A similar case has been recently ably described by Dr. H. Carlill.[1]

As regards cases of mononeuritis and neuralgia, tic, &c., of malarial origin, these are, as well known, quite common. I have seen an interesting case of *meralgia paræsthetica*, which had resisted every treatment for months, yield to quinine in twenty-four hours.

Malarial syndromes of the central nervous system are quite common in the Balkans. I may mention some of the types I have seen :—

Comatose Type.—This was quite common during the epidemic of malaria in Skopolje and the surrounding districts during August and September, 1915. The patient is brought into hospital comatose ; he cannot be roused to answer questions. The respiration may be stertorous, or sometimes quiet. The pupils are often contracted, there may be high fever, or the temperature may be normal or subnormal, and in this case a diagnosis of opium poisoning has occasionally been made. The comatose condition becomes deeper and deeper, and the patient often dies a few hours after admission : though at times intravenous and intramuscular injections of quinine may induce a cure. Very recently a soldier was brought to my ward at the Sorovich Hospital in a comatose condition ; temperature 101° F., spleen palpable. He was given at once 30 gr. of quinine intramuscularly (15 gr. both buttocks). He became conscious after a few hours and recovered. The comatose type of malaria is of great interest : it may develop quite suddenly in old malarial patients, also in temperate zones. A most interesting case has recently been observed in London by Professor Hewlett. The patient, a sailor, suffered from very indefinite symptoms and went to a venereal clinic. His blood was tested for Wassermann's reaction, which was found positive. He was given an injection of salvarsan. A few days later the same patient was brought in a comatose condition to the Seamen's Hospital and died within a few hours of admission. At the post-mortem nothing was found pointing to any definite pathological condition, but Dr. Hewlett made a histological examination of the brain, and this revealed enormous numbers of malarial parasites in the capillaries.

The Delirious Type.—In this type, of which I have seen several cases in Skopolje, delirium is the most marked feature : it may be followed by coma and death. These cases have been occasionally confused with alcoholic delirium tremens.

[1] *Lancet*, 1917, i, p. 648.

The Cerebrospinal Type.—This may simulate cerebrospinal meningitis very closely. A patient was admitted to one of the Uskub Hospitals in September, 1915, with extremely severe headache and vomiting, fever, retraction of head, Kernig's symptom. Blood: Slight leucocytosis, no parasites. A diagnosis of cerebrospinal meningitis was made, the patient isolated and a lumbar puncture performed. The cerebrospinal fluid escaped with higher pressure than normal, but was completely clear. Blood again examined—malaria. All the symptoms disappeared after a few intramuscular injections of quinine.

The hemiplegic and monoplegic types are occasionally met with, simulating cerebral hæmorrhage. I have seen several such cases. Subacute and chronic, cerebral and cerebellar types are not very rare. At the American Red Cross Clinic in Skopolje I saw a most interesting

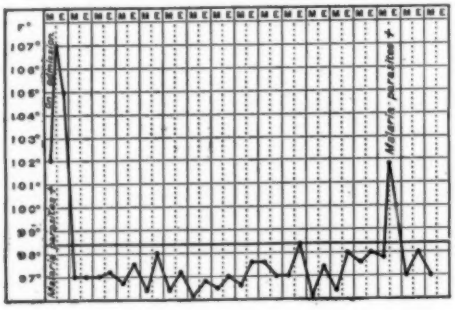


FIG. 1.
Hyperpyrexia.

case of the cerebellar type. A man aged about 30 came for treatment showing all the symptoms of a cerebellar tumour: he was ataxic, walking like a drunken man: he complained of severe headache, vomiting, and almost complete loss of vision. His temperature was normal and he did not give any history of fever. Spleen and liver not enlarged; the blood showed numerous crescents. All the symptoms disappeared after an energetic quinine treatment. In Ceylon I have seen a similar case, also cases with symptoms pointing to tumour of some other part of the cerebrum. Some cases have intermittent fever, generally low, and then an abscess is suspected. I have seen a case of bulbar malaria, with difficulty of speech, deglutition, and facial paralysis.

The spinal types are rare, but I have seen a case of clinically typical transverse myelitis due to malaria. I have seen one case in the

Balkans, and three in the Tropics, of a malarial condition closely simulating *disseminated sclerosis*, with scanning speech, intentional tremor, nystagmus, spastic gait, and increased reflexes. All the cases were cured by quinine.

The mental type of malaria may occasionally simulate mental disease. I have come across two cases of apparent mania, and one of apparent melancholia, all cured by quinine. I have already mentioned a case presenting Korsakov's syndrome.

Specific Infectious Diseases.

Malaria may simulate various specific infections. *The typhoid-like type* of malaria was far from rare in South Serbia and Macedonia in 1915, and I have again seen similar cases very recently near Monastir.

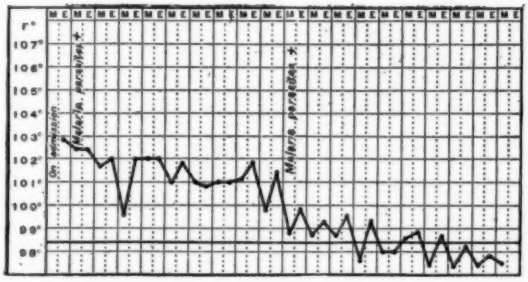


FIG. 2.

Malaria simulating typhoid; hæmocultures and serum reactions for enterica negative.

From Gevgheli for instance it was reported in September, 1915, that an epidemic of typhoid had broken out. My friend, Dr. Strong, Head of the American Sanitary Commission, Dr. Th. Jackson, and later on myself, went there and soon found out that more than 90 per cent. of the so-called cases of typhoid were cases of typhoid-like subtertian. This type as a matter of fact was fairly common also in Skopolje, and numerous cases were admitted to the Lady Paget Hospital in September, 1915. In this type of malarial fever, as seen in the Balkans (I have seen similar cases in the Tropics), the onset was often slow; the patient looked apathetic and complained of headache; tongue very coated; the fever was continuous or subcontinuous; abdomen slightly tumid, spleen generally palpable, but neither very

large nor very hard. The patient had certainly all the appearances of a typhoid case. The blood showed as a rule numerous ring forms, and—a most important feature—quinine given in large doses had practically no influence for a long time either on the fever or the parasites. All the bacteriological examinations for typhoid, paratyphoid, and intermediate germs, were as a rule, negative; very occasionally a case of mixed infection—malaria and typhoid—was found.

Malta Fever Type.—The undulant type of malaria is extremely rare, but it certainly exists. I have seen a case in Macedonia in 1915: the fever of a typical undulant type continued many weeks and was not influenced in the least by quinine 10 gr. t.i.d. given by the mouth for several weeks. Malarial parasites were found in the blood repeatedly; agglutinins for Malta and paramelitensis fever negative. The fever finally yielded to quinine given intramuscularly.

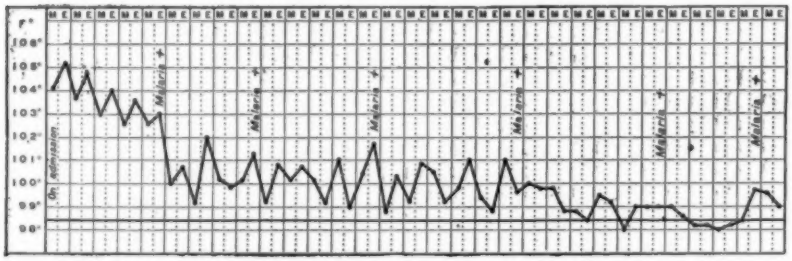


FIG. 3.

Temperature chart of a case of malaria simulating typhoid; hæmocultures and serum reactions negative.

Yellow Fever Type and Weil's Disease.—A Serbian officer in Macedonia suddenly developed fever (temperature 103° F.) with severe headache, flushed face, severe pains in the body, no rigors, pulse quick, full and bounding, severe vomiting with tenderness in the epigastric region; slight albuminuria. On the morning of the third day the temperature dropped to 100° F., the pains disappearing, and the patient felt better, but on the evening of the same day the temperature rose to 105° F., while the pulse became comparatively slow (64 per minute); jaundice appeared, the spleen became slightly palpable, severe vomiting started again, of dark brown colour, containing red blood corpuscles; the jaundice deepened very quickly, all the symptoms became worse, and the patient died on the sixth day, notwithstanding a very intensive

quinine treatment. The blood was teeming with subtertian ring forms. The type of fever I have described belongs to the group which the old authors called bilious remittent fever. I saw in an island of the Adriatic zone a very similar case, but with severe hæmorrhages in the skin, closely resembling icterus hæmorrhagicus and terminating fatally. Cases of malarial jaundice simulating a simple catarrhal jaundice were also seen occasionally.

Tetanus-like Malaria.—I have seen a typical case in Macedonia in 1915, and three in the Tropics. The patient had a normal temperature,

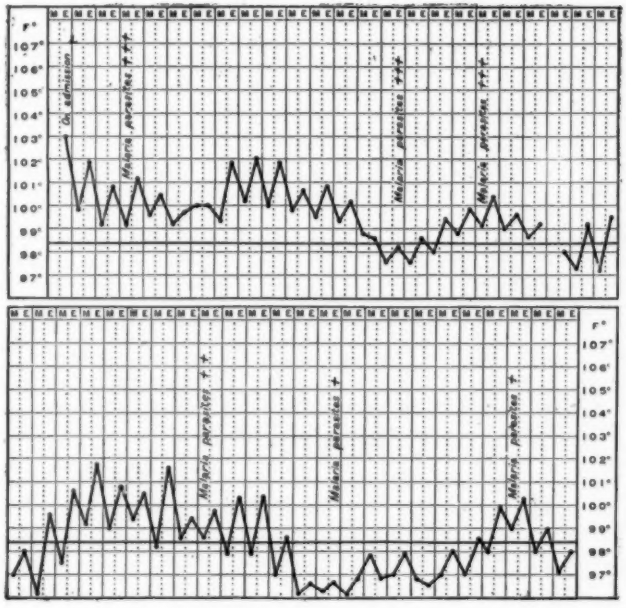


FIG. 4.

Temperature chart of a case of malaria simulating Malta fever.

with trismus opisthotonus, and typical tetanic spasms. In all the cases the blood was +, and an intensive quinine treatment induced a cure.

Hydrophobia-like Malaria.—I was once called by a Ceylonese colleague near Colombo, to see a case which had been diagnosed as hydrophobia. At the time I saw the patient he was in a maniacal condition, temperature 102° F. At the mere sight of water a severe spasm of the larynx occurred. As he had a large spleen I examined his

blood with my portable microscope; it was teeming with parasites. Quinine hyd., 15 gr., given by intramuscular injection in each gluteal region, caused all the symptoms of hydrophobia to disappear in a few hours. It may have been a case of hysterical hydrophobia syndrome in a malarial patient, though the man was not of neurotic tendency, and no hysterical stigmata were present.

Sleeping Sickness Type.—This type of malaria, so far as I know, has never been described. I have seen a typical case in a tropical country where sleeping sickness does not exist. The patient had low fever for months, with slight trembling of the hands and tongue and progressive general weakness; he then became drowsy and occasionally had convulsions. The blood examination was negative for every known parasite, until many months after the onset of the symptoms a few malarial parasites were once found. A course of quinine injections cured the patient. A somewhat similar case though not so severe, with general debility, trembling of hands and tongue, very marked drowsiness for weeks, was seen by Dr. Mitchell and myself in Skopolje, but the correct diagnosis in this case was made at once, as the blood was full of malarial parasites. The patient very slowly recovered on being given quinine treatment.

Diseases of the Digestive System.

I have already called attention to those cases of severe malarial anæmia in old people with dyspeptic symptoms which may suggest carcinoma ventriculi. I may mention now a case with symptoms pointing to gastric ulcer which I saw in Ceylon. The patient, a young girl, was very anæmic and had severe gastralgia, the pain shooting often backwards; severe vomiting, hyperchlorhydria. The examination of the blood showed a few malarial parasites. Cases of so-called simple dyspepsia or gastric catarrh, due to a patent malarial infection, have not rarely been observed by me.

Pseudo-dysentery of Malarial Origin.—Of this form two types in my experience may be distinguished: one with typical dysenteric stools containing blood and muco-pus, the other with purely hæmorrhagic stools without any pus, and very little or no mucus. I shall always remember a case of the first type which I once saw in consultation in a tropical country. The patient had been treated with ipecacuanha, anti-dysentery serum, bismuth, salol, nitrate of silver irrigations, &c., for months: it very rapidly and completely yielded to quinine within

forty-eight hours. In this case the temperature always had been normal: on examining the patient I was just able to feel the spleen, and this put me on the right track. Blood +. On questioning the patient we elicited the fact that he had had attacks of malarial fever six years previously. Of course one must not forget that there are quite a number of cases of mixed infection: malaria and bacterial or amœbic dysentery; but the case I have described was certainly not one of these; the examination of the stools which I made being negative for entamœbæ and germs of the Shiga-Kruse and Flexner groups. I have seen very similar cases also in the Balkans.

Pseudo-cholera. — During the epidemic of cholera in Ceylon in 1914, I was called to confirm the diagnosis of cholera in a European assistant of one of the big tea firms of Colombo, so that he could be removed to the infectious diseases hospital, which proceeding the patient strongly objected to and was resisting with what little strength he had left. He had been taken ill suddenly in the night with profuse serous diarrhœa. When I was called to see him at 11 a.m., he was feeling very cold, his skin clammy, his cheeks sunken, and he looked very ill indeed. Temperature 99° F., pulse 104. The stools were absolutely cholera-like—the well-known rice water appearance. Examining the patient I found the spleen just palpable, but very hard. On questioning him I found that he had an attack of malarial fever three weeks previously, on an estate where he had gone for a short holiday. The blood was examined at once: it was swarming with ring forms. Quinine was given immediately by intramuscular injection, 30 gr., and the diarrhœa stopped within a very few hours. The patient was up two days later. The complete bacteriological examination of the stools revealed entire absence of the cholera vibrio, or of vibrios of the para-cholera group. A somewhat similar case, not so bad, which also had been taken for cholera, was seen by me in Northern Macedonia in 1915. This case also recovered very quickly on quinine. Cases of the same nature, though not so typically cholera-like, have been seen by me in Corfu and at Salonika.

Cholecystitis Syndrome.—I have seen three cases in Skopolje with all the symptoms of acute cholecystitis due to calculi: severe pain shooting towards the right shoulder, tenderness over the region of the gall-bladder, severe vomiting. In two cases slight jaundice; the spleen was very slightly palpable in one case, liver enlarged in two cases. Blood positive for malaria in all three. All were completely cured by quinine.

Malaria simulating Abscess of the Liver.—I have seen one such case in Ceylon in consultation with a local doctor; the patient had developed his disease on an estate up country where malaria is practically unknown, and he gave a history of having had dysentery ten years previously. When I saw him he had been suffering from intermittent fever for two months; the fever had not yielded to quinine (30 gr. daily by the mouth). Profuse sweatings, loss of flesh, spleen not palpable, liver enlarged but not very hard, severe pain on pressure all over the region of the liver. Blood said to have been negative for malaria every time it had been examined (four times). The day I examined the patient, however, the blood showed a few crescents. An intramuscular quinine treatment supplementing that given by the mouth was ordered, and all the symptoms disappeared within three weeks.

For the benefit of the young practitioner in the Tropics I may say, however, that mistaking abscess of the liver for malaria is a far more common error than mistaking malaria for abscess of the liver; while I have come across only one case of malaria diagnosed as abscess of the liver, I have seen a great many cases of abscess of the liver diagnosed as malaria and drenched with quinine for months, and even years.

Cirrhosis of the Liver of Malarial Origin.—This condition, in my experience, is not very frequent. I treated a typical case, however, in the Lady Paget Hospital in Skopolje in 1915. The patient, a soldier, aged 23, with typical hepatic facies, had ascites. After tapping, one could feel the liver and spleen to be enlarged and hard; the fluid soon collected in the peritoneal cavity, and the feet and legs commenced to become œdematous. After tapping again the liver was found to be much smaller. After repeated examinations of the blood parasites were found; a quinine treatment cured the condition, though very slowly.

Acute Hæmorrhagic Pancreatitis Syndrome.—A man, aged 42, in apparently perfect health, was taken suddenly ill with violent pain in the epigastrium without any apparent reason; this was very quickly followed by very severe vomiting and complete collapse. There was a circumscribed area of very severe tenderness over the upper portion of the epigastric region which was very tympanitic and seemed to be somewhat swollen. A diagnosis of probable acute hæmorrhagic pancreatitis was made by the attending physician, and an operation suggested, but on examining the blood this was found to be swarming with malaria parasites.

Pseudo-appendicitis.—I have seen several cases in the Balkans and two in Ceylon. A Serbian soldier in Skopolje in 1915 was suddenly

taken ill with violent pain in the appendicular region; vomiting and fever, but no rigors; spleen and liver not enlarged; palpation revealed severe tenderness and well-marked rigidity all over the right lower abdomen. The blood showed numerous malaria parasites and the patient got well within twenty-four hours after two intramuscular injections of quinine. A colleague had a similar case, with all the typical symptoms of appendicitis. He examined the blood, but no malaria parasites were seen. A surgeon called to see the patient decided to operate, and the appendix was removed; macroscopically it was quite normal. On the third day after the operation the patient had a very severe rigor, and complained again of pain in the cæco-colon region, though more diffuse; his temperature jumped to 105° F., the spleen became palpable, and on examination the blood showed several subtertian rings. Quinine was administered by intramuscular injections and the temperature dropped to normal within thirty-six hours. The microscopical examination of the blood in these cases is always of great help, even if malaria parasites are not found, as the polymorphonuclear leucocytosis of true appendicitis is often absent, though I must not omit to say there are several exceptions.

Pseudo-peritonitis.—A lady in Skopolje was supposed to be suffering from peritonitis due to some old uterine affection. She had low fever, pinched face, vomiting, severe pain and tenderness all over the abdomen. The spleen could not be felt, but palpation was difficult owing to rigidity of the muscles. The blood was examined by me and found to be teeming with malaria parasites. Quinine cured the condition in a few days.

Diseases of the Respiratory and Circulatory Systems.—I would call attention to certain manifestations of malaria affecting the bronchi, lungs, heart and vessels. If the chest is thoroughly examined at the very commencement of an ordinary malarial attack when the temperature just begins to rise, one will often notice a few dry râles, rather coarse; but the feature to which I should like to call attention is that, not rarely, especially at the pulmonary bases, very minute crepitations are heard, which may possibly be of pleural origin; they generally disappear when the temperature has reached its summit. They are quite different from the pseudo-crepitations one hears at times in patients who have been lying long in bed on their backs, as they do not disappear after a few inspirations. I have come across several cases, both in the Tropics and in the Balkans, of acute *dry bronchitis* of undoubtedly malarial origin often with very little or no fever. One such case had

been treated for months with expectorants and potassium iodide without any result, while a few doses of quinine caused all the symptoms to disappear. Similarly, I have come across a few cases of dry pleurisy of malarial origin, and two cases of consolidation of the lung also of malarial origin, closely simulating lobar pneumonia. One of the patients was taken suddenly ill with a rigor and fever after a cold bath; severe pain on the left side, cough with expectoration, which became blood-stained on the evening of the second day; at the right base a tympanitic zone, which on the third day become somewhat dull; spleen just palpable. As the blood of the general circulation and the red cells in the expectoration contained numerous malaria parasites, quinine was given intramuscularly in large doses. The fever dropped on the fourth day. As regards the *circulatory system* I have observed three cases of angina pectoris syndrome, very severe, to be completely cured by quinine. *Peripheral arteritis* is not very rare. One of my native assistants in Ceylon had several attacks of a peculiar condition; his superficial temporal artery on the left side used to become very swollen, and at the same time very hard and knotty to the touch, and extremely painful on palpation. The attack at times came on without any fever, with parasites in the blood. Quinine in large doses generally caused the attack to subside within forty-eight hours. I have seen cases of arteritis of the lower extremities ending in gangrene and conditions resembling Raynaud's disease. This malarial gangrene has been ably investigated at Salonika by Falconer and Anderson. I have seen cases of what the French authors call "Claudication intermittente," which cleared up completely on quinine being given; also a case of erythromelalgia and acroparæsthesia due to the same cause. Of so-called functional affections of the heart I have seen cases of palpitation, tachycardia, arrhythmia, and one typical case of heart block, certainly of malarial origin. This case of heart block was in a middle-aged man; he had had several attacks—slow pulse, epileptic seizures, visible auricular impulses in the veins of the neck, 3 : 1 rhythm. He denied ever having had fever. His spleen, however, was slightly enlarged and very hard. The blood showed a few malaria parasites. A persistent quinine treatment freed him of his attacks.

Diseases of the Uro-genital Organs.—Cases of malarial nephritis have been described: in men a neuralgia of the testicles; in women a neuralgia of the ovaries of malarial origin is occasionally met with. Epididymitis and orchitis have also been described. I have seen a case in Macedonia. The patient, who was living in a very malarious district,

but had never had attacks of fever, complained suddenly of severe pains in the testicles. When I saw him both testicles were swollen and very tender, but there were no signs of effusion; no gonorrhœa; temperature 101° F.; blood +. Quinine caused all the symptoms to disappear in three days. I have seen a very interesting case of *priapism* of malarial origin. Blood positive. The condition, which had not been influenced in the least by full doses of bromide, hot baths, belladonna, &c., disappeared on large doses of quinine being given.

Skin.—Various skin eruptions may be seen in malaria, but on the whole—apart from herpes—in my experience they are not very frequent. In acute cases the most frequent is herpes, of which the commonest type is herpes labialis, then herpes progenitalis, seldom herpes zoster. I have seen various cases of diffuse erythema and a few of urticaria of malarial origin, also a peculiar patchy œdematous condition which had been erroneously diagnosed as Calabar swelling and Quincke's œdema. I have seen a case of very severe scarlatiniform erythema of malarial origin, followed by desquamation in large flakes; also a typical case of erythema multiforme, and one of erythema nodosum. Cases of purpura I have already described.

Papular rashes are very rare. A young European in Ceylon was suddenly taken ill with high fever, vomiting, and extremely severe pains in the back. Two days after a papular eruption appeared; the papules were small and shotty, and very abundant on the face. The case was suspected to be one of incipient small-pox until, after repeated examinations, malaria parasites were found in the blood. Quinine cured the condition quickly.

Pigmentation.—Everyone is acquainted with the earth-coloured appearance of the skin in chronic cases; at other times the skin is pale yellow, greenish-yellow, or ashy-grey. Patches of hyperpigmentation (chloasma malaricum) are very common, and cases of diffuse pigmentation with great loss of flesh and severe asthenia, closely resembling Addison's disease, are occasionally met with.

Diagnosis, Prognosis, and Treatment of Malaria.

Before leaving the subject of malaria I should like to say a few words on the diagnosis, prognosis, and treatment of the malady.

Diagnosis.—To the young medical officers going to the Balkanic zone, I would give the following hints:—

(1) Whatever the type of fever and the symptoms of the case you

have been called in to see—even in presence of a syndrome pointing to an acute surgical condition—keep in mind the possibility of malaria.

(2) A negative blood examination is of no importance unless repeated with the same result very many times, and at different intervals.

(3) The fact of quinine not influencing the fever does not always exclude malaria.

(4) While attaching very great importance to laboratory methods—it is only by this means that a positive diagnosis can be made with absolute certainty—do not neglect the clinical examination of your patients. Train your senses as much as possible. The importance of simple inspection, the importance of simple palpation, can hardly be exaggerated. A slight abnormality of the colour and pigmentation of the patient's skin, easily overlooked by the inexperienced, may suggest to the tropical clinician a diagnosis of probability in an obscure case; and, acting on it, he may perhaps save the patient's life. And in how many cases may not the faintest feeling of the tip of the spleen—impalpable to less trained hands—put one on the right track in a difficult case.

Prognosis.—In the Balkans, as in any other malarial country, provided an appropriate quinine treatment be given, the prognosis is good in the enormous majority of cases. It must be remembered, however, that (1) in pernicious cases a high mortality may be present notwithstanding an energetic quinine treatment; (2) in a large number of cases, even of the usual benign type, the cure which is brought about by an appropriate treatment is merely clinical—that is, the patient feels well and shows no signs of the malady, except perhaps a persistent large mononuclear increase, but a complete sterilization is not obtained. The affection remains dormant for months and years, and any cause lowering the resisting power of the individual, such as chill or traumatism, may cause the acute symptoms to reappear. I have known the disease to recur in patients in England ten and fifteen years after leaving the Tropics. This, after all, is analogous to what one sees in certain other protozoal diseases — syphilis, for instance, yaws, and sleeping sickness. I have mentioned traumatism; an operation, for instance—the simplest operation may re-awaken a very old dormant malarial infection; the patient, shortly after the most simple surgical intervention, may have a rigor, the temperature rapidly rises to 104° or 105° F.; very severe vomiting sets in: symptoms which may cause a great deal of anxiety to the surgeon, who naturally may be very far from thinking of malaria.

Treatment.—In my experience the treatment of malaria in the Balkanic area, as in any other country, may be described in the one word—*quinine*. Only it must be given in very large doses, 30 to 60 gr. and even more, daily. The drug should be given by the mouth in ordinary cases, by intramuscular injection in serious cases; intravenously in the pernicious forms. In the many years I have practised in the Tropics, and during the last two years in the Balkans, I have given almost numberless injections of quinine without any bad results except a slight local infiltration at the place of injection. It is, of course, essential that the solution should be accurately prepared and sterilized; the simplest way is to use the ampoules each containing 15 gr. of hydr. of quinine, which should be obtained from a reputable firm. Of the enormous advantage of using intramuscular and intravenous injections of quinine in serious cases I have no doubt whatever; they are a means of saving life in many pernicious cases. I well remember a certain hospital in Serbia in which the mortality for pernicious malaria was practically 100 per cent.; this rate was reduced to less than 20 per cent. after injection treatment was introduced. While quinine, in my opinion, is the most powerful drug we possess for fighting malaria, and in a general way may be called a specific, it must be admitted that there are cases of malaria which respond very slowly or not at all to a simple quinine treatment. In such cases other drugs have been tried; of these I may mention picric acid, methylene blue, antimony, arsenic; and of arsenic both the old preparations—liq. arsenicalis, arsenious acid—and the more recent compounds such as atoxyl, salvarsan, galyl, &c. In my experience when given alone none of these drugs has any marked effect on malaria. When given, however, in association (sometimes alternately) with quinine, they are useful in certain cases and seem to increase the effect of the quinine. The efficacy of a combined quinine-arsenical treatment in certain forms of the malady has been recognized for many years, and has been extensively used in the Tropics. A combined method which has given me satisfactory results in certain cases in which quinine alone was acting very slowly is a combined quinine, phosphorus, and tartar emetic treatment. The patient takes daily a dose of the following quinine-tartar emetic mixture:—

Quin. sulph.	10 gr.
Ac. sulphur. dil.	10 minims
Tartar emetic	$\frac{1}{6}$ to $\frac{1}{4}$ gr.
Codein	$\frac{1}{4}$ gr.
Syr.	1 dr.
Aq. chlorof.	ad 1 oz.

t.d. or every four hours.

In addition to taking this mixture daily, the patient is given one day an intramuscular injection of quin. hyd. (15 gr.), and the next day a subcutaneous injection of 1 to 4 minims of phosphorated oil. This phosphorated oil should be of recent preparation, and may be diluted with sterile oil. The injection is made subcutaneously in the arm, just like an ordinary injection of morphia; at the time of the injection the patient feels no pain whatever, but after a few hours some pain may be felt which, however, is not very severe—much less so, for instance, than after an emetin injection. I have used phosphorated oil by subcutaneous injection for many years in the treatment of various diseases such as osteomalacia, rickets, leukæmia, &c., and I have never noticed any untoward symptom, provided, of course, the treatment is carried out with all due caution and care. Neither antimony nor phosphorated oil when given alone have any distinct effect on malaria; they seem to act as coadjuvants.

Prophylaxis.—I will limit myself to stating that, in my opinion, both methods of prophylaxis—the one based on anti-mosquito measures and the other based on the preventive administration of quinine—should always be used together. Exclusive reliance on the one or the other may be the cause of unsatisfactory results in practice. As regards anti-mosquito measures, the destruction of larvæ by oiling all pools and ponds near the camps, hospitals, &c., is by far the most useful; mechanical protection by means of mosquito nets, &c., should also be carried out. Chemical preparations to keep off mosquitoes are occasionally advantageous: the best is citronella oil; a more pleasant preparation to use is menthol powder, though less effective. As regards prophylaxis based on quinine administration, the usual dose of 5 gr. daily is utterly inadequate in the Balkans. The daily dose should be increased to at least 8 or 10 gr., and even these doses are at times insufficient to prevent infection.

Typhus Exanthematicus.

In reference to this malady, which according to certain observers has killed off nearly one tenth of the Serbian population during the years 1914-15, I would like to make a few remarks on the following points:—

(1) The rash.

(2) Course and duration of the malady: certain sequelæ and complications.

(3) Prophylaxis.

Rash.—Definite preliminary rashes are rare. What one generally sees is a very marked flushing of the face, neck, and upper portion of the chest, with a subcuticular mottling of the skin of the lower part of the chest and abdomen (cutis marmorata). This feature was present in about 80 per cent. of my cases during the second and third day of the malady. It should be noted at once that this symptom is far from being specific: it was extremely common, in fact almost constant, in the very numerous cases of pappataci fever I saw in the Skopolje district and other parts of Macedonia, as I shall mention later, and may be observed also in certain cases of relapsing fever and paratyphoid B. The true typhus rash appeared generally on the fourth or fifth day, in the form of small roseola spots, indistinguishable from typhoid roseola except that it was very much more abundant. It generally started on the abdomen and then spread to the chest, arms, and legs. Over the last two regions, however, it was rare, this being contrary to what one reads in text-books. The spots are at first all roseola-like and disappear on pressure, then some of the spots slowly fade away while others become of darker hue and do not disappear on pressure, but seldom become so dark bluish as in true purpuric conditions. According to my experience it must be admitted:—

(1) That the rash may remain purely roseola-like without any of the spots becoming petechial. It is then indistinguishable from enteric rash but for its profuseness in most cases. It should be noted, however, that certain cases of enteric may have a profuse rash: one such case in Skopolje turned out to be paratyphoid B, the patient being covered with a roseola.

(2) There are cases in which the rash is not only roseolar—and remains so—but is extremely scanty and may be altogether absent.

The newly arrived medical man should be on his guard not to mistake for typhus rash a petechial rash which I have called *Balkanic rash*, composed of innumerable, perfectly circular dark red petechiæ, which is extremely common in the Balkans in peasants and soldiers. I have many a time seen a patient taken to the typhus wards only because he had fever (often malaria) and this petechial rash which is merely due to the multiple bites of innumerable fleas. Anyone who has not been to the country can scarcely believe how profuse this rash can be: the whole body, with the exception perhaps of the face, is completely covered with it, while the shirt of the sufferer may be absolutely black from the numbers of living fleas upon it. With a little practice one soon learns easily to distinguish the two rashes: each

flea-bite shows at first a central hæmorrhagic spot surrounded by a hyperæmic circular zone which disappears on pressure. This peripheral hyperæmic zone fades away spontaneously within a day or two, while the central hæmorrhagic spot remains as a "petechial" area which is as a rule perfectly circular, not raised, and of a dark red colour which does not disappear on pressure.

Course of the Malady. Complications and Sequelæ.—The disease begins generally suddenly with pains all over the body, a feeling of prostration and occasionally rigors; but cases in which the onset is slow, somewhat typhoid-like, are far from rare. On an average the duration of fever in my cases was from fourteen to eighteen days. In 80 per cent. of them the fever will not come down by crisis, as usually stated, but by lysis, lasting in general three to five days, and occasionally much longer. Relapses may occur, but are rare: only once have I seen re-infection (interval between the two attacks was four months). I would call attention to the frequent co-existence of typhus and relapsing fever. This is explained by the same insect (louse) carrying the two infections. The commonest complications and sequelæ I have observed were, in order of frequency, parotitis, often proceeding to suppuration, gangrene of the feet, polyarthritis, neuritis. Several cases developed during convalescence symptoms of severe depression almost amounting to melancholia.

As regards the prophylaxis of typhus, I am a believer in the efficacy of taking every possible measure against lice, especially the sterilization of all soldiers' underclothing and uniforms at fixed intervals, and frequent bathing. As regards the use of various substances to keep away lice from the body, most are in practice rather disappointing, but may be useful in certain cases. Dr. Thomas W. Jackson and myself carried out a number of experiments on the subject in Serbia in 1915 and come to the following conclusions[1]:—

(1) In regard to solid and liquid insecticides, the substances which we have found to be deleterious to body lice (*Pediculus corporis* de Geer 1778) are, in the order of their efficiency: (i) kerosene oil and benzene, (ii) plain vaseline, (iii) guaiacol, (iv) anise preparations, (v) iodoform, (vi) lysol, cyllin, and preparations of similar type, (vii) carbolic acid solution (5 per cent.), (viii) naphthalene, (ix) camphor. We found that pyrethrum had a very feeble action, while boric acid, sulphur, corrosive sublimate and zinc sulphate, when used in powder form, had apparently

[1] See *Journ. Trop. Med.*, 1915, xviii, p. 253.

no action whatever. As regards bed-bugs, kerosene oil is the best insecticide. Next to it comes guaiacol.

(2) It is interesting to note that substances which are powerful licecides may have very little or no action on bed-bugs, and vice-versa. For instance iodoform, which kills lice in ten to fifteen minutes, has practically no deleterious action on bed-bugs, which may live for more than twenty-four hours when exposed to it. It has also very little effect on fleas. Pyrethrum, on the other hand, has a much more powerful action on bed-bugs than on lice. It may be useful, therefore, in preparing insecticide powder for general use, to mix various substances, some deleterious to lice, others to bed-bugs, and so on.

(3) For use against lice on a large scale as among troops or prisoners, perhaps the best insecticide powder is naphthalene. This substance has a lower licecide action than kerosene oil, guaiacol, iodoform, and anise preparations, such as anethol, but it has a less unpleasant odour than the first three named and is much cheaper than anethol powder. In stored blankets and clothing it is also practicable and of use, as frequently lice are found upon the clothing and blankets stored through the summer. Naphthalene is useful for its well known deterrent action upon moths. We are speaking here of insecticide powders. As regards liquid insecticides the American Sanitary Commission sanctioned the use of kerosene by using it daily for troops and prisoners.

(4) For the better class of patients in practice a menthol powder (menthol 3 to 5 gr., zinc ox. 1 oz.) is to be preferred to naphthalene in most cases, as its odour is not unpleasant, while it is repellent to mosquitoes, in addition to lice and fleas. Such powder is especially useful in summer and in hot countries, as it has a cooling effect on the skin and often prevents prickly heat.

(5) As regards the use of ointments, plain vaseline is a powerful licecide, and it is doubtful whether the addition of white precipitate, &c., increases its action. In practice, however, it is useful to add a small amount of white precipitate (3 gr. to 1 oz.) as it has a beneficial effect on secondary pyogenic infection so common in people harbouring lice.

Relapsing Fever.

Relapsing fever was extremely common in Serbia in 1915, in the spring and summer especially. It was not rare in 1916 among the Serbian troops in Corfu, after the retreat through Albania, and cases have been seen by me again in the Balkans this past winter. I would

call attention to the comparative frequency of mixed infections which were seen in 1915: typhus exanthematicus + relapsing fever; malaria + relapsing fever. In Skopolje in 1915 it was extremely common to see relapsing fever developing in convalescents from typhus. The frequent coexistence of the two infections is probably explainable from their being carried by the same insect, the louse; though personally I am inclined to believe that relapsing fever is carried also by bed-bugs. As regards the ætiology of the disease, as I hope to demonstrate in a future publication, I believe that—as is the case in Africa, America, and Asia—it is also due in Europe to several species of spirochætes and not to one only (*Spirochæta recurrentis*, Obermeyer). As regards clinical symptoms I would point out how extremely difficult it is to diagnose from purely clinical symptoms at the onset of the first attack, relapsing fever from typhus, malaria, and pappataci fever. The different diagnosis will be discussed later.

I would call attention to two skin features I have noted: the so-called *cutis marmorata*, and flushing of the face, very frequent, and occasionally a very fine rash composed of very small roundish delicate pinkish or red roseola spots on chest, abdomen, and trunk, even in cases in which a mixed infection with typhus exanthematicus or enteric could be excluded with certainty, as the fever dropped on the fifth or sixth day.

On the whole, though relapsing fever is certainly a serious disease, it is not very dangerous, and the mortality is low—in Serbia less than 3 per cent.

In the treatment of the malady I have found salvarsan, neo-salvarsan and galyl, to be fairly efficacious when given intravenously in 0·3 grm. doses, but there are a certain number of cases in which the results are disappointing. I have found it useful to associate tartar emetic with salvarsan. I give it intravenously, using the usual 2 per cent. solution (4 to 8 c.c.) or the following formula:—

Tartar emetic	4 grm.
Sol. ac. carbol., 1 per cent.	100 c.c.

Half to 2 c.c. is diluted at the time of injection with sufficient sterile saline to bring it to 5 c.c., and the whole injected into a vein, taking the usual precautions.

In conjunction with the intravenous administration, I have given it in certain cases also by intramuscular injection (special formulæ to be mentioned later on in connexion with kala-azar) and by the mouth in the form of my yaws mixture, which contains tartar emetic, potass. iodid.

sod. salyc. and sod. bic. Care should be taken to stop the drug during the crisis, or unpleasant symptoms of collapse may appear. The drug seems to have a beneficial action, but given alone seldom cuts short the first attack of fever, though in a certain number of cases apparently it prevents relapses. On the whole a mixed salvarsan-tartar emetic treatment is the best.

Pappataci Fever.

This fever is extremely common in summer all over the Balkanic and the Adriatic zone. The so-called Uskub or Skopolje fever, epidemics of which occur regularly every year in Skopolje and surrounding Macedonian districts, I found out in 1915 to be also pappataci fever. The disease, but for the rash, is very similar to dengue as I have seen it in the Far East and Ceylon. It begins suddenly with rheumatoid pains all over the body, headache, pains in the eyeballs, fever (which seldom goes higher than 104° F.) ; no rigors. The face is flushed and has often the appearance of being slightly bloated. The flushing is often so marked and severe as to amount to an erythematous rash, which often involves the neck. I would call attention to the appearance of the skin of the chest and at times of the abdomen, which shows frequently a delicate subcuticular mottling—*cutis marmorata*. The fever keeps high (102° to 104° F.) for thirty-four to forty-eight hours; it drops by crisis on the third day. Occasionally the fever lasts for four or five days, and may not drop so suddenly. At times some superficial lymphatic glands are enlarged.

I should like to make a few remarks on the following points: (1) The erythematous flushing of the face and neck and the appearance of the fauces. (2) Differential diagnosis between pappataci fever and typhus at onset.

(1) A feature of the well-known erythematous flushing of the face and neck has apparently escaped previous observers; this flushing is persistent, that is to say it lasts long after the fever is over—eight, ten, twelve, and even fifteen days—fading away very slowly. In the Skopolje wards it was not difficult to distinguish, among the convalescents from various diseases, those who had had pappataci fever, from this peculiar sign. As regards the appearance of the fauces, not only is there the usual congestion of tonsils and pharynx common to most acute fevers, but the mucosa of the soft palate presents a peculiar appearance—numerous small hyperæmic roundish spots, the eruption being sharply limited by a line of demarcation between the soft and hard palate. This appearance of the soft palate is not, of course,

peculiar to pappataci fever, for it may be found, for instance, in cases of relapsing fever and typhus exanthematicus, and in certain cases of malaria; it is rare however in typhoid. This appearance of the soft palate often persists for several days after the fever is over, in the same way as the flushing of the face.

Diagnosis between Pappataci Fever and Incipient Typhus.—When in the Balkans a patient is admitted into hospital with high fever, flushing of the face, subcuticular mottling of the chest and abdomen, it is most difficult to make a diagnosis between incipient typhus and pappataci fever, though to make such a diagnosis is of great practical importance. Great help is given by examining the blood: in pappataci fever leucopænia is always present, in typhus leucocytosis, or, at any rate, no leucopænia. When there is an epidemic of the two diseases, and there are hundreds of patients admitted daily, there is no time to count the leucocytes with a hæmocytometer; a rapid examination of a stained film, especially at the edges, generally enables the experienced observer to come to a conclusion at once as to whether there is leucocytosis or leucopænia. I have found this procedure extremely useful in many a doubtful case.

Dengue.

According to certain observers there have been quite a number of cases of dengue in Salonika, Gallipoli, and Macedonia. The cases which have been shown to me in Serbia and Macedonia were all pappataci fever, but there has been dengue in Gallipoli. Dengue cannot be distinguished from pappataci fever the first two days, as the symptoms of the two diseases are identical, including the leucopænia. On the third day the fever generally abates in both maladies, but whereas in pappataci fever the temperature often remains normal—though there are many exceptions—in dengue very often there is again slight fever, though not so high on the fourth, fifth, and occasionally also on the sixth day, and the patient during these days again feels ill. Moreover, when the temperature falls on the third day, the typical roseola or erythematous rash of dengue appears (during the crisis or shortly afterwards).

Though I have not myself seen dengue in the Balkans, I have seen almost innumerable cases of it in Ceylon and the Far East. I should like to call attention to one or two clinical points which have apparently been overlooked by some writers on the subject:—

(1) The condition of the superficial lymphatic glands.

(2) Relapses.

Superficial Lymphatic Glands.—In most of the text-books I am acquainted with, it is definitely stated that the lymphatic glands are never enlarged in dengue. In my experience, the enlargement of some lymphatic glands — cervical, axillary, &c. — is comparatively frequent, varying from 30 to 70 per cent. of the cases I have seen, and may be considered to be one of the common features of the disease. The glands most frequently affected are the cervical, then the axillary, then the inguinal, &c. The glands may occasionally be tender on pressure and may remain enlarged for some days, at times weeks, after the fever is over. The gland juice, aspirated with a sterile syringe and injected in healthy individuals will reproduce the disease (two positive cases out of three).

Relapses.—Relapses and reinfections are generally considered never to occur, or to be at least extremely rare, in dengue, but in my experience they are far from being so. I have known several individuals in Ceylon get a typical dengue attack, with rash, &c., in three consecutive years. A member of my own family had an attack of dengue in Colombo in November, 1913, another in February, 1914, and again in October of the same year. Every time the attack was typical and the rash was present.

Sequelæ.—I should like to say a few words about a sequela which is very distressing and very difficult to cure—polyarthritis, with swelling of various joints, either the small ones or the large ones, or both. The condition in certain cases, but for the fact that it develops after a typical attack of dengue, may be indistinguishable, clinically, from polyarthritis of rheumatic origin. Salicylates and iodides are useless, and the treatment is symptomatic; opiates have often to be used to allay the severe pains. This sequela in its severe type is, fortunately, comparatively rare, considering the very large number of cases of dengue occurring during an epidemic, but it is most distressing, being so little influenced by treatment. It may last from two to three months and sometimes a much longer time. A change of climate is often beneficial.

Other Tropical Diseases.

As the time is getting short I propose to omit relating *in extenso* the observations I have made on the other tropical diseases found in the Balkans, and will limit myself to the following very brief *résumé*:—

(1) Numerous tropical diseases are found in the Balkanic zone.

(2) Malaria is the most common and important of these diseases.

(3) Next to malaria the diarrhœas and dysenteries are the most common affections. The majority of cases of dysentery are of bacterial origin, but very numerous cases of the amœbic type are also seen. Cases of enteritis due to flagellates and, rarely, to ciliates, occur. Coccidiosis has been observed. Cholera and paracholera have been rare. Attention should be called to a peculiar choleraic type of bacterial dysentery; also to a type of diarrhœa seen in Serbian soldiers who had suffered terribly from starvation during the Albanian retreat. This



FIG. 5.

A case of "famine diarrhœa" in a starved Serbian soldier after the Albanian retreat.

diarrhœa closely resembled the famine diarrhœa observed in India by officers of the Indian Medical Service.

(4) *Camp jaundice (icterus castrensis)* is common. According to my experience two varieties can be distinguished: (1) A very severe type, hæmorrhagic, with high fever lasting ten to twelve days, showing occasionally a relapse—true Weil's disease, or icterus castrensis gravis,

rare; (2) a mild type, icterus castrensis levis, often afebrile, very common. Weil's disease, as is well known, is of spirochætic origin, but in my opinion it is probable that many cases of the mild type are also spirochætic. Owing to the almost complete lack of guinea-pigs the study of spirochætic jaundice is difficult in the Balkans. In my experience, however, the simple microscopic examination (Indian ink method, Giemsa staining) of urine, repeatedly centrifugalized, will, however, occasionally reveal spirochætes two or three weeks after the onset of the malady, and also in certain cases of a peculiar type of purpura without jaundice. In addition to true camp jaundice one comes across cases of jaundice of malarial origin, paratyphoid origin,



FIG. 6.

Vaccination of Serbian troops with tetravaccine (T.A.B.C.) by an American Red Cross doctor.

streptococcal origin, &c., nor should one forget the icteric type of relapsing fever.

(5) Fevers of the enteric group are fairly frequent in the Balkans, but during the last year have never assumed an epidemic type. Paratyphoid A and B are in certain districts more frequently met with than true typhoid. Paratyphoid and similar fevers due to intermediate germs are not rare. Lurie and myself have found cases due to *Bacillus columbensis*. Attention should be called to the somewhat frequent occurrence of mixed infections such as "typhoid-paratyphoid A," or "typhoid-paratyphoid B," or "paratyphoid A-paratyphoid B." I have

observed two cases of triple infection: "Typhoid-paratyphoid A-paratyphoid B"; diagnosis based on hæmocultures. In regard to the prophylaxis, the method of combined vaccination I devised many years ago and have used on a large scale ever since, has given good results. It has recently been adopted in all the allied armies, thanks especially to the efforts of Dreyer, Walker, Rho, Bassett-Smith, Conte and others.

(6) Of Malta fever I have seen only two cases in Macedonia and the interior of the Balkanic zone; it is more frequently met with on the coast and in the islands.

(7) Kala-azar of adults I have never seen in the Balkans; of the infantile type I have observed many cases in certain islands of the Adriatic and Ægean seas. It is rare in Macedonia, where I have seen only one case.

(8) Relapsing fever is quite common. In the treatment of the malady I have obtained the best results by using a combined salvarsan-tartar emetic treatment.

(9) Typhus exanthematicus is at the present moment very rare, whereas a terrible epidemic raged in 1914-15. Trench fever is occasionally met with; both of the types described in France have been met with also in the Balkans.

(10) Pappataci fever is extremely common in certain parts of the Balkans, especially so in the late summer and early autumn.

(11) *Bronchomycosis* and *Bronchospirochætosis* are observed.

(12) Pellagra is quite common in several districts of Macedonia, but I do not think there is any danger for our troops. I have seen a case of leprosy and a typical case of venereal granuloma.

(13) Of the tropical diseases which are of rarer occurrence, I may mention blackwater fever, filariasis, sprue, intestinal myiasis, mycotic, spirochætic, and flagellate urethritis.

(14) Certain tropical skin diseases are frequently met with. The following are a cause of great discomfort in summer and are often wrongly diagnosed: dermatitis interdigitalis epidermophytica, or "mango-toe"; tinea cruris, or "dhobie itch"; prickly heat; various types of tropical pyosis—such as pyosis Mansoni, pyosis discoides, &c. I have seen in 1915, in Macedonian peasants, cases of ulcus tropicum, oriental sore, ulcus infantum, blastomycosis, sporotrichosis, accladosis, &c. I have seen numerous cases of trichomycosis axillaris flava, nigra et rubra, of intertrigo saccharomycetica, and various other hyphomycetic affections. I have observed two cases of Madura foot; two of keratoma plantare sulcatum, and one, typical, of *ainhum*.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

EDITED BY

J. Y. W. MacALISTER

UNDER THE DIRECTION OF

THE EDITORIAL COMMITTEE

VOLUME THE TENTH

SESSION 1916-17

SECTION OF NEUROLOGY



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1917

Section of Neurology.

OFFICERS FOR THE SESSION 1916-17.

President—

W. D. Halliburton, M.D., F.R.S.

Vice-Presidents—

R. Percy Smith, M.D.
Henry Head, M.D., F.R.S.

Hon. Secretaries—

H. Campbell Thomson, M.D.
C. M. Hinds Howell, M.D.

Other Members of Council—

E. Bramwell, M.B.
E. Farquhar Buzzard, M.D.
James Chambers, M.D.
T. R. Elliott, M.D., F.R.S.
E. G. Fearnsides, M.D.
David Orr, M.B.
H. R. Prentice, M.B.
Percy Sargent, F.R.C.S.
W. T. Holmes Spicer, F.R.C.S.
T. Grainger Stewart, M.D.
James Taylor, M.D.
H. G. Turney, M.D.

Representative on Library Committee—

Leonard Guthrie, M.D.

Representative on Editorial Committee—

C. M. Hinds Howell, M.D.

SECTION OF NEUROLOGY.

CONTENTS.

October 26, 1916.

W. D. Halliburton, M.D., F.R.S. PAGE

Presidential Address: The Possible Functions of the Cerebrospinal Fluid 1

November 23, 1916.

George Riddoch, M.B., Captain R.A.M.C.(T.).

On the Relative Perceptions of Movement and a Stationary Object in Certain Visual Disturbances due to Occipital Injuries 13

January 25, 1917.

G. Newton Pitt, M.D., Major R.A.M.C.

Demonstration of Cases 35

J. S. Collier, M.D., Captain R.A.M.C.

Demonstration of Cases 36

Sir William Collins, M.D., M.P.

Gunshot Wound Producing Damage of Twelfth and Spinal Accessory Nerves, with Cross Union of their Fibres 39

H. S. Clogg, M.S.

A Case of Pituitary Tumour 40

E. Farquhar Buzzard, M.D.

(1) Complete Division of Spinal Cord with return of Reflex Activity below the Lesion 40

(2) Bullet Wound Producing a Segmental and Sensory Lesion of the Brain Stem 41

March 22, 1917.

PAGE

JAMES TAYLOR, M.D.

Case for Diagnosis 43

J. S. RISIEN RUSSELL, M.D.

Case of Labyrinthine Deafness and Vertigo 44

W. ALDREN TURNER, C.B., M.D., Lieutenant-Colonel R.A.M.C.

(1) Case of Tumour of First Dorsal Region of Spinal Cord 44

(2) Case of Myotonia Atrophica 45

(3) Neuritic Type of Progressive Muscular Atrophy (Charcot-Marie-Tooth) 46

F. E. BATTEN, M.D.

(1) Case of Amyotonia Congenita 47

(2) Case of Myasthenia Gravis 48

E. FARQUHAR BUZZARD, M.D.

(1) Case of Huntington's Chorea 48

(2) Internal Hydrocephalus? 49

E. D. ADRIAN, Captain R.A.M.C.

Neuritic Type of Progressive Muscular Atrophy (Charcot-Marie-Tooth) 49

The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

JOHN BALE, SONS & DANIELSSON, LTD., 83-91, Great Titchfield Street, London, W.1.

Section of Neurology.

President—Professor W. D. Halliburton, M.D., F.R.S.

(*October* 26, 1916.)

PRESIDENTIAL ADDRESS.

The Possible Functions of the Cerebrospinal Fluid.

By W. D. Halliburton, M.D., F.R.S.

So much has been written within recent years about the cerebrospinal fluid that I must apologize for my want of originality in selecting that subject for this address. But however hackneyed the subject, we are still far from possessing complete knowledge as to the functions and significance of the fluid. From the pathological as well as from the physiological point of view, we at least know that this remarkable fluid is one of the greatest importance, and its careful examination in many diseases has yielded results of inestimable value. During the last few years it is a subject at which I have myself worked, and I have been fortunate in securing as my collaborator such a skilful and accurate experimenter as my colleague Professor W. E. Dixon.

One cannot work at any subject for any length of time without speculating beyond actually observed data. It is the safest rule to keep ideas of this nature to oneself, but there are some occasions when it is permissible to speak rather more freely; this is one of them. I happen to be the unworthy occupant of a presidential chair, and for an hour or less I shall be free from contradiction and criticism, so I propose to inflict upon you during that time some speculations, and have only

safeguarded myself in the title of this address by alluding to the functions of the fluid as possible rather than probable. As my colleague Dixon is away on War Service, I have had no opportunity of discussing the matter with him, so that if I say anything that is wildly improbable he at any rate is free from responsibility

I shall dwell on the subject from the physiological rather than from the pathological standpoint, but I trust that what I have to say will be found of interest to the pathologist and the clinical observer.

May I, after this preface, proceed to another chapter, which is also of a prefatory character, and state as shortly as I can the present state of our knowledge regarding the characters, composition and fate of the fluid. The fluid, as you know is, under normal conditions, as clear as water and of low specific gravity; it contains in solution inorganic salts similar to those in the blood-plasma, a trace of coagulable protein, and a certain amount of a reducing substance which has now been definitely proved to be glucose; it is practically free from formed elements. Under various abnormal conditions the protein matter may be largely increased, or substances other than the usual ones may be added to it, such as cholesterol and choline (or a choline-like substance). It may further have added to it various kinds of cellular structures, the differentiation of which is a valuable aid in diagnosis, and in other cases parasites of different kinds may be found. The fluid is primarily formed by the secreting cells which are most prominently found covering the choroid plexuses in the cerebral ventricles, so that this structure may be appropriately designated, as Mott first suggested, the choroid gland. The fluid is normally present at a certain pressure, and this pressure is not the result of arterial pressure passively transmitted to the fluid. The cerebrospinal pressure, it is true, may be affected by changes in the arterial and venous pressures, but it is not dependent on them; and it may and often does vary quite independently of these. The true cerebrospinal pressure is the result of the secretory pressure of the choroid epithelium cells. In other words, the craniospinal contents cannot any longer be regarded as a fixed quantity without the power of expanding or contracting in volume.

The rate of flow and the pressure of the fluid can be readily investigated in animals by placing a cannula in the subcerebellar cisterna and connecting it with the necessary apparatus. It is then found that there are three groups of substances which promote the flow and increase the

pressure independently of those which affect it secondarily by altering the blood-pressure. The first group consists of excess of carbon dioxide (or lack of oxygen) in the blood, as in asphyxia, and drugs which interfere with respiration. The second group is that of the volatile anæsthetics, which may act by interfering with the respiration or by altering the physical conditions of secretion. The third group is specific, and consists of an extract of the choroid gland, or of the brain. The former is the more powerful. The chemical nature of the hormone in this extract is uncertain, but it is probably some product of nerve metabolism, which, arising in the brain, passes to the choroid plexuses, accumulates there, and stimulates the secreting cells to activity; it cannot be discovered in the normal secretion, but in cases of general paralysis and brain softening—conditions in which catabolic processes are excessive—it can be recognized (by physiological tests) in the fluid itself. The choroid plexuses are abundantly provided with nerves, but there is no evidence that these are secretory in nature; indeed the evidence derived from experiments with atropine and similar alkaloids points in the opposite direction. The hormone, whatever its nature, acts probably, not on nerves, but on the secreting cells directly.

There is no doubt that this fluid is being continually formed, and the next question is—What becomes of it? Large quantities of a neutral fluid, such as physiological saline solution, disappear within a few minutes when introduced into the craniovertebral cavity, and the course taken by such fluids, and presumably also of the normal cerebrospinal fluid, can be traced by adding some substance which can be easily recognized by its colour, or by chemical tests, or by its physiological action. Using such methods it has been demonstrated that the exit is by the blood-stream, and not by the lymph channels of the nerves, which was formerly thought to be the case by a number of French observers.

If such substances are readily diffusible the speed with which they appear in the blood is very remarkable, especially if they are introduced into the subcerebellar region. An injection, for instance, of adrenaline, nicotine, or atropine produces typical physiological actions within a few seconds, in fact, almost as rapidly as if the injection had been made into the venous circulation direct.

On the other hand, substances which are not readily diffusible (such as commercial peptone) do not produce their characteristic effects when

they are introduced into the cerebrospinal fluid, so that one of the older theories that actual valved orifices exist leading into the large veins at the base of the brain must be abandoned.

The diffusion process is most rapid in the subcerebellar district, but is extremely slow in the spinal, especially the lower spinal region.[1] The fluid probably reaches the venous sinuses by the microscopic arachnoid villi described by Weed. There is also a possibility that in addition to this, transference may take place through the thin walls of the blood-vessels within the central nervous system, for, as Mott has pointed out, contact of these vessels with the cerebrospinal fluid is maintained throughout their extent by the perivascular spaces which are continuous with the subarachnoid cavity. Diffusion in the opposite direction from blood to cerebrospinal fluid does not occur except in an almost negligible degree in the case of a few drugs, such as alcohol and urethane.

But in addition to this there is another and minor communication between the fluid and the other parts of the body. Dixon and I found that dyes added to the fluid travel along the course of certain cranial nerves, and this is especially true for the olfactory nerve. This is not the case for the spinal nerves; no dye can be detected in their sheaths outside the spinal canal, and no dye is discernible in the lymph of the thoracic duct. I have spoken of this cranial (olfactory) outlet as a minor one, but clinical experience has shown that it is not a negligible one. For this loophole affords an opportunity for the entry of infective agents, as Flexner has shown in the causation of infective poliomyelitis.

Such then is a brief and I fear imperfect summary of the present state of our knowledge, and I propose now to pass to the main object of my address, which is to discuss the meaning and functions of the cerebrospinal fluid.

In the first place it can hardly be doubted that the presence of fluid within and around the structures of the central nervous system fulfils certain mechanical functions of support and pressure. That, however, does not explain why ordinary lymph would not do just as well, as it does in the majority of other organs. The relationships, however, of the brain and cord within a closed cavity are peculiar, and it may be that

[1] This is tacitly accepted by those who produce spinal anæsthesia by the injection intrathecally of substances of the cocaine group, for they recognize the danger of absorption if the drug reaches the medullary region.

ordinary lymph is here insufficient to maintain a more or less constant pressure. For the pressure of ordinary lymph is wholly dependent on blood-pressure; here, in addition, we have an independent pressure, namely, the secretory pressure of the choroidal secreting cells, and it is therefore quite possible that this may come into play in maintaining, equalizing, and adjusting those pressure relationships which are most advantageous for the well-being and function of the brain and cord. It is further quite possible that the ameliorative effects not infrequently noticed as a result of withdrawal of the cerebrospinal fluid by lumbar puncture may be due to the relief of undue pressure. But such considerations offer no explanation of the peculiar composition of the fluid. Its characteristic chemical composition must have a deeper meaning.

It is very common to speak of the cerebrospinal fluid as the lymph of the brain. Let us now examine this phrase more fully, and see if it has any scientific accuracy.

First of all we must be quite clear as to what lymph is, and what it does. It is a fluid which exudes through the thin walls of the blood capillaries. Whether lymph formation depends solely on physical conditions (filtration and osmosis), or whether in addition we have as a factor a secretory activity of the vascular lining membrane is an interesting physiological problem, which need not concern us now. The leakage fluid resembles blood-plasma in its composition, except that it is comparatively poor in protein material, the diffusion of which through membranes is so difficult. This lymph, when formed, acts as the intermediary or middle-man between the blood and the tissue elements, conveying to the tissue elements on the one hand the oxygen and nutritive substances they need; and on the other hand it is into the lymph primarily that the tissues pour the waste products of their activity, and thus these are started on their journey to the organs of excretion (lungs, kidneys, &c.). The very essence of a lymph is that it should be in free communication with the blood-stream, except for an intervening membrane, and that this membrane should be equally permeable to water and other substances in both directions. The arrangement of the perivascular and perineuronal spaces, filled as they are with cerebrospinal fluid, certainly resembles that of a lymphatic system, and therefore it is not surprising that the idea has been accepted that cerebrospinal fluid plays the rôle of lymph in the central nervous system. The peripheral nerves have a true lymphatic system analogous to that found in other organs,

but anatomists are far from unanimous on the question as to whether the central nervous system possesses real lymphatic channels apart from the system of intercommunicating spaces occupied by cerebrospinal fluid. The mere difficulty of discovering true lymph-vessels does not indeed finally negative their existence, but until they are satisfactorily demonstrated we may provisionally assume that they are absent. If, then, cerebrospinal fluid is the only fluid which actually comes into contact with the tissue elements of the brain and cord, it necessarily follows that it must play the part assumed by lymph in other districts of the body; it must be for example the intermediary medium which is traversed by the oxygen on its way from blood to the tissue elements; and oxygen we know is essential for the continuance of nervous life and energy. Furthermore it must be the vehicle by which other nutriment reaches the cells and fibres of nervous tissue. The next point is, have we any evidence that the products of nerve katabolism pass into the fluid from the tissue elements as they do into ordinary lymph? If it were only possible to analyse and compare the composition of the fluid before it enters the perineuronal spaces, and after it leaves them, this question could be answered authoritatively, but in the absence of such proof one can only argue from probabilities. Seeing that the fluid is the only one available for the purpose, an affirmative answer seems inevitable, and this is supported by the fact that cerebrospinal fluid is rich at any rate in one waste product — namely, carbon dioxide.

The expression "lymph of the brain" is therefore so far justifiable. But we have absolutely no proof that the cerebrospinal fluid is in part an exudation from the blood, and it is in the origin of the fluid that the analogy between lymph and cerebrospinal fluid breaks down. Whatever views we may hold as to whether a secretory factor comes into play in the production of ordinary lymph (and, at the best, such a factor can only be an insignificant one), we can have no hesitation in proclaiming that cerebrospinal fluid is a true secretion arising in a definite glandular structure. Such a fluid must obviously be the best for maintaining normal life in the nerve cells, and the whole lining membrane of the spaces in which it resides appears to co-operate with the choroid gland in maintaining its constancy of composition, and to militate against the escape into it of substances from the blood-stream, such as drugs or poisons which would be foreign to the fluid or harmful to the delicate and sensitive structures which it bathes.

We have seen that the essential feature of a true lymph is the free interchange between it and the blood in both directions. This essential character is lacking in the cerebrospinal fluid; in all probability the lining membrane of the cerebrospinal spaces is permeable to substances passing from it into the blood, but it appears to be impermeable (except for oxygen) in the direction from the blood to the fluid. The nutritive materials the fluid contains appear to be formed in the choroid gland, and not to be merely exuded from the blood-stream; otherwise one cannot explain why the protein it contains is not similar to that in the blood or in exudations (lymph) formed from the blood.

If, as Weed has suggested, cerebrospinal fluid is partly formed, as is ordinary lymph, by exudation from the blood in the perivascular spaces, it is difficult to understand why readily diffusible drugs and poisons do not escape easily into the cerebrospinal fluid as they do into ordinary lymph. The difficulty is quite intelligible when we regard the choroidal epithelium as a stalwart barrier of cells which keeps back these materials, and only allows its own normal secretion to escape. This useful work would be undermined and frustrated if the general lining were easily permeable to foreign substances.

Camus found that barium chloride, which is a very active poison to the central nervous system, will kill a rabbit of 2 kgr. weight when $\frac{1}{10}$ mgr. is introduced into the subarachnoid space, whereas the lethal dose is one thousand times greater when this salt is given subcutaneously. It is well known that anaphylaxis can be produced by smaller doses of proteins introduced into the brain than when given elsewhere. The use of salvarsan in locomotor ataxy and similar late syphilitic affections *via* the cerebrospinal fluid has been abandoned, as it is fatal not only to the syphilitic organisms, but also to the patient. It is further known that salvarsan and its homologues are of little or no use in tabes and general paralysis when given by the ordinary channels, for in these later manifestations of syphilis the baneful spirochæte has got into a harbour of refuge (which we may speak of as extravascular) beyond the reach of the poison. The stalwart epithelial layer lets none escape, and does not realize how much both doctor and patient would rejoice, if it could be made to understand that in this instance at least it did not form such an effective barrier.

May I add by way of parenthesis how very desirable it is that chemists and pharmacologists should apply themselves to the solution of this difficulty. The future treatment of tabes and allied conditions should

aim at the discovery of some blander arsenic compound which could be introduced straight into the cerebrospinal fluid and kill the syphilitic organism in its lair without at the same time slaying the host who harbours it.

Is not the very simplicity of the normal fluid suggestive? I remember when I was a student that normal or physiological saline solution was regarded as a physiological fetish, for surgeons continued to use sponges (usually dirty ones at that time) soaked in water during operations. What a change has now come over the spirit of their dream! They have realized that water, even clean water, is a protoplasmic poison, and that osmosis is a real force and not a negligible phenomenon. About the same time I remember witnessing, and in a humble measure assisting, Dr. Sydney Ringer in his epoch-making work on the effects of saline mixtures on living structures. The attitude of the profession generally in those days was a tolerant one, just the sort of attitude adopted by a grandfather indulgently watching his descendants playing with their toys. But who has not heard of Ringer's solution to-day? Who can write a paper on almost any physiological or pathological subject without mentioning it or alluding to the part it has played in his investigations?

Ringer's fluid is the ideal physiological salt solution; its saline constituents are present in the same proportion as they occur in the natural body fluids, and in that way the normal osmotic pressure is maintained when living structures are placed in it. Ringer's original solution resembled frog's blood in its saline composition, but since then several modifications have been introduced. The most important of these is that which we owe to Locke, who has altered the proportion of the various salts (increasing, for example, the amount of sodium chloride from 0·6 to 0·9 per cent.), so that the fluid may be employed with success on the living tissues of the mammal. Locke has also added a small proportion of the sugar glucose, and when in use it is kept saturated with oxygen. It is well known that in such a fluid living structures can be preserved in a living condition for hours, or even days and weeks. Locke's most striking results have been obtained with the mammalian heart; this can be kept beating for prolonged periods after its complete isolation from the body if it is perfused with the oxygenated fluid. The salts supply the normal stimulus to the cardiac fibres and maintain their physical integrity; the sugar acts as a source of energy and is consumed as activity continues.

Ringer's fluid, so compounded in the laboratory, is somewhat more than thirty years old. But in reality it is as old as the hills, or rather as man himself. Just as the Venus of Milo existed potentially from past ages in the block of marble from which it was ultimately hewn, so does Ringer's fluid exist in the blood and lymph, although its simple composition is obscured there by admixture with the blood proteins and corpuscles.

Here may I introduce another parenthesis and allude to another physiological puzzle, and that is the significance of the large amount of protein in the blood-plasma and lymph. The meaning and use of the blood corpuscles we understand, or at least we think we do, but the meaning of so large a quantity of protein in the blood fluid and the part it plays in nutrition is still hidden from us. The amount of protein necessary for the repair of the tissues is not great, and the modern doctrine of maintenance and growth is that the tissues help themselves, not from proteins directly, but from the various amino-acids which are the result of protein cleavage. The great protein store may be another instance of the prodigality of Nature in providing a large margin for conditions of weakness and stress. However this may be, the presence of protein is not necessary, or it may even be harmful when it is added to Ringer's or Locke's solutions in experiments with perfused organs.

But to resume the thread of our argument. Thanks to Ringer some thirty years ago, physiologists were provided with a suitable fluid for experimental work, but the choroidal epithelium knew the secret of extracting it from the blood for untold ages before Ringer was born. For what after all is cerebrospinal fluid but Locke's modification of Ringer's fluid?

The view I have been led to take is—the nervous mechanism being so sensitive, so easily influenced by anything unusual—that therefore the neurons must be bathed in an ideal physiological saline solution to maintain their osmotic equilibrium; the trace of protein it contains is probably quite sufficient for nutritive processes, and is no doubt the kind of protein particularly suited to repair the small amount of wear and tear which is the result of nervous action. The sugar, just as in Locke's solution, would serve for a supply of energy. The choroidal epithelium, in its wise choice of a suitable circumambient medium for the neurons, is really exercising a protective function. In order to keep out harmful proteins (toxins and the like), the comparatively harmless ones are kept back also, almost completely; all share the same process

of exclusion. This protective action applies in addition to the majority of soluble drugs; this, as we have seen, may operate so as to be detrimental in diseased conditions. But we can hardly expect discrimination on the part of the epithelial secreting cells. The non-access of metallic and other poisons to the nervous elements is such a *sine quâ non* for their health, that during those periods when such substances are given for the relief of disease, or the slaughtering of parasites, the choroidal cells are unable to change their habits and therefore the drugs do not get through.

Such I believe then is the real significance of the simple composition of this remarkable secretion. But before I sit down I must allude, in conclusion, to some other possible additional functions which the fluid may exercise.

The late Dr. Gaskell approached the question from the embryological and developmental point of view; he held that the neural tube represents an ancestral digestive canal, and those who adopt Gaskell's hypothesis might conceivably argue that the cerebrospinal fluid is the representative of a primitive digestive juice, and is secreted by an organ which was formerly a digestive gland. However interesting such speculations may be, one can hardly suppose that anyone could seriously urge that the cerebrospinal fluid retains any such functions to-day.

Dendy is one of many comparative anatomists who have not accepted Gaskell's views, and he has suggested that the important function fulfilled by the choroid plexuses is that of an intracerebral gill, and is concerned in respiration. The structure of the plexuses is certainly gill-like, but it is always unwise to argue from mere anatomical resemblances. Mott, who analysed the gases of cerebrospinal fluid, found the quantity of carbon dioxide there very high (about 60 per cent.), and there is, therefore, something to be said in favour of the respiration hypothesis. It is quite possible that the choroidal epithelium may allow the escape of or even actively excrete this katabolic product, and it is unnecessary to point out how severe a poison any undue accumulation of carbon dioxide is in the central nervous system. Coupled with this fact is another I have previously mentioned—namely, that carbon dioxide is one of the most potent means of promoting a flow of the fluid. Just as urea is the best diuretic, and bile the best cholagogue, so carbon dioxide is one of the most powerful cerebral lymphagogues.

One further point and I have done. The ependyma is lined by

ciliated epithelium. Some doubt exists, I believe, as to whether the cilia are functionally active. But if we admit they are, the question arises—In which direction do they act? This is a question which is hardly susceptible of actual observation in man or the higher animals, but in the Amnocætes, which Professor Dendy has so largely used in his investigation, he has brought forward evidence which shows that the movement is in a forward direction. I know it is not always wise to draw conclusions from such lowly animals, and apply them without reserve to the higher ones. There are certain cases where such deductions are quite impossible, for instance, in some of the humble fishes the central canal of the spinal cord does not terminate blindly at its posterior extremity, but opens out by an orifice into the surrounding tissues.

In this case one can only speculate, and the rapidity of absorption of the fluid in the forward regions in comparison with that in the spinal district appears to favour Dendy's surmise. The cilia, if they are active, would no doubt promote the flow of fluid from the cord region to the large veins at the base of the brain, where it so readily leaves the subarachnoid space by entering the venous blood-stream.

There are doubtless other problems in connexion with the cerebrospinal fluid that demand solution, but my task for this evening is over. My main object has been to present the thesis in which I feel there is, at any rate, some truth—namely, that of regarding the cerebrospinal fluid as the perfect physiological medium, more perfect doubtless than the artificial fluids we can make in the laboratory, but in its essential features closely resembling those associated with the names of Ringer and Locke.

I think already I have said "finally," or "in conclusion," more than once, but you will pardon me if, like the proverbial writer of sermons, I say it a third time, and these are really my concluding sentences. These are days of specialization, but however necessary specialization in medical science may be it has its attendant evils. Particularly regrettable is the divorce between those who pursue their investigations by the bedside and those who work in the laboratory. Neurology, specially, is a branch of our science in which an attempt should be made to bring about a closer *rapprochement* between the two sets of workers. Speaking personally I have derived inestimable benefit from meeting pathologists and hearing here the clinical side of the subject. Those responsible for the management of this Section of the Royal Society

of Medicine have realized this, and in having elected this year a President who is a laboratory person they have shown that they see the value of the *entente* to which I have alluded; and I can only trust that my year of office may not be detrimental to those who form the larger contingent in the Section—namely, those who pursue more particularly the study of disease by the bedside.

Section of Neurology.

President—Professor W. D. Halliburton, M.D., F.R.S.

(*November* 23, 1916.)

(Chairman—Dr. R. Percy Smith.)

On the Relative Perceptions of Movement and a Stationary Object in Certain Visual Disturbances due to Occipital Injuries.

By George Riddoch, M.B., Captain R.A.M.C.(Temp.)

It is well known that the visual perceptions of form and colour, are to be found dissociated as a result of injury to the occipital lobes. In the records in the literature of several cases of visual defects of cortical origin, it has been noted too, that coarse or even fine movements were detected before fingers presented to the so-called blind field could be recognized or counted. Wilfred Harris [2] has given very full accounts of cases of transient hemianopia, and, in a few, movement was perceived before form or colour. Indeed, he had noticed this dissociation in cases in which the lesions were more gross and the recovery only partial. But, hitherto, so far as I know, movement as a definite visual perception has not been recognized, nor have its fields been charted systematically.

In the examination of war cases of injury to the occipital lobes, I was struck by the frequency with which movement could be detected in the affected field, before a stationary object was seen. Accordingly, I proceeded to chart both, to find out if any conclusions could be drawn from the records, and I hope to show from the cases to be quoted, which are in no way selected, (1) that movement should be recognized as a definite visual perception; (2) that it may be dissociated from

vision for stationary objects apparently completely or partially; and (3) that as vision returns, perception of movement recovers before the object can be seen.

The fields for both were recorded with a McHardy perimeter, the results being checked in two of the later cases by the Bjerrum screen. The method employed was as follows: As the slide or the disk on the pointer was approached from the periphery, as soon as the patient said he saw something moving, the chart or the screen was marked. If the square 1 cm. white disk was not distinguished at the same point, the slide or the disk was brought nearer the fixation point till it could be clearly seen, when a record was again made on the screen or chart. The moving slide was thus used as an indicator of the appreciation of "something moving," and of the disk, of the object.

I have purposely limited the disk or finger test to meaning only the appreciation of a stationary object; though frequently, the patient could simultaneously tell the shape of the disk or recognize the finger, when the rough test was used, but this was inconstant. This lack of uniformity of response and apparent untrustworthiness of the patient in appreciating the form of the test object in a part of the field, at one time and not at another, in view of the situation of the injury, was very suggestive. The same irregularity of response to stimuli is known to be typical of cortical disturbances of sensation.

The patients have great difficulty in describing what "the something moving" is like, it is so vague and shadowy; but they are quite sure that neither shape nor colour can be attributed to it, and that it can be seen in a field which is entirely blind to stationary objects.

Notwithstanding the admitted crudeness of the perimetric method, especially for the accurate recording of scotomata, I think the charts will demonstrate fairly well the points I wish to make. Where the Bjerrum screen was used to check the results, they were fully corroborated. In this series of eight cases, seven show the dissociation in the affected field, and one does not. They fall naturally into three groups.

Group I.—Showing only Perception of "Movement" in the Affected Field.

Case I.—Lieutenant-Colonel T. was wounded on April 6, 1916, by a bullet in the right occipital region while advancing into action with his men. He did not fall, and, though dazed, he "carried on." He noticed nothing wrong with

his vision at the time. About a quarter of an hour afterwards he apparently lost consciousness, for he remembers nothing further till he found himself in the Rawal Pindi General Hospital, Amara, eleven days after he was wounded. He had been trephined and the bullet removed. It was only when he was transferred to Bombay on May 17 that he noticed he could not see objects on his left. He used to miss pieces of meat on the left side of his plate, but in a good light he could tell if an object were moved in his left half field. He was evacuated for England on June 6 with no change in his vision. On roughly testing his visual fields I found that he quickly perceived finger movements in the whole of the left half field, though when the fingers were kept stationary he saw nothing; and, moreover, the field for "movement" compared with my own was normal.

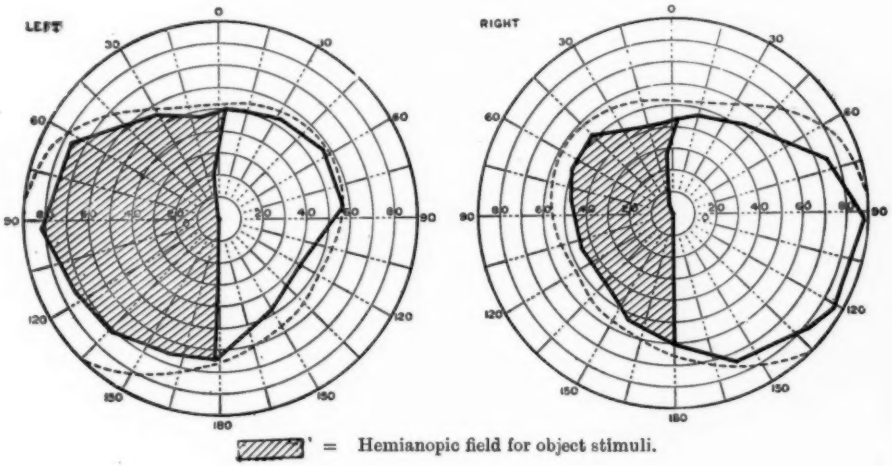


CHART 1 (Case I).

Chart 1: Central vision $\frac{6}{6}$ right and left. Three months after the date of the injury. There was a complete homonymous hemianopia up to the fixation point for stationary objects, and a full field for movement.

Charts 2 and 3, mapped out, respectively, four, and six and a half months' after the injury, showed no alteration.

The striking features of this case are:—

(1) The complete dissociation of the two types of visual stimuli in the left half field.

(2) The full field for movement. As will be shown in the charts

that follow of cases of hemianopic loss, where appreciation of movement returns, the recovery is never complete, a paracentral or central scotoma remaining as an end-result to represent the amount of irreparable damage done to the visual area. The nearest approach to a scotoma I could get by repeated examination, was a more feeble perception of movement in a small part of the upper quadrant, about 20° from the fixation point; but the patient declared, emphatically, that it was never absolutely lost.

(3) The effects of the dissociation on the patient. This vision for "a something moving" alone, introduces more difficulties into the ordinary life of the patients than when the half field is totally blind. The disparity between the sensations received from the two halves of the retina, is frequently sufficient seriously to affect equilibrium in walking. They tend usually to sway to the partially blind side, though in the cases I have seen, the disability has never been sufficient to make them fall. The disturbance is, of course, most noticeable where the original loss of vision was in the full half field.

Apart from its effect on equilibrium, the inequality of the stimuli received is often a source of considerable annoyance to the patients. Case I, for example, when travelling by train, could see nothing but vague movements as he looked out of the windows on his left, but he could read the names of the stations and appreciate the scenery from the windows on his right. This was so disconcerting to him that now he thoroughly dislikes train journeys. The perception of "motion" kept up a continual desire to turn the head to see what was moving, and the gratification of the desire resulted only in annoyance at being tricked again. The "moving things" had no form, and the nearest approach to colour that he could attribute to them was a shadowy grey.

Case II.—Second Lieutenant W. was wounded in the occipital region by a piece of bomb casing on February 9, 1916. He was knocked down, but did not lose consciousness. He could see from the beginning, though he knew his sight was affected. He had photophobia but no abnormal subjective visual sensations. At the casualty clearing station he was operated upon the same day. He was transferred to No. 2 Red Cross Hospital, Rouen, and Major Austin, on February 23, noted that there was a definite left hemianopia associated with optic neuritis. The following fields were taken a few days after his admission to the Empire Hospital and six weeks after the date of his wound:—

Chart 1: Central vision $\frac{6}{\infty}$ indistinctly, right, and $\frac{6}{9}$ left. The right and left charts differ considerably, but in both the fields for object and "movement" vision are dissociated. The dissimilarity of the charts for the two

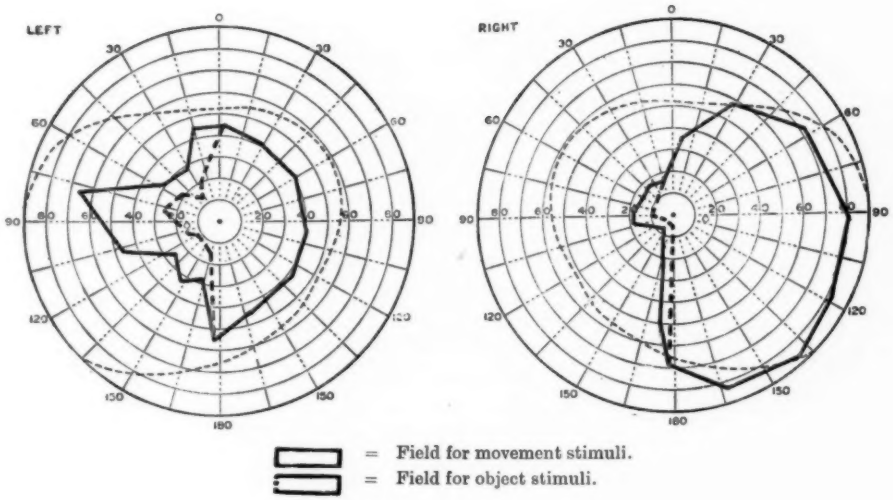


CHART 1 (Case II).

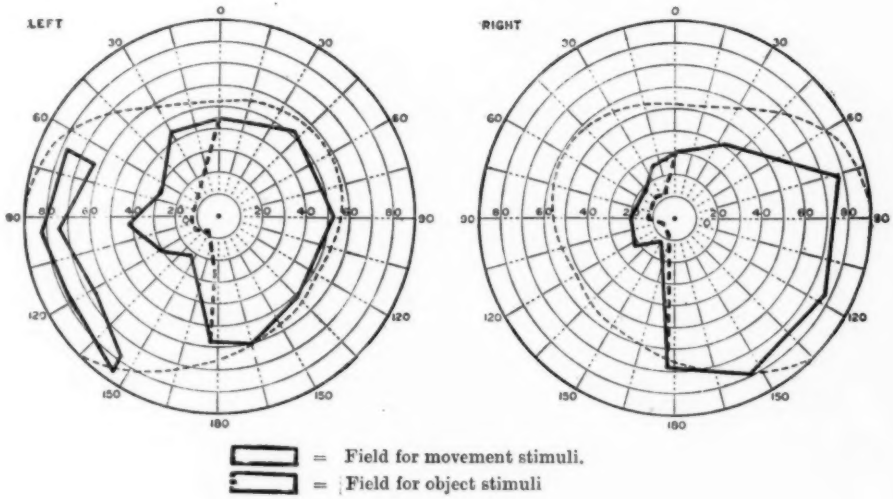


CHART 2 (Case II).

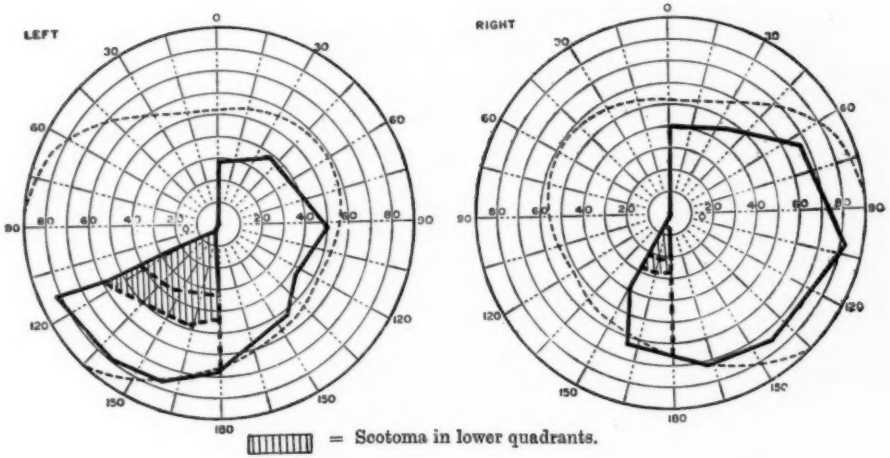
eyes made the observer doubt the accuracy of his observations, but a second record made of the fields was very similar. One notes that the dissociation is greatest in the horizontal plane through the fixation point where most vision is retained. Two months later the patient volunteered that when he was walking in the street he could see "a something moving" when people coming from behind passed him on his left side. There was, however, an area in the middle of his left half field in which it was lost. The "something" had neither form nor colour to him, and he was still as blind as before to stationary objects.

Chart 2: The perimetric charts confirmed his statements, but showed that only the left eye perceived "movement" peripherally. The fields of the right eye had not changed. Moreover, peripheral vision in the left eye was not complete, being absent in the upper and lower parts of the left half field.

The patient was soon afterwards transferred to a convalescent home, so that I have not had the opportunity of observing whether there has been any return of object vision or extension of perception of "something moving."

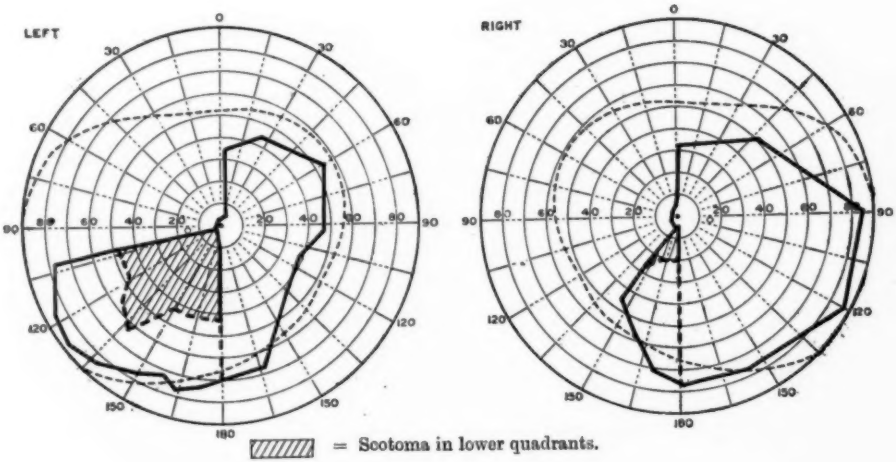
The isolated strip of peripheral vision is interesting. Harris [3] has recorded a case of partial hemianopia in which the object could be seen in a small part of the extra-central field, and he mentioned a similar case of Delépine's [1] of right hemianopia up to the fixation point, with a narrow peripheral zone of vision in the lower right quadrant of the right eye only.

Case III.—Captain de W. was wounded by a shrapnel bullet on December 4, 1915. The entry wound was in the left frontal vertex and the bullet was found, by X-ray examination, to be lying near the right occipital pole inside the skull. The patient never lost consciousness, and he was aware almost from the first that his left half fields were blind. The fields were examined at Alexandria a few days after he was wounded and a complete left homonymous hemianopia up to the fixation was reported. The patient missed meat on the left side of his plate. He noticed first some return of vision in May, 1916. "Movement" only was perceived and only in the periphery of the left lower quadrant. The patient, describing this vision in a small part of the field, said that he saw "something moving" below and to the left, momentarily, only to lose it again as it approached the centre of his field. When walking in the street he could see what he knew must represent a person's feet, though they had no shape, moving on the left, but he saw nothing to represent the body above the ankles. To Dr. Farquhar Buzzard, who examined the patient on May 31, I am indebted for the following note on the visual fields: "Vision in left upper quadrant lost. Some return of vision in the left lower quadrant." On admission to the Empire Hospital in September,



= Scotoma in lower quadrants.

CHART 1 (Case III).



= Scotoma in lower quadrants.

CHART 2 (Case III).

the patient showed symptoms of right motor apraxia. These had been more pronounced during the first six months of his illness, judging by the description he gave of his inability to perform purposive movements with his right hand. The left half field was totally blind for stationary objects, but he had become aware that the narrow area in the periphery of the lower quadrant had enlarged, for the perception of "a something moving," from the margin towards the centre of the field and upwards towards the left upper quadrant.

Chart 1: Central vision $\frac{6}{12}$, right and left, nine months after the date of the injury. The right half field is fairly complete for object and "movement." The left half field is blind to the stationary object, but "movement" is perceived in the lower part of the periphery of the inferior quadrant. This strip in the field is of larger area in the chart for the left eye.

Chart 2: Eleven months after injury. The hemianopic line is seen to miss the fixation point by about 4° or 5°. The "movement" field in the lower quadrant is now broader and longer, but a paracentral scotoma is still left, and a stationary object is not appreciated in any part of the left half field.

The perimetric results of this case have been checked by the Bjerrum screen. He exhibited other interesting visual phenomena, which, like the dissociations, can be compared to some of the sensory defects from cerebral injuries, described by Head and Holmes.

(1) The ability to localize objects seen was disturbed. If a matchbox was held in front of him, though he could bring it at once into central vision, his hand wandered all round in search of it before the box was finally grasped. The difficulty was the same whichever hand was used. The right arm was practically as strong as the left, and only slight loss of sense of position could be found. He always succeeded in the end in finding the thing he wanted, but he had to grope for it. For a long time after he was wounded he was frequently unable to get his eyes fixed on an object, even though he knew quite well its position in space—e.g., his hand.

(2) He was unable to estimate correctly relative distances and lengths. On a piece of paper held flat on the table before him two large dots 4 in. apart, one nearer to him than the other, appeared to be equidistant from him. Their relative positions were, nevertheless, easily recognized when he was allowed to touch the dots with his left forefinger. His answers were more correct if the dots on the paper were side by side, or one above the other. A tall and a short man standing side by side were to him about the same height. He was able, often, to judge with a fair degree of accuracy, the height or breadth of an object, but occasionally, he made gross mistakes—e.g., sitting in a

chair 3 ft. away from me, he thought we were separated by at least 6 ft. Again, a room 4½ yds. broad, was estimated by him to be 10 yds. in breadth. The next minute he gauged correctly the length of a stick and the breadth of a table. His answers were irregular and lacked uniformity.

(3) The stereoscopic element of vision was gone. Everything he saw was flat. People had the appearance of cardboard figures; they had outline, but no depth. If two persons were standing in front of him, he could differentiate them only by their outlines, for they were featureless and had no rotundity. Their noses might have been painted on their flat faces. Friends were recognized only by their voices. A landscape was like a piece of stage scenery. Trees, hills, everything he saw, were at the same level; and yet he could recognize light and shade. A ball was simply a circle, an egg an oval, and a box a rectangle. This phenomenon was evident only if the objects were more than 1 ft. away from him.

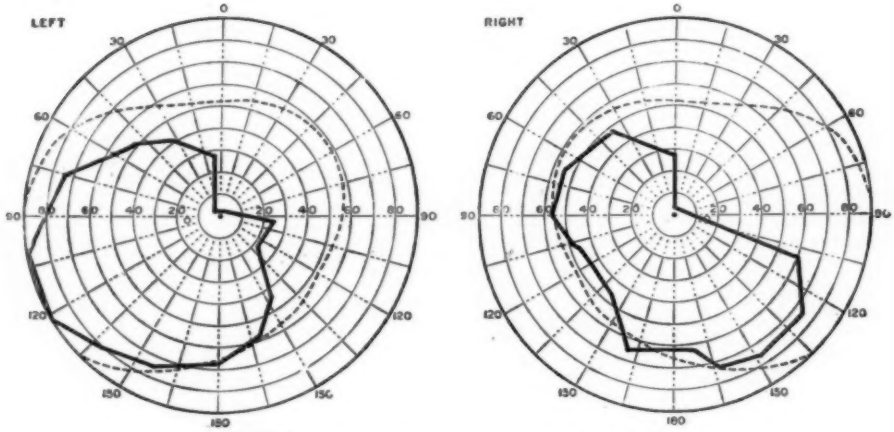
Case IV.—Captain C. received a wound at Gallipoli by a shrapnel bullet on June 6, 1915. He was hit in the left occipital region while advancing; and though dazed, he did not lose consciousness at once. He got up and was able to walk back part of the way to the dressing station before he fell down unconscious. He had noticed nothing wrong with his vision. I examined his fields first on September 28, 1915. Though he was quite aware of his blind upper quadrant, the visual loss hardly inconvenienced him at all. He had no appreciation of movement or other visual stimuli in the blind part of the field.

Chart 1: Central vision $\frac{6}{6}$, right and left. The charts show that the right lower quadrants are also restricted at the periphery. The movement and object fields coincide, and the visual loss reaches the fixation point.

A record of the fields was again made on November 18: There was still no dissociation of the two stimuli. The peripheral restriction of the right lower quadrants was greater. He complained of more difficulty in getting about, and of being more blind to the left.

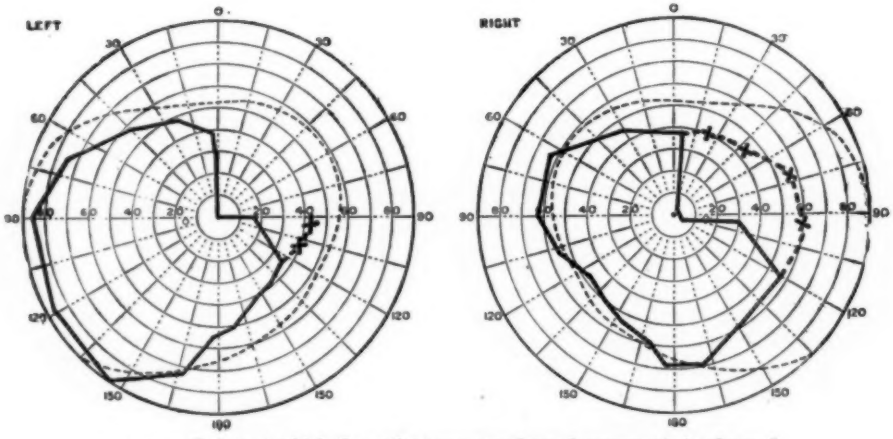
Chart 2: Fields mapped out on December 13, six months after injury. The right lower quadrant field has filled out, and at the points marked ×, he sees "a something moving" just for a second. These points complete the periphery of the field in the right eye chart, but correspond in the chart of the left eye only to the periphery of the defective part of the right lower quadrant. The patient declared that he could not distinguish any object as the slide went past, but that he knew something had moved through his blind field. Vision was obviously recovering in the periphery.

On February 1, 1916, he was admitted to the Empire Hospital for observation. Chart 3 shows his fields for the perception of "movement" and object, taken three days afterwards.



▭ = Fields for movement and object stimuli.

CHART 1 (Case IV).



+ + = Points at which the patient was conscious of movements to a second.

CHART 2 (Case IV).

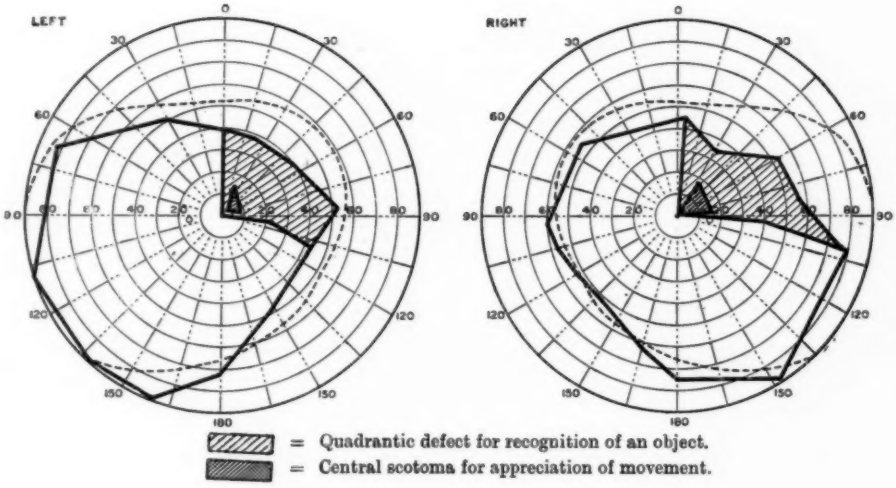


CHART 3 (Case IV).

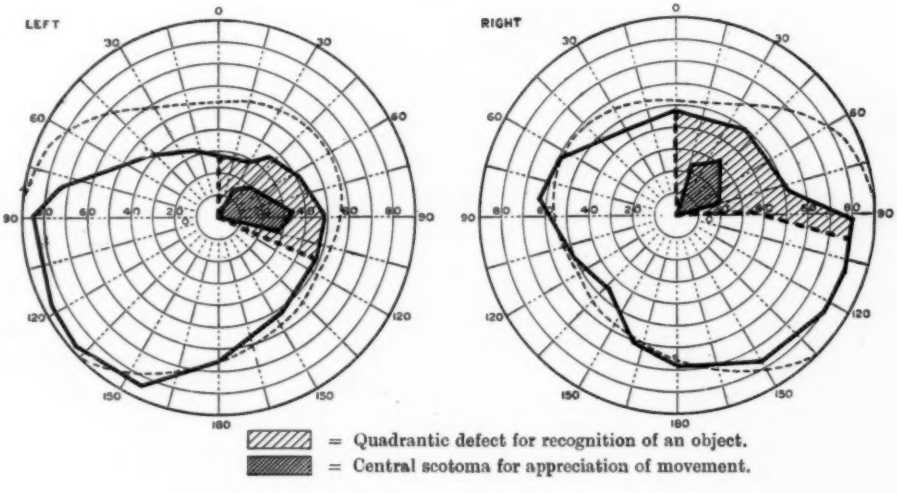


CHART 4 (Case IV).

Chart 3: Except for a slight restriction of the peripheral parts of the right upper quadrant, more especially of the right eye, and a triangular central scotoma for movement, in the original blind field, the field is complete for movement, and has not changed for object vision.

The patient was discharged a few days after to the country, and there was no opportunity of making a further record till July 10, when he presented himself complaining that his vision was not quite so good.

Chart 4: The field for movement has altered little, except that the scotoma is a little larger; and a complete absence of object vision, up to the fixation point, is demonstrated.

The fields were again recorded thirteen months after he was wounded. There was still no sign of return of object vision in the upper quadrant, and the scotoma for movement has increased in size.

The prominent features of this case are:—

(1) The return of perception of movement alone.

(2) The recovery beginning in the periphery of the original blind field, leaving as an end-result, a central scotoma.

Case V.—Captain H. was wounded on December 16, 1915, by shrapnel in the occiput. Dazed but not unconscious, he realized he was completely blind. They carried him back to the field dressing station, where his wound was attended to. The medical officer at No. 2 Casualty Clearing Station reported that he was admitted there the same night with a serious gunshot wound of the vault, which was bleeding freely. He was unconscious and in a critical condition. The following day the wound was cleaned, and some fragments of bone which were found to be damming a torn longitudinal sinus were removed. On January 10, he was transferred to No. 1 Red Cross Hospital, Le Touquet, for better nursing and comfort. The first note made on vision was dated January 28: "?Very slight vision right eye; blurring of left disk." On admission to the Empire Hospital on February 26, he was very confused and moaned continually. There was a large sloughing septic hernia protruding from the back of his head, and to all appearance involving the whole of the occipital lobes. The parietal bones were loose, and could be moved under the scalp. On the following day he was quite collected, though suffering from pain in the head and neck, and to rough testing he was totally blind. On March 28 the patient said he could see something moving on his right. The stimulus proved to be his wife's shadow on the wall. When tested, he could distinguish between light and darkness, and could detect finger movements in the upper part of the periphery of the right superior quadrant. Stationary objects, form and colour were not perceived. He was totally blind in the rest of the field, and no attempt was made to chart the seeing parts, as it was naturally impossible for him to fix. Later when he began to get out for walks, he noticed that he could detect movements as he passed objects—e.g., lamp-posts, on his right side. He said, "they don't appear to have colour or

shape. They look like shadows. Sometimes I can tell if the moving things are white." On November 4 his vision had not altered. He was aware of movement in the same limited part of the field, but he had no idea of shape or colour, and he could not count fingers.

The damage to the occipital lobes was obviously very extensive and deep from the appearance of the hernia; but the case is of interest in so far as it shows that what remained of vision was for movement and light stimuli only.

Group II.—Recovery of Both Perceptions, though in Different Degree.

Two cases are included in this group, the first showing dissociation and partial recovery of vision for object and movement stimuli in concentric contraction of the fields, and the second in quadrantic defect.

Case VI.—Lieutenant H. was wounded by a bullet in the occipital region on July 13, 1915. He was unconscious for three days, and during that time he had been operated on at Merville. From the scanty information on th transfer sheet I gathered that the entry wound had been enlarged, and that the bullet, which was lying well forward between the occipital lobes, was removed along with a few small pieces of bone, some of which had followed the bullet, and some had remained superficial. The patient, who had sensory and motor loss in both legs below the knees, noticed nothing wrong with his vision for about a month; but during that time he had been kept in bed. His difficulty was in seeing things to the right and on the ground. When he walked he stumbled over objects as high as his knee, unless he kept his eyes fixed on the floor. He had the same difficulty in avoiding things situated on his right side. His fields were first charted for movement on October 5, two months after the injury.

Chart 1: Central vision $\frac{6}{9}$, right and left. The defect was shown to be mostly in the lower quadrants, though the right upper quadrant was also restricted. He was discharged on October 30, and while on leave the sinus reopened. He was again admitted to the Empire Hospital on January 21, 1916, complaining of nausea in the morning and occasional frontal headache and dizziness. No vomiting. He did not think his eyesight had got more restricted, but he noticed the defect more. The fields for movement were charted four days after admission, and the loss was found to be, if anything, a little greater, especially in the lower quadrants.

Chart 2: On February 9 the fields were again mapped out, and they showed a marked general restriction, most pronounced in the lower quadrants and more especially in the right. Vision for stationary objects and for movement

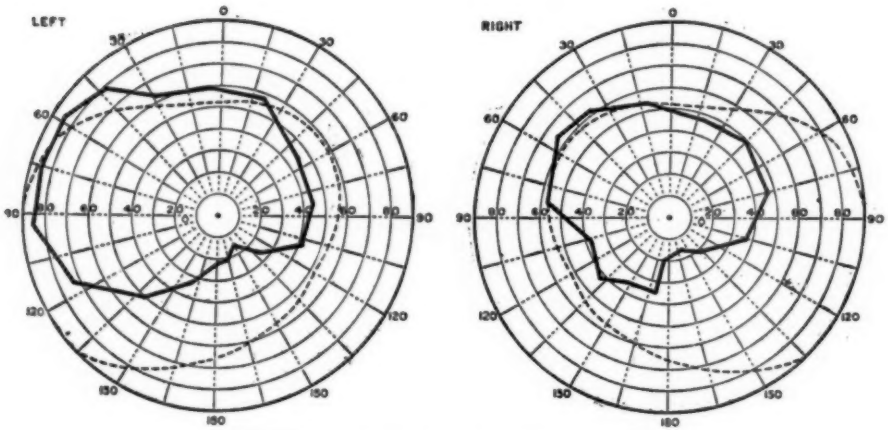


CHART 1 (Case VI).

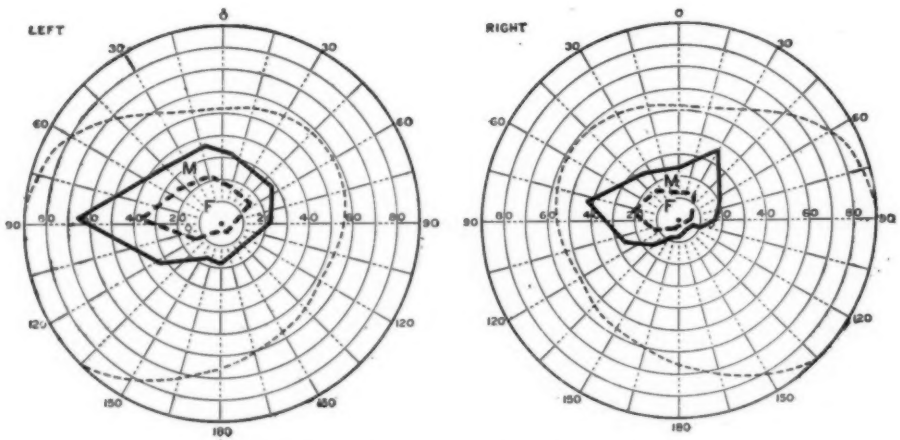


CHART 2 (Case VI).

were completely dissociated, the movement field being the larger. The patient had practically telescopic vision. A radiogram revealed two small pieces of bone, lying between the hemispheres, and on February 3 Mr. Walton operated and removed them. Six days after operation the fields for movement and object, on being taken, were found to have enlarged considerably, and the increase was relatively about the same for both perceptions.

When he was discharged on April 26 the fields were *in statu quo;* but as he had got accustomed to move his eyes about more freely his visual defect disturbed him much less.

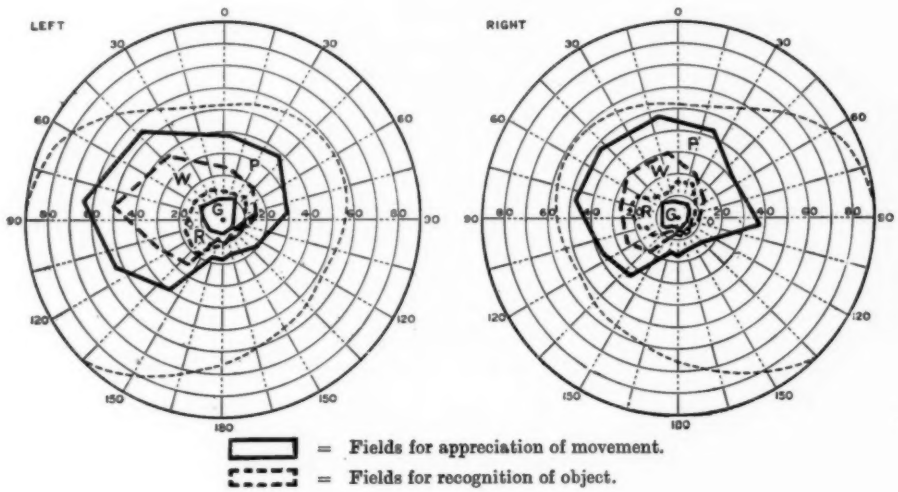


CHART 3 (Case VI).

This case shows that dissociation of movement and object perceptions occurs not only in quadrantic and hemianopic defects, but also where the fields are generally restricted.

Case VII.—Captain R. was wounded on July 25, 1916, in the occipital region, by pieces of shrapnel casing. He was walking at the time when the shell pitched behind him. He fell down, but was not unconscious. He could move all his limbs, but he noticed his vision was gone in both lower quadrants. He walked back to Battalion Headquarters, a distance of three miles, but to prevent stumbling over obstacles he had to keep his eyes fixed on the ground. After having had his wound dressed by the medical officer, he walked three miles to the field ambulance, experiencing the same difficulty with his vision.

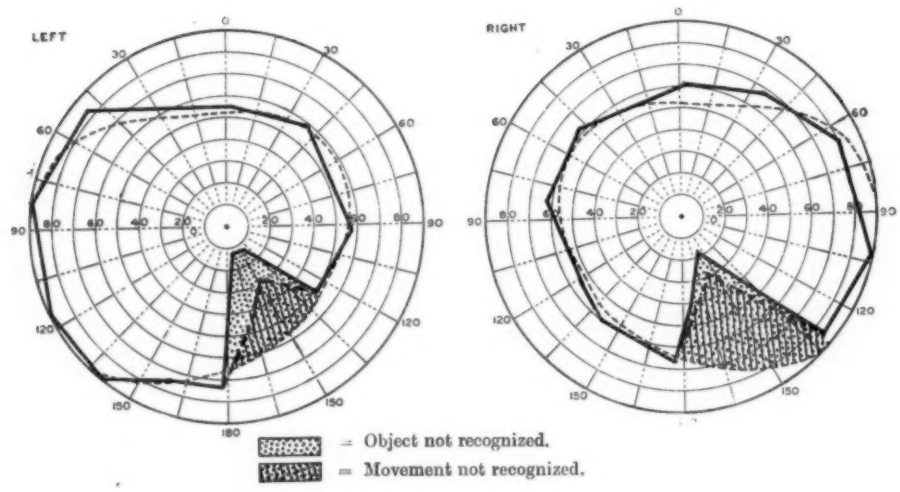


CHART 1 (Case VII).

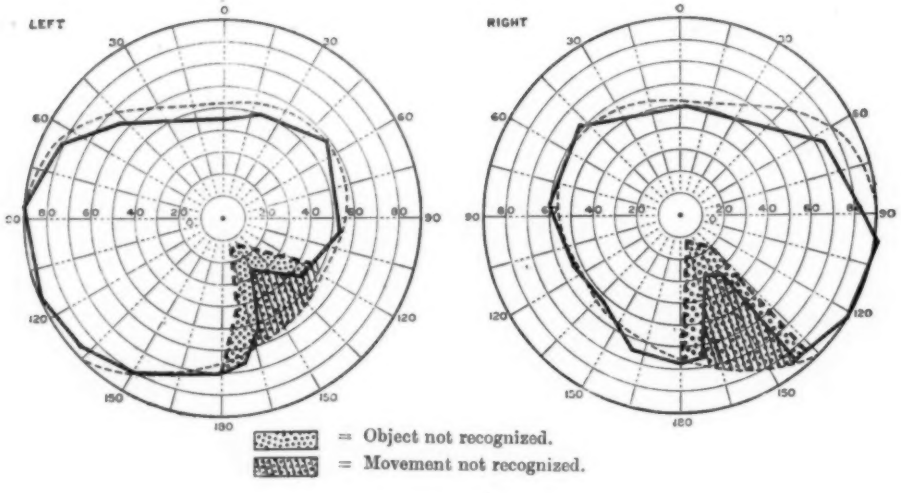


CHART 2 (Case VII).

He was "gassed" on the way, though not badly, the effects passing off in about twelve hours. During the eighteen hours he remained at the casualty clearing station no improvement in vision occurred. He was sent by train to No. 2 Red Cross Hospital, Rouen, where he was operated upon by Major Austin on July 28, who reports: "Two foreign bodies against the skull; depressed fracture below, situated a little to the left of the mid-line; trephined; dura cut, but not through, just over longitudinal sinus. No hæmorrhage. Wound excised: now slightly septic." A week after the operation the patient first noticed some return of vision in the left lower quadrant. He is quite certain that recovery began in the peripheral field, and it was first for light and movement only. He could not see stationary objects. The improvement in the left lower quadrant went on till the whole quadrant recovered for all stimuli, light and movement being the pioneers. Movement, he was certain, came back before stationary objects, form, or colour could be perceived. On admission to the Empire Hospital on August 6 his wound was still septic. On roughly testing the visual fields with fingers, I found that part of the right lower quadrant was defective. The loss, which was more for stationary objects than for movement, did not reach the fixation point. Charts were taken on August 26, one month after the date of the injury.

Chart 1: The apex of the wedge in the inferior part of the quadrant reached to between 10° and 20° of the fixation point. Dissociation of the fields for stationary objects and movement was present, the former being the smaller. The left lower quadrant had completely recovered. The fields were again recorded on November 4.

Chart 2: A little improvement had occurred in the fields for both perceptions, more noticeable in the field for movement; and the filling out of the fields was more at the periphery, so that the base of the wedge was narrower. Colour stimuli were not perceived in the blind field. He was more conscious of bright than of dark objects.

Group III.—Where no Dissociation or Recovery of Vision occurs.

Certain cases of hemianopia, however, show no dissociaton of object and movement vision, and remain stationary, at all events, for many months. But I have found them to be relatively few in number, and only one occurs in this series of nine cases. One might suggest tentatively, that coincidence of the fields and absence of any sign of recovery, are due to the injury being more extensively subcortical. I cannot bring forward pathological material to prove this assertion, but Swanzy, writing on relative hemianopia, said that he believed it was the result of a lesion of less intensity than that which caused absolute hemianopia. The following case belongs to this group:—

Case VIII.—Lieutenant M. was wounded at Loos about September 30, 1915, by a rifle bullet in the left occipital region. On admission to Rawal Pindi Hospital the next day he was in a semi-conscious condition, quite unable to give the name of his unit or his people's address. Later he demonstrated to the doctor his right hemianopia during a fairly lucid period. On November 2 he was admitted to the Empire Hospital. The fields were first recorded on November 13, a right homonymous hemianopia up to the fixation point being found. There was coincidence of the fields for the perceptions of object and movement.

Chart 1: Central vision $\frac{6}{6}$, right and left. Charts were taken at intervals till June, 1916, and no change in the fields could be detected.

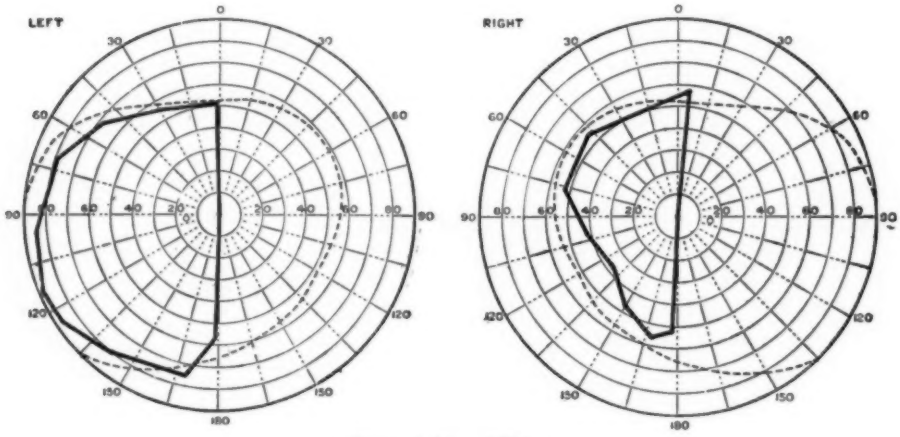


CHART 1 (Case VIII).

This series of cases more fully described will be incorporated in a paper in *Brain* on "Certain Visual Dissociations due to Occipital Injuries."

CONCLUSIONS.

I have attempted to demonstrate from cases of visual defects of occipital origin, (1) that the consciousness of "a something moving" should be recognized as one of the visual perceptions; (2) that it may be dissociated from the perception of a stationary object; (3) that where recovery of vision occurs, the perception of "movement" precedes that of the object; and (4) that recovery of "movement" vision begins at the periphery.

In investigating visual disturbance due to occipital injury, I have been deeply impressed by the points of similarity which these disturbances present with other sensory defects associated with damage to the cerebrum. Vision is a form of general perception, and I feel that much valuable information might be collected, and that a wider conception of what vision is might be obtained, if it were considered not as a special sense, but as a part of general sensation.

The visual apparatus belongs to the afferent sensory system, and conveys sensory impressions. Vision, indeed, is highly specialized for the performance of particular functions. Physiologically, however, sensory activities appear to be governed by certain laws, and one might suppose that an interference with the visual paths would give rise to results comparable to what obtains when other sensory mechanisms are disorganized. It should be possible to compare defects of vision and of sensation from lesions at corresponding physiological levels. I will be content, however, with merely mentioning a few of the visual defects which I think resemble disturbances of sensation.

(1) Dissociations of primary visual perceptions of light, movement, stationary objects, form and colour.

(2) Inability to localize an object seen, and to estimate its length.

(3) Inability to appreciate "difference"—relative lengths and distances.

(4) Inability to distinguish between a flat disk and a sphere.

(5) Irregularity of response to stimuli. This apparent untrustworthiness of the patient has been described by Head and Holmes [4] as being a typical feature of cortical disturbances of sensation.

My grateful thanks are due to Dr. Farquhar Buzzard, Dr. Collier, Dr. Leonard Guthrie, Dr. Head, and Dr. Kinnier Wilson, for permission to publish my notes on their cases and for kind advice.

REFERENCES.

[1] Delépine. *Trans. Path. Soc. Lond.*, 1890, xii, p. 2.
[2] Harris, Wilfred. *Brain*, 1897, xx, p. 307.
[3] *Idem. Ibid.*
[4] Head and Holmes. *Brain*, 1911-12, xxxiv, p. 189.

DISCUSSION.

Dr. LEONARD GUTHRIE: I wish to congratulate Captain Riddoch on the excellence of his paper. As he himself mentioned, the fact that recovery of perception of movement may precede that of stationary objects, has already been recorded. But he is the first to draw attention to the significance and importance of the observation. He suggests that this dissociation between perception of movement and of stationary objects may be analogous to the dissociation which is known to occur in other forms of sensory disturbance, and that a similar dissociation may be found in the case of special senses other than the visual. I would now suggest that dissociation is always between the more crude and primitive functions and those which are more elaborate and later acquired. It is well known that in cases of gross cerebral lesions recovery occurs sooner and more completely in the case of simple functions than in those more specialized. My contention is that perception of movement is the most rudimentary form of vision, and is necessary for the preservation of lower forms of life. Many examples drawn from the animal kingdom show that the perception of movement serves to attract the prey to the captor and is excited by the captor in order to catch its victims. The cerastes viper for instance lies half buried in the sand and keeps its horns in motion in order to entice small animals within its reach. The Indian hunter is said to draw deer within gunshot range by waving a rag on the end of his ramrod. Some years ago there was an outcry in the public Press against the cruelty of feeding the snakes at the "Zoo" on live animals. The answer was that many kinds of reptiles will not touch their food unless it is alive and moves. As a boy I used to keep lizards as pets, and I found that they would walk over or by dead flies and other insects, without taking the slightest notice of them, but if I introduced living insects, the lizards would see them at once and dart from one end of the cage to the other in order to seize them. I could, however, train them to eat dead insects when I impaled the insect on a pin at the end of a stick and waved it about at a distance from the lizard. Many inhabitants of the marine world are provided with tentacles and antennæ, by movement of which they induce other creatures to come within reach in the hope of obtaining a meal for themselves. On the other hand, the python will lie coiled on a tree by the waterside, motionless and invisible to the deer which comes to drink. We have all seen a cat squatting immobile for hours before a mouse hole. If the cat moved the mouse would see it and would never be foolish enough to emerge from its hole and get caught. Certain birds, notably the quail, and insects are said to feign death when in danger. But the quail or insect is like Wordsworth's "simple child." "What should it know of death?" It is, however, quite probable that instinct leads it to become a "stationary object" and thus escape detection. I have made no special investigations in the case of human beings, but every one knows that young

infants will turn their heads and eyes towards a moving body brought from the periphery within their range of vision, whereas they may take no notice of stationary objects held before their eyes. Amongst the many interesting observations in Captain Riddoch's paper are two to which I would refer. One is that in some cases of occipital lesions, perception of movement occurs first at the periphery of the fields. This is exactly as might be expected. It is obvious that perception of movement will be the more useful in proportion to the width of its range towards the periphery. Also, the recovery of perception of movement from the periphery inwards, seems in favour of the view that the peripheral vision is a crude and primitive form, and therefore returns more readily than more specialized visual functions. The other interesting point is that one patient recovered vision for objects, but complained that they all looked flat. He could not see any difference between a sphere and a flat disk. In other words he has lost the appreciation of gradations of light and shade which enables us to make such distinctions. He sees objects flat, as they appear in a brilliant searchlight or in "stageland," where shadows are eliminated by the intensity of the illumination. This again implies a return of vision which is nevertheless rudimentary and decadent. He sees objects flat just as they appear to the aborigine, the young child or the "post-impressionist" who attempts to depict them.

Mr. J. HERBERT PARSONS: This paper opens up a field of far reaching importance, not only from the biological standpoint, to which Dr. Guthrie has so ably drawn attention, but also from many other points of view. It is customary in physiological and ophthalmic text-books to divide visual sensations into the light sense, the colour sense, and the form sense. This has always seemed to me a convenient but not very scientific division, but if it is accepted the sense of movement has just as much (or as little) right to be included as a fourth division. The crudity of the classification is best seen on considering threshold values. Thus, Charpentier's beautiful experiments have proved conclusively that at the threshold the minimum light appreciable is a function not only of the intensity of the light but also of the area of retina stimulated. So far as the macula is concerned, where adaptation is negligible, the relation of intensity to area follows a quite definite mathematical law. As regards colour, it is legitimate, with Hering, to include under this designation both toned and untoned colours, and in any case it is obvious that colour without light is unthinkable. The foundation of the form sense is the appreciation of two points of light as discrete points, and here again the threshold values are dependent entirely upon the light sense, modified or unmodified by a toned colour constituent. The visual appreciation of movement is a function of light, colour and form, and from the physiological point of view it is impossible to dissever these constituents. Hence I think that Captain Riddoch emphasizes too strongly his distinction between appreciation of movement and that of form. So far as the highest cortical representation is concerned I agree that there is representation of movement as such, almost

entirely divorced from appreciation of form, colour, and even light: but this is because I regard the highest cortical centres as the physical basis of psychological phenomena, and therefore of that analysis of precepts and selective re-synthesis which creates the concept, in this case—movement. From the biological point of view the appreciation of movement is primitive. Here the inchoate mass of dimly perceived sensations doubtless arouses the mere impression of "something moving." With the differentiation of the sense organs, and notably of the projicient sense organs, as has been so admirably demonstrated by Sherrington, there proceeds *pari passu* a differentation of the inchoate mass of sensations, an analysis or selective segregation, which permits of the separate appreciation of light, colour, and so on. Yet the analysis appears never to be complete, and it is, moreover, accompanied by re-syntheses which are the basis of a scientific, as opposed to a metaphysical, creative evolution.

Section of Neurology.[1]

President—Dr. W. D. Halliburton, F.R.S.

(*January* 25, 1917.)

Demonstration of Cases.

By G. Newton Pitt, Major R.A.M.C., M.D.

Case I.: Fracture of Spine; Paraplegia, with Sweating below the Level of the Lesion.—P. Fracture of spine: fracture dislocation of lower dorsal region, December 25, 1915. When admitted on February 24, 1916, there was partial loss of sensation to umbilicus, with some muscular power in leg. A suprapubic operation had been done in Boulogne. Knee-jerks both present. Plantar reflexes flexor and, later, extensor. Operation, March 29, 1916 (Colonel Armour): Removal of fractured lamina of dorsal vertebra and separation of adhesions. Patient has gradually recovered power; on September 14 his suprapubic opening closed, and now he has good control of urine, although the wound has broken down three times. There is marked sweating below the level of the lesion, and the levels of hypo-æsthesia and sweating are identical; the sweating is sufficient to keep his bed constantly wet. Sheets have to be changed four times daily, and blankets twice daily.

Case II.: Cerebral Hernia, following Bullet Wound.—C. Wounded by rifle bullet on October 1, 1916, in right parietal region, 3 in. above by 4 in. behind external auditory meatus. There was a depressed fracture and pieces of metal were removed with small trephine opening, October 3, 1916. Condition on admission: Hemiopsia (left), no motor nor sensory signs. There was a cerebral hernia with septic

[1] Clinical Meeting held at King George Hospital.

granulations. November 9: Pain, vomiting, and increase in size of hernia. Colonel Armour enlarged trephine wound and drained a small abscess, with relief resulting. December 3: Rise of temperature, headache, and stupor; hernia very much larger and an escape of a quantity of cerebrospinal fluid, which continued till December 26. Wound now nearly healed, and hernia has disappeared.

Discussion is invited as to what extent the size of large herniæ is due to distension of the lateral ventricle.

Case III.: Aphasia, following Cerebral Injury by Shrapnel Bullet.—J. Wounded by shrapnel on October 10, 1916, in right external frontal process; a fragment of metal is lodged 2 in. above the inion. The patient was aphasic on admission; his reading was gibberish. He had auditory aphasia and did not understand all that was said to him. No motor nor sensory symptoms; he was dull, heavy, and incontinent, both of urine and fæces. His fields of vision are normal, and there is no optic neuritis.

The point of interest lies in the occurrence of aphasia, considering the course the bullet took.

(*January* 25, 1917.)

Demonstration of Cases.

By J. S. Collier, Captain R.A.M.C., M.D.

Case I.: Abortive Poliomyelitis.—V. This man was admitted to King George Hospital on November 17, 1916. He was on leave from Lark Hill. Temperature, 103°F., pulse 96, respiration 24. He complained of severe headache and stiffness of neck, but there was no vomiting nor other signs; all reflexes were normal. As several cases of cerebrospinal meningitis had occurred at this camp, lumbar puncture was performed. There was no increase of pressure, and the fluid appeared clear. Cell count gave 1,250 mononuclear cells per cubic millimetre, but was sterile on culture. Temperature lasted for a few days, and except for slight left facial herpes nothing else occurred. Wassermann reaction was negative. No physical signs in chest, but X-ray suggests

old tubercle of lung. Lumbar puncture, January 17, 1917: Cerebro-spinal fluid not under pressure; fluid clear and cell count normal.

Case II.: Myasthenia Gravis.—B. This man was shot through the upper jaw in November, 1914; at the same time he received an injury to the left eye. In September, 1915, he noticed that he stumbled while carrying his pack and fell on his knees after walking a short distance; after rest he could go on again. This disability upon exertion gradually increased, and he had difficulty in raising himself from the lying to the sitting position. More recently the same paralysis on fatigue has appeared in proximal muscles of upper limbs. There has never been any sign of ocular, facial, buccal, or pharyngeal weakness. There is great weakness of all the muscles of the trunk, with attitude and gait resembling that of myopathy, the shoulders being thrown back with relative lordosis, and moving from side to side with each step. He is unable to regain his feet from the supine position without help, and in making the attempt goes through the well known movements characteristic of weakness of the trunk. On electrical examination the myasthenic reaction is well marked. Pupils react to light and convergence. There is no wasting of muscles. Deep reflexes are present.

Case III: Spinal Injury; Paraplegia in Flexion, with Pressure Palsy of External Popliteal Nerve (Right).—F. Gunshot wound of the spine, eighth dorsal vertebra, on July 31, 1916. Admitted to King George Hospital on August 10, 1916. The condition is one of complete motor and sensory paralysis in flexion. The point of interest is that there is atrophic palsy, with loss of sensibility in right anterior tibial and peroneal muscles, with a drop foot and dropped toes and a flexor response on left side, with a retracted foot and extensor response on opposite side.

The point on which discussion is invited is the cause of this peripheral nerve or root lesion. The fifth lumbar area on the left side is not reflexogenous, nor is it clearly so on the right.

Case IV: Spinal Cord Injury, complicated by Root Lesions.—D. Injury from being buried in dug-out on October 1, 1916. Patient was unconscious for twelve hours. Complete paralysis in both legs for five days. Retention of urine with overflow for two days. There is

an angular curvature at the junction of the dorsal and lumbar region, the most prominent part of angle being the twelfth dorsal spine. There is a separation of 1¼ in. between the eleventh and twelfth dorsal spines, and there does not appear to be any lateral displacement. There is some weakness of the lower lateral abdominal muscles, as shown by bulging of the lower abdominal walls on sitting up, or by cough, which is improving. Right leg normal; left leg shows fair power in all movements, but rather less than the right, except in the peroneals and extensors of toes, which are almost completely paralysed. The tibialis anticus is weak, but contracts more powerfully. The faradic excitability of these muscles is much diminished. Knee-jerk and ankle-jerk on the left side are clearly increased. Rectus and ankle clonus are present; flexor plantar response. Vibration felt well and equally both sides; no loss of sense of position in either leg. There is well marked diminution of pain sensibility over left half of trunk and left lower limb below level of nipple. No loss to touch; heat and cold are not altered. There is much deeper relative sensory loss to all forms over areas of twelfth dorsal, first and second lumbar roots, and over fifth lumbar and first sacral roots; full control of sphincters. The trunk reflexes are much diminished below the level of the nipple on the left side and below level of umbilicus on right side. Cremaster absent on both sides. Deep reflexes in right leg are normal. There is apparently a lesion of the left pyramidal tract, giving rise to spastic signs in left leg, and lesions of the roots, twelfth dorsal, first, second and fifth lumbar and first sacral, the latter lesions preventing the exhibition of extensor response.

The sensory loss is a subject for discussion.

Case V.: Paraplegia with Sweating below the Level of the Lesion. —S. Gunshot wound of the spine. Wounded August 29, 1916. Complete paralysis and loss of sensation below eighth dorsal vertebra, with knee-jerks and plantar reflexes retained. Shown because of the marked sweating below the lesion, with sweat rash.

(*January* 25, 1917.)

Gunshot Wound Producing Damage of Twelfth and Spinal Accessory Nerves, with Cross Union of their Fibres.

By Sir WILLIAM COLLINS, M.D., M.P.

LANCE-SERGEANT M., aged 35. Patient was wounded on March 8 by bullet which entered on right side of neck, escaping through left orbit, the left eye being shot away. He was a prisoner of war at Bagdad till August, 1916. Operated for resection of left eyelid on December 20, 1916. Examined by Dr. Buzzard on December 29, 1916, who reported as follows: After being wounded on March 8, 1916, patient was unconscious for a short period, and found he could not raise his right arm beyond a certain distance; unable to phonate or articulate properly. Has gradually recovered power of articulation and phonation, but articulation interfered with whenever he throws his right trapezius or sternomastoid into action. Tongue atrophied on right side and protruded to right. When spinal accessory muscles are thrown into action he is conscious of a spasm in the tongue drawing it more to the right. With violent exertion of these muscles defect in articulation becomes extreme. No obvious wasting nor loss of power in trapezius or sternomastoid, though when at rest they stand out with a little increase of tone compared to those on left side. What is the condition of the larynx? Probably some right-sided palsy at one time. The palate moves well. No paralysis of other parts, no other exertion except that involving trapezius and sternomastoid which interferes with articulation. Dr. Dundas Grant's report (January 17, 1917): Immobility of right vocal cord slightly external to mid line (cadaveric position), complete recurrent laryngeal palsy. Palate moves well. Nasopharynx normal.

(*January* 25, 1917.)

A Case of Pituitary Tumour.

By H. S. Clogg, M.S.

W., aged 42. Pituitary tumour. Admitted to King George Hospital on November 26, 1916. Past history: Five months history of (1) severe frontal headache; (2) failing sight; "things looked black"; (3) feeling drowsy; (4) putting on weight, loss of sexual desire. He was doing sanitary work at Boulogne at the time. Condition on admission: Rather drowsy; facies "Napoleonic," double chin, large supra-clavicular pads of fat; thyroid gland not felt; complains of failing sight; says this is getting worse; also of frontal headaches, which awaken him at night; he cannot get about or read much on account of his sight failing. Eyes: Both optic disks pale. General atrophy throughout. Left eye white and more atrophic than the right. Temporal hemianopia more marked in left eye. Loss of central vision in right eye. Wassermann reaction positive on December 6, 1916. He is having iodide and mercury. Galyl, 30 cgm., given on December 8, 1916, and January 1, 1917. Wassermann reaction positive on January 17, 1917.

(*January* 25, 1917.)

Complete Division of Spinal Cord with Return of Reflex Activity below the Lesion.

By E. Farquhar Buzzard, M.D.

Patient wounded by shrapnel while lying down on July 21, 1916. Bullet entered the back of chest at inner border of left scapula at level of fifth dorsal spine and lodged itself in the vertebral canal between the fourth and fifth dorsal vertebræ, causing immediate and

complete paraplegia. Admitted into King George Hospital on August 6, 1916, with complete loss of sensation and movement and reflexes below the level of the nipples. Severe cystitis. August 25: Plantar stimulation elicited extensor responses in both feet. September 11: Laminectomy performed by Colonel Armour. A large round lead bullet was found lying in the thecal cavity, having completely divided the spinal cord. After removal there was a clear gap of three-quarters of an inch between the proximal and distal portions of the cord, containing no vestige of granulation or other tissue. September 12: Knee- and ankle-jerks present on both legs. Involuntary flexor spasms. November 10: Plantar stimulation causes homolateral flexor spasm with an extension spasm of the contralateral limb. Stimulation of the abdominal surface causes (1) an early local contraction, and (2) a delayed general contraction of the abdominal muscles with flexor spasms of the legs. Cremasteric and bulbo-cavernous reflexes are present. The sphincters are not relaxed and the bladder and bowel are evacuated at periodic intervals. A very strong faradic current is required to obtain the slightest contraction of any muscle below the knees, and this is also the case when the electrode is placed over the peroneal nerve on the outer side of the fibula. The patient is not conscious of any visceral sensations in the abdomen.

(*January* 25, 1917.)

Bullet Wound Producing a Segmental and Sensory Lesion of the Brain Stem.

By E. Farquhar Buzzard, M.D.

Private R. C. Wounded by rifle bullet on July 27, 1916. The bullet entered the left temple, traversed the head, and came out through the right external auditory meatus, causing complete loss of sight in the left eye and destruction of the right ear. He was unconscious for some days and a prisoner with the Turks in Bagdad, being afterwards exchanged. He complains now of some pain in the lower part of the right face, and deafness and some giddiness. He presents complete blindness of the left eye and complete deafness of

the right ear. The left eye moves well only in an inward and downward direction. The left side of the face does not move very well in emotional movements, but equally with the right in voluntary movement. There is much loss of sensibility to pain over the greater part of the right side of the head and more complete anæsthesia over the area supplied by the lower two divisions of the fifth nerve, but the corneal reflex is present on that side. There is also weakness of the right masseter muscle and deviation of the jaw to the right when the mouth is opened. Vision in the right eye is $\frac{6}{9}$, and ocular movements on that side are good.

Section of Neurology.

President—Professor W. D. HALLIBURTON, M.D., F.R.S.

(*March* 22, 1917.)

Chairman—Dr. PERCY SMITH.

Case for Diagnosis.

By JAMES TAYLOR, M.D.

W. P., PLATELAYER, was admitted on March 6, 1917, with a flaccid paraplegia. In July, 1916, patient became suddenly ill with headache, fever and vomiting. The fourth day after the onset he attempted to get out of bed, but fell. The legs were œdematous from the knees to ankles, and there was a feeling of pins and needles in the knees and ankles. A local doctor was called and his case diagnosed as typhoid fever. He was taken to Bethnal Green Infirmary on the tenth day following the onset. There was no epidemic of enteric at the time. Past health and family history were good.

On admission there was a large sacral sore about the size of half a crown; the legs swollen and œdematous. The lymphatic glands of the groin were enlarged on both sides. There was no pain nor tenderness in the spine. Vision: Right $\frac{6}{9}$, left $\frac{6}{9}$; disks healthy, and no limitation of visual fields. The pupils reacted to light and accommodation. Motor system: flaccid paraplegia of lower extremities, with involvement of the trunk muscles as far up as the level of the umbilicus. There was double foot-drop, with inversion. There was very slight movement of the flexors and adductors, but no movement in other muscles of the legs. The lower limbs were not flail-like, but there was limitation of movement at knees and ankles on passive movement. On attempt to sit up

there was marked upward deviation of the umbilicus. There were no involuntary movements and the upper extremities escaped. Electrical reaction: Weak response to faradic stimulation in adductors and flexors of thigh; reaction of degeneration in remaining muscles. Sensation: Feelings of pins and needles in the legs, and tenderness in the right inguinal region and in the calves of legs. There was no objective sensory loss. Reflexes: Knee-jerks, ankle-jerks and plantar reflexes absent. Lower abdominals absent, upper present. Arm-jerks all present. There was no loss of sphincter control. Wassermann reaction negative.

(*March* 22, 1917.)

Case of Labyrinthine Deafness and Vertigo.

By J. S. Risien Russell, M.D.

W. W., aged 45, chauffeur. Two months before admission sudden attack of unsteadiness and giddiness, unaccompanied by sense of rotation. Six attacks occurred on first day, and have persisted ever since. He is fairly comfortable when at rest. Sensations of nausea were present at onset, but he never vomited. Three weeks later he began to go deaf, and now has humming noises in both ears. Some headache occasionally.

On examination: Bilateral nerve deafness, with loss of both high and low tones. Rotation in various planes does not evoke the normal response. On passage of galvanic current patient inclines away from cathode. On stoppage of the current he sways to the anode. Nystagmus was increased during passage of current (15 to 18 ma.). Wassermann reaction negative. No other physical signs save slight nystagmus to sides.

(*March* 22, 1917.)

Case of Tumour of First Dorsal Region of Spinal Cord.

By W. Aldren Turner, C.B., M.D., Lieut.-Col. R.A.M.C.

J. S., schoolmaster, and since September, 1914, Quartermaster-Sergeant in West Kent Regiment. He was admitted on March 12, 1917, complaining of pain in shoulders and arms. In May, 1914, he

underwent an operation for carcinoma (?) of the left breast at the Miller Hospital, and the following September had swollen glands removed from the left axilla. In December, 1915, he had sciatica of the right side, but this did not necessitate his going off duty. In February, 1916, neuralgic pains developed in the right shoulder, which caused him sleepless nights; pain was absent during the day time. In May, 1916, the left shoulder began to pain him, and the following month there was excruciating pain in both shoulders, radiating down the arms, with a feeling of pins and needles in the hands. In July, 1916, he rejoined his regiment at Chatham, and the condition became worse; the right hand was weak, and he noticed wasting in the small muscles of the hand. He was discharged from the Army on August 23, 1916. In January, 1917, the pain became more intense, and was present day and night.

There is at present tenderness over the cervico-dorsal region. Vision: Right, $\frac{6}{9}$; left, $\frac{6}{9}$; fields and disks normal. There is narrowing of the right palpebral aperture, and the right pupil does not dilate to shade or to mechanical stimulation. Sweating occurs over the left half of the body. Definite weakness and wasting of the right hand is present; patient cannot make a fist on that side. Wasting of the intrinsic muscles of the right and left hand, but more definitely so on the right. Electrical reaction shows reaction of degeneration. No objective sensory loss can be detected. The knee-jerks are present, and plantar response is of flexor type. The arm-jerks are inactive and the abdominals sluggish. There is urgency of micturition. Patient walks without defect.

(*March* 22, 1917.)

Case of Myotonia Atrophica.

By W. Aldren Turner, C.B., M.D., Lieut.-Col. R.A.M.C.

K. N., female, aged 29, was admitted on February 19, 1917, complaining of attacks of severe epigastric pains, which have existed since last August. Pain which radiates to the left scapula and left clavicle, occurs every seven to ten days and lasts for fifteen minutes, subsiding without treatment.

Patient was born in Ireland, went to Paris at the age of 3, remaining there until the age of 20. She was always delicate, and during menstrual periods had to rest in bed for three to five days. At the age of 21 she returned to England and attended school at Tunbridge Wells. At that time she first observed difficulty in relaxing hand grasp when playing tennis, and she would easily become exhausted when walking short distances. Weakness has been progressively getting worse. A family history shows no disease of a similar nature.

Patient is tall and thin, has a sallow complexion, and is poorly nourished. The lungs and abdomen are normal. Disks are normal. Both lenses show peripheral ring of faint granular opacities. The special senses are normal. There is definite bilateral localized weakness and wasting with slow relaxation of the hand grasp (myotonia). The following muscles are wasted: Occipito-frontalis, corrugator supercilii, levator palpebræ superioris, temporals, masseters, facial, laryngeal, sternomastoid, flexors and extensors of the forearm, and the intrinsic muscles of the hand. The lower extremities though thin are not wasted. All muscles show good reaction to the interrupted current. Mechanical stimulation to extensors and flexors of forearms and legs show slow relaxation. There is no sensory loss, reflexes are normal, and there is no sphincter disturbance. She walks fairly well.

(*March* 22, 1917.)

Neuritic Type of Progressive Muscular Atrophy (Charcot-Marie-Tooth).

By W. Aldren Turner, C.B., M.D., Lieut.-Col. R.A.M.C.

E. C., male, aged 39, a steel turner, was admitted on January 24, 1917, with weakness and wasting of the legs. Weakness and wasting first appeared below the knees seven years ago; during seven months condition developed very quickly, and seven months from onset he was admitted to King's College Hospital, where he remained one month. His state has remained more or less stationary since, but wasting and weakness have developed in the muscles of the thigh. He walked

into the hospital with a stick and supported by his wife's arm. After walking a mile he would become completely exhausted, and there would be an aching pain in the lumbar region. Family history quite free from similar complaint.

On admission there were no cranial nerve defects. Motor system: Flaccid partial paraplegia of the legs; there is present fair but limited movement in hip and knee; movement of flexion and abduction better than of extension and adduction. No movement in ankles, but slight movement in toes of right foot. Right limb is a little stronger than left. There is symmetrical wasting of muscles of lower extremities with fibrillary tremors. Electrical reactions show slight contraction to faradism in adductors of leg and hamstrings. Other muscles show reaction of degeneration. The upper extremities have escaped. No sensory loss could be detected. No sphincter loss. Reflexes: Knee-jerks; ankle-jerks absent. Plantar gives no response. Other reflexes quite normal.

(*March* 22, 1917.)

Case of Amyotonia Congenita.

By F. E. Batten, M.D.

W. S., aged 3. Seventh member of S. family. Appeared normal at birth. Bottle-fed. Has never got on, and now, at the age of 3, is unable to walk. Family history: Father and mother healthy. Eldest child, girl, died at the age of 5 months. Second, girl, aged 10, healthy. Third, stillborn. Fourth, fifth and sixth, Alfred, Grace and Ettie, all affected. Seventh, patient. Eighth, baby, aged 1½, healthy. Tiny, sallow child, large abdomen and undeveloped limbs. Power very feeble, marked hypotonus. No paralysis. Liver and spleen enlarged. Deep reflexes present when in health.

(*March* 22, 1917.)

Case of Myasthenia Gravis.

By F. E. Batten, M.D.

L. B., aged 13, schoolboy. Four months ago was knocked down at school and bruised his face. His speech became "peculiar" immediately afterwards, and has become progressively worse. He now complains of tightness of the tongue. No trouble with biting or swallowing. Previous history: Old perforation of right tympanic membrane. Family history, *nil.*

On examination, face expressionless and sleepy. Palpebral fissures very small, ? ptosis. Orbicularis palpebrarum and ocular muscles good. Slight weakness of orbicularis oris. Bilateral atrophy of tongue. Palate moves fairly well. Larynx: Lax cords but no paralysis. Power of limbs very fair, but muscles tire easily. Sensation: Reflexes and fundi normal. He is said to be of average intelligence, but somewhat backward at school.

(*March* 22, 1917.)

Case of Huntington's Chorea.

By E. Farquhar Buzzard, M.D.

W. C., aged 34, ex-soldier. Irregular movements of feet and hands started one year ago while patient was in hospital at Cairo for a septic finger. He pulled faces, dropped things, and walked as if drunk. Previous history: Platelayer on railway; no infectious illness; temperate. Family history: Father is in an asylum for Huntington's chorea; grandmother died of chorea.

On examination: Irregular, inco-ordinated movements. Mental condition: Not highly intelligent, but no obvious signs of degeneration.

(*March* 22, 1917.)

Internal Hydrocephalus (?).

By E. Farquhar Buzzard, M.D.

A. C., male, aged 8. Irregular athetoid movements began after an attack of diarrhœa eighteen months ago. He was brought to hospital because he had "no control over the use of his hands and walked catching one foot in the other." He is a sallow, quiet boy. Slight involuntary movements of fingers and toes are present when patient is at rest. On performing voluntary movements everything appears to be done backwards. Movement is slow and creeping; hands are carried in position of extreme pronation. He walks clumsily, with toes turned in. Muscular power good; no sensory defect; all deep reflexes absent. Plantars flexor, abdominals active. Appears to be of average intelligence.

(*March* 22, 1917.)

Neuritic Type of Progressive Muscular Atrophy (Charcot-Marie-Tooth).

By E. D. Adrian, Captain R.A.M.C.

W. S. B., aged 26, driver in A.S.C. At the age of 9 he had an attack of weakness of the legs which confined him to a chair for six months. Recovered and played football when aged 16, but had to give it up as it made his legs weak. A few years later noticed increasing weakness of the legs and some blunting of sensation as high as the knees. His weakness varies; sometimes he feels quite strong, especially in the summer months. He has had difficulty in getting up after he has fallen down. Rejected four times on trying to enlist. Has been employed as motor ambulance driver for the past eight months and able to do his work.

In February, 1917, severe cold, pain in the legs, and loss of power. Admitted to hospital and improved.

Present condition: Upper limbs and trunk well developed; legs generally wasted and flabby, but below the knee the legs are very thin. There is considerable general weakness in all the muscles of the lower limbs, most pronounced in the anterior tibial and peronei. The patient, when placed on the floor, is quite unable to get into the erect position. He stands with a wide base and knees slightly bent, and walks with a high-stepping gait. Reflexes: Tendon-jerks all present; plantars not obtained; sphincters normal. Sensation: Considerable blunting of sensation in both legs below the knees. Electrical reactions: The muscles all react normally.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

EDITED BY

J. Y. W. MacALISTER

UNDER THE DIRECTION OF

THE EDITORIAL COMMITTEE

VOLUME THE TENTH

SESSION 1916-17

SECTION OF OBSTETRICS & GYNÆCOLOGY



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1917

Section of Obstetrics and Gynæcology.

OFFICERS FOR THE SESSION 1916-17.

President—

G. F. Blacker, M.D.

Past Presidents—

Sir Francis H. Champneys, Bt., M.D.
Sir J. Halliday Croom, M.D.
Alban Doran, F.R.C.S.
M. Handfield-Jones, M.D.
Herbert R. Spencer, M.D.
Sir William J. Smyly, M.D.
W. D. Spanton, F.R.C.S.

Vice-Presidents—

T. Watts Eden, M.D.
Archibald Donald, M.D.
J. M. Munro Kerr, M.D.
Cuthbert Lockyer, M.D.
J. D. Malcolm, F.R.C.S.Ed.
C. Hubert Roberts, M.D.
G. Drummond Robinson, M.D.
Amand Routh, M.D.
Thomas Wilson, M.D.

Hon. Secretaries—

H. Russell Andrews, M.D.
T. G. Stevens, F.R.C.S.

Other Members of Council—

Louisa Aldrich-Blake, M.S.
Lady Barrett, M.D.
J. D. Barris, F.R.C.S.
Victor Bonney, M.D.
William Gilliatt, M.S.
W. S. A. Griffith, M.D.
T. B. Grimsdale, M.B.
J. P. Hedley, M.C.
Gordon Ley, F.R.C.S.
Ewen Maclean, M.D.
Miles H. Phillips, M.B.
Hugh J. M. Playfair, M.D
D. W. Roy, M.B.
W. Fletcher Shaw, M.D.
G. Bellingham Smith, F.R.C.S
E. Hastings Tweedy.
Clifford White, M.D.

Representative on Library Committee—

Comyns Berkeley, M.D.

Representative on Editorial Committee—

John Phillips, M.D.

SECTION OF OBSTETRICS AND GYNÆCOLOGY.

CONTENTS.

October 5, 1916.

	PAGE
WALTER S. A. GRIFFITH, M.D.	
Fibrolipoma of the Right Broad Ligament weighing 13 lb.	1
HUBERT ROBERTS, M.D.	
Large Ureteral Calculus removed by Abdominal Section	3
FRANK BELBEN, F.R.C.S. (Communicated by HUBERT ROBERTS, M.D.)	
Large Vesical Calculus, 3½ oz., around a Slate Pencil	7
CUTHBERT LOCKYER, M.B.	
(1) Three Ureteral Calculi removed by Abdominal Section	8
(2) Recto-vaginal Calculus: a Ureteral Injury	10
(3) Large Vesical Calculus removed through a Vesico-cervico-vaginal Fistula	11
ARTHUR J. MCNAIR, M.D.	
Concealed Accidental Hæmorrhage with Intraperitoneal Bleeding ...	13
CARLTON OLDFIELD, F.R.C.S., and REGINALD G. HANN.	
Acute Toxæmia of Pregnancy, with Acute Nephritis and Accidental Hæmorrhage; Cæsarean Hysterectomy; Recovery	17
W. FLETCHER SHAW, M.D.	
Cæsarean Section and Hysterectomy for Accidental Hæmorrhage ...	23

November 2, 1916.

SPECIAL DISCUSSION ON THE NEED FOR IMPROVEMENT IN THE CARE OF PREGNANT WOMEN.

S. G. Moore, M.D. (Huddersfield). PAGE

The Need for Improvement in the Care of Pregnant Women, and a Direct Means to that End 37

Amand Routh, M.D.

The Importance of getting a Pregnant Woman under Medical Supervision and affording Her the Necessary Treatment 44

Comyns Berkeley, M.D.

The Importance of getting Medical Practitioners and Midwives to co-operate with the Local Health Authorities 52

Lady Barrett, M.D.

The Importance of linking up all Organizations for Maternity and Child Welfare in Local Health Districts 62

Professor L. A. Krivsky, M.D. (p. 70)—Professor Munro Kerr, M.D. (p. 73)—Professor Henry Briggs, F.R.C.S. (p. 76)—Miss Rosalind Paget (p. 77)—Sir Francis Champneys, Bt., M.D. (p. 78).

March 1, 1917.

J. P. Hedley, M.C.

A Uterus containing a Carcinoma and a Sarcoma 81

Herbert R. Spencer, M.D.

Carcino-sarcoma Uteri 82

Cuthbert Lockyer, M.D.

(1) Simultaneous Bilateral Tubal Pregnancy 88

(2) Metastatic Glioma (Neuro-epithelioma) of the Right Ovary in a Child aged 3 102

C. Hubert Roberts, M.D.

(1) Curious Degeneration of a Cervical Tumour 106

(2) A Calcified Fibroid which caused Complete Axial Rotation of the Uterus 109

G. Blacker, M.D. PAGE

Case of Supravaginal Amputation of Uterus for Sarcoma mistaken for Myoma 111

Herbert R. Spencer, M.D.

(1) Two Cases of Supravaginal Amputation of the Uterus for Supposed Myoma found to be Sarcoma 112

(2) Four Cases of Undiagnosed Cancer of the Cervix in 200 Total Hysterectomies for Myoma 120

May 3, 1917.

Gordon Ley, F.R.C.S.

(1) Case of Spontaneous Rupture of the Uterus 135

(2) Decidual Reaction in a Subperitoneal Fibromyoma of the Uterus ... 137

John D. Malcolm, F.R.C.S.Ed.

Post-operative Tympanites: Its Nature and some Points in its Treatment 140

Cuthbert Lockyer, M.D.

Two Cases of Primary Ovarian Pregnancy (with a Review of the Literature, 1910-17) 158

The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

LONDON:
JOHN BALE, SONS AND DANIELSSON, LTD.,
OXFORD HOUSE,
83-91, GREAT TITCHFIELD STREET, OXFORD STREET, W. 1.

Section of Obstetrics and Gynæcology.

President—Dr. G. F. Blacker.

(*October* 5, 1916.)

Fibrolipoma of the Right Broad Ligament weighing 13 lb.

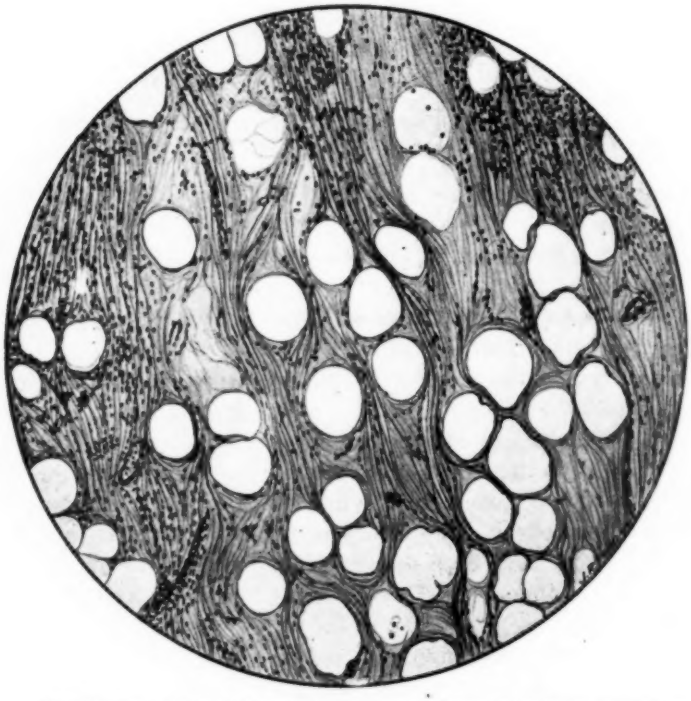
By Walter S. A. Griffith, M.D.

M. N., aged 57, was admitted to Martha Ward, St. Bartholomew's Hospital, in May, 1916. Family history, nothing important. Menstruation began at the age of 13, and ceased at the age of 40; duration four to five days; average quantity. Four pregnancies: three children, one miscarriage fourth month; no complication. No bowel troubles; micturition lately frequent, urine normal; no vaginal discharge. She has lost weight considerably. When menstruation ceased seventeen years ago she noticed some enlargement of the abdomen and thought herself pregnant, and from that time she has suffered abdominal pain though not continuously. There has been some prolapse of the uterus since that time.

The abdomen was found to be considerably distended by a tumour on the right side, and by flatulent bowel on the left. The tumour was elastic and tender, and extended from the pelvic brim to the right costal margin in front of the right kidney. The cervix, enveloped in the everted vagina, protruded from the vulva when straining; the tumour can be felt at the pelvic brim and was thought to be ovarian.

Operation, May 29, 1916: The tumour was found to be retroperitoneal, extending from the right broad ligament to the lower surface of the liver; it occupied the whole right half of the abdominal cavity, and extended beyond the middle line into the left half. It had stripped off the peritoneum of the abdominal wall posteriorly and on the right

side in the course of its growth up from the pelvis. The peritoneal capsule in the middle line was incised over its middle third by a vertical incision, and the tumour was gradually enucleated from above downwards, and its vessels, when necessary, were ligatured. The lower part was enucleated from the right broad ligament, exposing the right side of the uterus down to the cervix, the chief vessels being secured here. The large cavity in which the tumour lay soon contracted, and was



Fibrolipoma of broad ligament. Groups of large fat-cells in connective tissue; many deeply-stained rod-shaped nuclei indicate the presence of unstriped muscle-fibres.

gradually closed by catgut purse-string sutures which stopped the oozing; and the abdomen was closed without drainage. The uterus, ovaries and tubes presented normal senile characters.

The patient made a quite uneventful recovery, and left the hospital on July 14.

The tumour weighed 13 lb., and presented the yellow translucent

character of a lipoma. One half is preserved for the museum at St. Bartholomew's Hospital, the other was presented to the Royal College of Surgeons, there being no specimen of the kind in the museum. Sections show groups of large fat cells surrounded by connective tissue, the nuclei of which stain deeply with hæmotoxylin. In some places the fat cells predominate, in others the connective tissue. There are also many rod-shaped deeply-stained nuclei, indicating the presence of unstriped muscle fibre.

Lipoma of the broad ligament appears to be very rare. Many writers do not mention them, and so far the author has not met with a description of one. Pozzi,[1] in his "Treatise on Gynæcology," states that he has seen one case only which was mistaken for an ovarian tumour; the patient died suddenly three days after an exploratory puncture. Sir F. Treves[2] removed one weighing 4½ lb., also from the right broad ligament. No details were given. The specimen is in the London Hospital Museum.

Subperitoneal lipomata arising in the posterior portion of the abdominal wall near the spinal column are less rare, and several cases are recorded. Their removal or attempted removal has often been followed by fatal results.[3]

(*October* 5, 1916.)

Large Ureteral Calculus removed by Abdominal Section.

By Hubert Roberts, M.D.

F. S., aged 30, married; two children. There is history of trouble with the right kidney since the age of 7. She has been in several London hospitals from time to time at ages of 17, 22, and 25. She has also been X-rayed and examined under chloroform. The diagnosis was evidently either stone in the right kidney or tuberculosis; at one hospital she was treated with tuberculin. The most notable symptoms have been pain in the region of the right kidney and ureter, accompanied with attacks of "cystitis" and hæmaturia.

The patient has been under my care for the past two years. She

[1] "Treatise on Gynæcology" (Translation), New Sydenham Society, 1893, iii, p. 123.

[2] *Trans. Clin. Soc. Lond.*, 1893, xxvi, p. 106.

[3] Doran, *Trans. Obst. Soc. Lond.*, xliv (1902), 1903, p. 265.

was first seen at the Samaritan Hospital in 1914, where she was sent by Dr. Pelly, of Highgate, on account of a lump on the right side of the pelvis. She was then pregnant six months. The diagnosis made was that of a ureteral calculus or a tuberculous gland (calcified). The mass seemed to be the size of a large walnut. The urine at the time was clear, but contained albumin and a few pus cells. I decided to watch the case and advised her to come to me for her confinement at Queen Charlotte's Hospital. This she did, and she was delivered normally of a living child on June 23, 1914. The lump never moved; it did not seem to interfere with the passage of the head. Forceps were not used. We were not able to feel the right kidney.

I saw no more of the case till June 9, 1916, when she was re-admitted to the Samaritan Hospital for severe pains in the right side of the abdomen and in the rectum and for frequent micturition. The water on examination was clear but rather scanty. The mass to the right of the cervix was much as before, very hard, quite fixed, and high up in the right fornix of the vagina. It was very painful to the touch. The bladder sound and cystoscope revealed nothing. The diagnosis of ureteral calculus was again made, and I decided to try to remove it by abdominal section. This was done on June 12, 1916.

I made a median incision of some length in order to get a full view of the pelvis. On inspection, the right ureter was at once seen. It was enormously distended and very thick. It was of the size of two fingers and felt like a thick hose-pipe. It was considerably thicker than the patient's aorta. Deep in the pelvis in the huge ureter I could feel the stone exhibited. It was quite fixed in a sort of pouch, just at the base of the bladder. I clamped the ureter easily just above the stone and then cut down on it. The walls of the ureter were very thick; on my dislodging the stone much foul urine escaped. After cleansing the field of operation I was able to sew up the ureter, then a parametric layer, and finally the peritoneum of the posterior surface of the broad ligament. Fearing possible extravasation of urine if the stitches did not hold, I opened Douglas's pouch, using the instrument shown (a very useful one), invented by Wallace, of Liverpool. The abdomen was then closed in the ordinary way. I was very interested to know what the urine in the bladder would be like after removal of the stone, so a catheter was passed soon after the patient was put to bed. It was very putrid and contained pus and blood and débris of all sorts; previous to the operation the urine was clear.

The patient made an excellent recovery, the highest temperature

being 102·2° F. on the fourth day. She got up on the sixteenth day and left the hospital on the third week after the operation. The condition of the urine gradually improved and she passed, on an average, 60 to 70 oz. *per diem* just before leaving the hospital. The tube and plugging in Douglas's pouch were removed on the fourth day. There was never any extravasation of urine noticed, so I conclude my stitches held. I saw her on October 3, 1916, when she was quite well. There is still some thickening of the right ureter to be felt *per vaginam*.

Report.—The stone exhibited weighs over ½ oz. (275 gr.)—i.e., 4 dr. 35 gr. It measures 1 in. long, ¾ in. thick, 1¼ in. circumference. Chemical analysis shows the outside layers to be composed entirely of phosphates. As the central portion has not been examined, I cannot report on the composition as a whole. The calculus has a curious shape rather like a Hotchkiss gun bullet definitely pointed at its lower end—i.e., towards the bladder—and flattened on its upper part. The circumference is rounded and gives a good idea of the amount of distension of the ureter in which it lay.

I have only met with one other case of ureteral calculus. This occurred eleven years ago, also at the Samaritan Hospital. The patient refused operation, but subsequently was admitted to the Middlesex Hospital under Sir Henry Morris, who removed a large "date-stone" calculus from the right ureter by "the sacral extra-peritoneal" method. The patient had a fistula for some weeks, but ultimately recovered. I was present at the operation.

I have ventured to show this specimen as ureteral calculi are rare. The majority form in the renal pelvis, as in my case. Sometimes ureteral calculi form around a foreign body such as a silk stitch. Impaction usually takes place (if the stone is not passed) at the outlet of the renal pelvis, at the pelvic brim, or at the entrance of the ureter into the bladder. Ureteral calculi are very rarely bilateral, and are usually single. Most of the stones reported are "date-stone" in shape.

Choyce[1] reports stones weighing 816 gr. (Bloch), 803 gr. (Carless), and 780 gr. (Fe[illegible]). In most cases the kidney above is hydronephritic. Ureteral calculi give rise to severe attacks of colic, repeated at frequent or long intervals, ending only when the calculus is expelled into the bladder; after some hours or days the calculus may be discharged from the urethra. Stones in the lower part of the ureter may be felt *per rectum* or *per vaginam;* sometimes cystoscopy will even show

[1] "System of Surgery," 1912, ii, p. 843.

the tip of a stone projecting into the bladder. Kelly uses wax-tipped ureteral bougies which show scratches on the wax when a calculus is present. Radiography should always be employed in diagnosis.

Treatment.—From the small experience of two cases I can say little. Apparently small ureteric stones are passed from the female bladder with less trouble than from that of the male. Diuretic drugs may help sometimes in lessening or dissolving calculi. Operation seems to be needed with all large stones that become impacted in the ureter, as the condition leads to anuria, infection, or dilatation and destruction of the corresponding kidney. The ureter may be exposed either near the kidney, the brim of the pelvis, or at its entrance to the base of the bladder. The operation may be extra- or intra-peritoneal. Some stones have been removed by the vaginal route,[1] or a sacral-extraperitoneal route, similar to Kraske's operation. The drainage of Douglas's pouch may or may not have been necessary, but I thought it a safeguard.

I used the intra-peritoneal abdominal method as in my case it seemed to give the best exposure of the calculus and greater freedom for subsequent technique.

I append a list of references which may be of use to those who wish to look up this interesting subject.

BIBLIOGRAPHY.

ADLER, L. "Ureterstein," *Zentralbl. f. Gyn.*, 1915, xxxix, pp. 908-911.

ARCELIN, F. "Le diagnostic radiographique des calculs de l'uretère pelvien," *Journ. de Radiol. et d'Electrol.*, 1914, i, pp. 113-120.

BARRINGER, B. S. "Ureterocele and Ureteral Stone," *Trans. Amer. Assoc. Genito-Urin. Surg.*, 1914, ix, pp. 44-50.

BRAASCH, W. F., and MOORE, A. "Stones in the Ureter," *Journ. Amer. Med. Assoc*, 1915, lxv, pp. 1234-1237.

BRENNER, F. "Zur Diagnose und Therapie der Uretersteine," Inaug. Dissert., Kiel, 1915.

BUERGER, L. "Unusually large Ureteral Calculi," *New York Med. Journ.*, 1914, c, pp. 1103-1107.

CABOT, H. "Errors in Diagnosis of Renal and Ureteral Calculus," *Surg., Gyn., and Obst.*, 1915, xxi, pp. 403-406.

COLLINSON, H. "Notes on Four Cases of Ureteral Calculus, in one of which a Complete Cast of the Ureter was Present," *Lancet*, 1913, ii, pp. 1456-1460.

FURNISS, H. D. "Renal and Ureteral Calculi," *Trans. Amer. Assoc. Obst. and Gyn.*, 1913, xxvi, pp. 144-174; *Amer. Journ. Obst.*, 1913, lxviii, pp. 1107-1132.

GERAGHTY, J. T., and HINMAN, F. "Ureteral Calculi: Special Means of Diagnosis and Newer Methods of Intravesical Treatment," *Surg., Gyn., and Obst.*, 1915, xx, pp. 515-522.

GRASER, E. "Entfernung eines Uretersteins durch Ureterotomie," *Beitr. z. klin. Chir.*, 1914, lxxxviii, pp. 736-738.

[1] Kelly and Noble, "Gynæcology and Abdominal Surgery," 1908, ii, p. 742.

HEINSIUS, F. "Ueber die kystoskopische Diagnose eines Ureterstein und seine Entfernung auf vaginalem Wege," *Zeitschr. f. Geb. u. Gyn.*, 1913, lxxiii, pp. 441-451.

LEWIS, B. "Ureter Stones: The Technique of their Removal by Cystoscopic Methods, with Report of Cases," *Trans. South. Surg. and Gyn. Assoc.*, 1914, xxvii, pp. 458-470.

MURPHY, J. B. "Ureteral Calculus; Ureterotomy; Removal of Calculus," *Surg. Clin.*, Chicago, 1915, iv, pp. 931-945.

NEWMAN, D. "Ureteral Calculus, its Symptoms and Treatment, with a few Illustrative Cases," *Brit. Med. Journ.*, 1915, ii, pp. 557, 598.

WATSON, J. H. "Ureteral Stone; with Special Reference to those in the Pelvic Ureter." *Brit. Med. Journ.*, 1915, i, pp. 993-997.

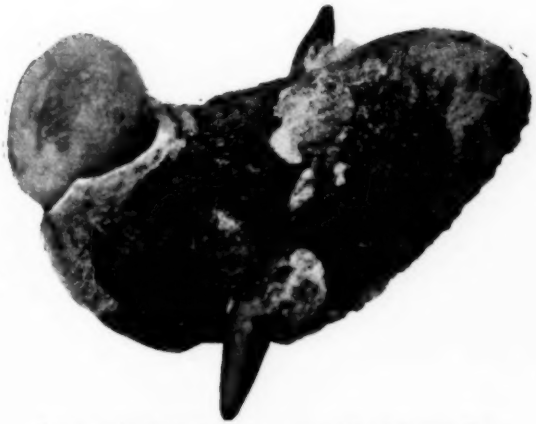
(*October* 5, 1916.)

Large Vesical Calculus, 3½ oz., around a Slate Pencil.

By FRANK BELBEN, F.R.C.S.

(Communicated by HUBERT ROBERTS, M.D.)

PATIENT, a girl, aged 17, complained of inability to hold water. Her illness began two years ago with frequency of micturition and pain during the act. She at first denied having at any time introduced any-



Large vesical calculus, 3½ oz., around a slate pencil.

thing into the urethra. Pain had been less during the last six months. She was pale and thin, and her face was covered with acne. Ammoniacal urine, containing blood and pus, constantly dribbled from the

urethra. An anæsthetic was given, but the beak of the cystoscope impinged on some hard substance, and could not be introduced. A rounded mass was felt bimanually. Suprapubic cystotomy was done, and the two stones were removed with a good deal of difficulty, as the bladder was completely contracted round them, and the ends of the slate-pencil were embedded in its walls. On being shown the specimen the patient admitted that in consequence of irritation she had introduced the slate-pencil into the urethra nearly two years ago. It had slipped in, and she was afraid to tell anyone. Convalescence was interrupted by what was apparently a sharp attack of left renal colic, accompanied by hæmaturia. She was again cystoscoped, but no stone was seen, and the bladder appeared normal. A radiograph of the kidney also proved negative. She was discharged quite well; the acne had entirely disappeared, though there had been no treatment for it. I think the small stone probably set up the bladder irritation, and that it was to relieve this that the pencil was introduced.

(*October* 5, 1916.)

Three Ureteral Calculi removed by Abdominal Section.

By Cuthbert Lockyer, M.D.

The patient from whom the first calculus was removed was a married woman, aged 38. She was sent to me in March, 1905, on account of attacks of severe pain in the right iliac and lumbar regions, accompanied by menorrhagia and leucorrhœa. She had been married for five and half years, and had one child aged 4½. The pain of which she complained started behind, and its maximum intensity was felt over the centre of the right iliac crest; it radiated round to the front to another spot of maximum intensity which corresponded with McBurney's point. It was first noticed in 1903, two years before I saw the patient. It came on in definite "attacks," and these had become progressively frequent until finally they were of weekly occurrence. The onset of an attack was occasionally accompanied by sickness; the pain was relieved by rest in bed. On examination no tumour could be felt. The uterus was bulky, the cervix deeply torn on the right side, and the right ovary was enlarged and tender. I advised admission to hospital, but the patient, who superintended a laundry, said she could

not leave her work, and I lost sight of her for some years. On May 16, 1911, she was again sent to me by her doctor with a letter saying he found "a distinct tumour, which is freely movable and appears to be rapidly increasing. . . . In all probability it is ovarian." The patient now complained of a continuous heavy pain in the right iliac fossa, and of a swelling in the abdomen, which caused her "to alter her dresses." She had been a widow for four years; the periods were still excessive; there was still much leucorrhœa; the bowels only acted with aperients. *Micturition was normal.* A tumour was present in the right lumbar region, which descended on inspiration, and was diagnosed as a renal new growth. On May 22, 1911, I operated through an incision in the right *linea semilunaris.* A stone was found in the upper segment of the right ureter; the kidney, which was enlarged, was drawn up into the wound and incised, the stone was "milked" into the open pelvis of kidney and removed. The renal incision was closed by interrupted catgut sutures and a rubber drainage tube inserted into the loin. The stitches were removed on May 29. No urine ever escaped from the drain in the loin. Convalescence was uneventful, and there has been no further renal trouble. A letter received from the patient in September, 1916, states that she is now in perfect health. The stone, a typical uric acid calculus, measures 1 in. by ½ in. In shape it resembles a plum-stone, its surface is irregular and furrowed, and its colour is dark brown.

The remaining two calculi which were situated in the lower segment of the ureter, close to the bladder, were removed from a patient, aged 63, during an operation for a uterine fibroid and a solid ovarian tumour. The stones had obstructed the right ureter, and this duct was the size of a segment of small intestine. The patient had suffered from bacilluria, which vaccines failed to relieve. The entire ureter was excised after clamps had been applied to its upper and lower ends. The right kidney was left *in situ.* A perinephric abscess developed, due to soiling of the perirenal fat with foul urine, which escaped from the upper cut end of the ureter. The abscess was opened and drained. The patient then made an uninterrupted recovery. This case has already been reported in full in the *Proceedings* of this Section as one of double ureter.[1]

The two calculi are phosphatic. Dr. Foster Morley has reported that they contain no uric acid.

[1] *Proc. Roy. Soc. Med.*, 1915, viii (Sect. Obst. and Gyn.), p. 47.

(*October* 5, 1916.)

Recto-vaginal Calculus: a Ureteral Injury.

By CUTHBERT LOCKYER, M.D.

THE calculus shown in the figure was extracted from a woman from whom I removed the uterus, a portion of the rectum and a segment of the vagina. This operation was performed on November 28, 1913, for what I took to be a malignant growth. The case was published in the *Proceedings*[1] as an adenomyoma of the recto-vaginal septum. The patient subsequently developed a ureteral fistula, and she was re-admitted into the Samaritan Hospital on July 14, 1914, for incontinence of urine. The calculus, now exhibited (*see* figure), was found lying astride



Recto-vaginal calculus (natural size).

the recto-vaginal septum, with one cusp occupying the disused bowel and the other lying in the vagina. The stone was removed in two sections, both being withdrawn *per vaginam*. Subsequently ureteral catheters were passed and urine was found to issue from the right ureter and none from the left duct. The latter was ligatured through an incision in the right loin and the leakage through the vagina ceased. Unfortunately urine began to escape through the loin-wound, which had to be freely drained. Finally the left kidney was removed through a lumbar incision; after this the patient made a good recovery.

[1] *Proc. Roy. Soc. Med.*, 1915, viii (Sect. Obst. and Gyn.), p. 47.

(*October* 5, 1916.)

Large Vesical Calculus removed through a Vesico-cervico-vaginal Fistula.

By CUTHBERT LOCKYER, M.D.

THE calculus, now exhibited, was removed on November 28, 1910, from the bladder of a multipara eleven months after the last confinement. The clinical history is as follows: Patient, aged 38, married twelve years; five children, no abortions. The last confinement fourteen months ago. All labours were instrumental. On October 16, 1909, labour-pains began at 9 a.m. Delivery by forceps was carried out at 2 p.m. Incontinence of urine commenced the same day. There was no *post-partum* hæmorrhage. Dr. Bell, of Luton, tried to close a fistula on the ninth day of the puerperium. Twelve days later (November 4) the patient was admitted into the Bute Hospital, Luton, because the first operation had not been successful. Two subsequent attempts at closure also failed, and the patient was sent to me on November 26, 1910—eleven months after the third operation. She was then wearing a rubber urinal; the buttocks were excoriated and bathed in foul urine. The perineum was sound. Large masses of phosphates hung on strands of silk from the roof of the vagina. The anterior wall of the cervix was lacerated for a distance of $1\frac{1}{2}$ in. in the vertical line, with the posterior wall of the cervix forming the floor of a large vesico-vaginal fistula. The laceration in the floor of the bladder admitted three fingers easily; it extended above for about $1\frac{1}{2}$ in. above the cervical tear. On November 28 I removed the lumps of phosphatic deposit adhering to the edges of the fistula, and after cleansing the parts, the bladder was separated from the anterior wall of the cervix and the latter was sutured by four interrupted thick catgut sutures. The bladder was then separated from the vaginal wall and from the lateral connective tissues. A large calculus measuring $5\frac{1}{2}$ in. in circumference was then withdrawn through the rent in the bladder; the latter was very contracted and its cavity seemed to be about the size of the phosphatic mass which occupied it. When fully exposed the vesical tear was seen to be circular in front and tailed off

into a linear wound above. The mucous membrane was nearly ½ in. thick and intensely congested. Redundant mucosa was cut away and the thick œdematous fibro-muscular wall was pared with a fine fistula-knife and brought together by interrupted catgut sutures. The deep lateral recesses between the bladder and pubic rami were closed by mattress-sutures of catgut passed through the retracted pubo-coccygei muscles and through the central line of the bladder-wall. The bladder was then sewn to the cervix by catgut. The vaginal flaps were now reduced to a suitable extent, and then united across the middle line, whilst their lower ends were united to the cervix to form a ⊥-shaped scar. Interrupted fishing-gut sutures were used for this, and between these stitches a drain of bismuth gauze was inserted. A self-retaining catheter was placed in the bladder.

On December 9, eleven days after the operation, sterile milky water introduced *per urethram* was seen to escape through the external os uteri. The bladder was again separated from the cervix and from the vagina. A fistula which admitted *one* finger was found; its edges were freshened and then united with catgut. This time the bladder was not sewn to the cervix but a gauze drain was inserted into the vesico-cervical interval. The fistula closed, and the patient writes annually to say she is in good health.

DISCUSSION.

Mr. DOUGLAS DREW: There is an interesting point which Dr. H. Roberts has mentioned—namely, that so large a stone did not cause any difficulty in delivery. My suggestion is that possibly the stone slipped up into the dilated ureter above. I here show a specimen of a ureteral stone, over 1 in. in length and about as thick as the little finger, which I removed by the ilio-lumbar incision. The interesting feature of the case was that the stone, although tightly impacted in the ureter, could not only be drawn down by the finger into the vagina, but it could also be pushed up so that the fingers on the abdomen could be placed below it, showing how very mobile the ureter may be when it contains an impacted stone. It is my experience that before a stone reaches so large a size the ureter is liable to become fixed to surrounding structures owing to periureteritis being set up.

Dr. HUBERT ROBERTS: The following are short notes of a curious case which I was called to see ten or eleven years ago at the request of Dr. Stanley Box, of Ealing. It was that of an old lady, aged 70, who had a horrible discharge and incontinence both of fæces and urine. On examination, a large hard mass was found in the vagina the size of a cricket ball, and in its centre the steel ends of a forgotten Zwancke's pessary. The mass was very foul, and tightly wedged

in the roof of the vagina. The patient thought a pessary had been placed there over twenty-four years ago. All ordinary efforts to remove the calculus with gynæcological instruments failed so I was forced to use some heavy tools from my motor car (boiled of course). With the help of these I broke up the calculus and removed the pessary piece-meal. One wing had entered the bladder and the other the rectum. The operation was a severe one but the patient ultimately did quite well. I rang up Dr. Box on October 2, 1916, to inquire about the case and he informed me that the lady was still alive and well (now aged 80), and that both the fistulæ had eventually closed.

(*October* 5, 1916.)

Concealed Accidental Hæmorrhage with Intraperitoneal Bleeding.

By Arthur J. McNair, M.D.

Mrs. A. K., aged 34, 6-para, was admitted into Guy's Hospital on May 12, 1916, from the Maternity Charity. Mr. Bellingham Smith kindly permits me to publish these notes. Five previous gestations terminated naturally at full term. Her present pregnancy, which was at the thirty-fourth week, had been complicated by the occurrence of profuse uterine hæmorrhage at the third month. This bleeding persisted for four days, but then ceased, and did not recur. She had also, in March, vomited a large quantity of blood on two occasions.

On the morning of the day of admission she arose from her bed to prepare breakfast, and was immediately seized with agonizing pains in her abdomen. She fainted, but soon recovered consciousness, and the pain became less. On again attempting to leave her bed, however, later in the day, she was once more attacked with griping abdominal pain, which gradually became continuous and more severe. She stated that the pains were quite unlike the pains of labour, and that there had been no "show." It is, perhaps, worthy of mention that on the previous day she had carried a child, aged 3, to the hospital and back.

On admission she was blanched, her skin was moist with perspiration, her breathing was rapid and restrained, and her pulse was feeble and irregular. Her uterus, the fundus of which reached to the ensiform cartilage, was of woody hardness; there was no alternation of contraction

and relaxation. Pain was intense, and palpation of the abdomen was scarcely tolerated. The fœtal head could be felt at the pelvic brim, but no other fœtal parts could be distinguished, nor could the heart-sounds be heard. On examination *per vaginam* the cervix was found one-third dilated, the membranes were intact, and the head was very firmly engaged in the third vertex position. There was no trace of hæmorrhage.

Having made a diagnosis of internal hæmorrhage I decided to empty the uterus at once, as the woman's condition was becoming progressively more serious. Cæsarean section was considered, but as the cervix was partially dilated, and did not seem to be unduly rigid, delivery *per vias naturales* was thought to be the more favourable method.

The patient was transferred to the theatre, where intravenous saline infusion was begun. Under ether anæsthesia the cervix was manually dilated with ease, and the fœtus, 5 lb. in weight, was delivered with a cranioclast. During the dilatation of the cervix the membranes were ruptured, and, on displacement of the head, the pulseless cord was prolapsed by a forcible gush of amniotic fluid untinged with blood. When dilatation was nearly completed blood escaped from the uterus for the first time. As soon as the head was extracted the placenta, which was completely detached, presented, and was delivered with the body. This was followed by the discharge of a large quantity of blood, mostly recently clotted. The amount was estimated at 3 pints. A hot intra-uterine douche was given, and pituitrin was injected. The uterus retracted perfectly, and subsequently there was only a negligible amount of hæmorrhage. Urine, obtained by catheterization, contained a trace of albumin, but no casts were detected in the centrifugalized deposit. The placenta was normal in every respect: there were no areas of infarction.

The patient was back in bed in twenty-five minutes. Saline infusion, which had been given intravenously to the extent of 2½ pints was continued subcutaneously. She was suffering from severe shock and collapse, but during the next three hours her condition materially improved. Half an hour later, however, she became uncontrollably restless, complained of pain in her abdomen, and died rather suddenly.

At the autopsy the peritoneal cavity was found to contain 24 oz. of recently shed blood, the source of which was evidently the uterus. The uterus was preserved for further investigation. All other organs were anæmic, but apparently free from disease.

The specimen consists of the uterus with a small part of the vagina attached. On its anterior surface are a number of lacerations, some of which are mere fissures of the serous coat, whilst others penetrate more deeply into the uterine muscle. The right half of the posterior wall has been removed to show the placental site, which occupies a position on



Concealed accidental hæmorrhage. Uterus seen from in front showing peritoneal lacerations.

the anterior wall corresponding with the situation of the superficial tears on its exterior. A deep furrow runs vertically across the placental site. Numerous subserous hæmorrhages can be seen, while in the line of section patches of hæmorrhagic infiltration can be detected.

Microscopical sections show areas of interstitial hæmorrhage, separating apparently healthy muscle fibres. No excess of elastic tissue is demonstrable, nor is there an unusual amount of white connective tissue.

Superficial laceration of the uterus has been described in a number of cases. Braxton Hicks [2], reading a paper before the Obstetrical Society of London in 1861, mentioned several recorded instances of this complication of concealed accidental hæmorrhage, and gave particulars of a case which had occurred in the Guy's Maternity Charity. In his case, however, no blood had escaped into the peritoneal cavity. Recently the condition was reviewed by M. Fraipont [3], who described a case in many respects similar to the subject of this communication. He laid stress upon the association of peritoneal tears with the condition which Couvelaire had designated as "apoplexe utéro-placentaire." Shannon [4], Clifford [1], and Fletcher Shaw [5] have published notes on similar cases. The last-named found areas of extravasated blood in four out of five uteri, which had been removed for concealed hæmorrhage. In no instance have I found reference to an internal tear such as can be seen running across the site of placental attachment in my specimen. These lacerations may logically be assumed to result from sudden over-distension of the uterus. I can bring forward no additional evidence as to the causation of this catastrophe, a question which has been the source of much controversy. Whitridge Williams [6] has dealt with the subject of premature separation of the placenta in uteri extensively infiltrated with blood. He gives details of searching histological investigation of two uteri, and appends a full bibliography of the whole subject.

REFERENCES.

[1] Clifford. *Journ. Obst. and Gyn. Brit. Emp.*, 1914, xxv, p. 48.
[2] Hicks, Braxton. *Trans. Obst. Soc.* (1860), 1861, ii, p. 53.
[3] Fraipont. *Ann. de Gyn. et d'Obst.*, 1914.
[4] Shannon. *Glasgow Med. Journ.*, 1912, lxxvii, p. 290.
[5] Shaw, Fletcher. *Journ. Obst. and Gyn. Brit. Emp.*, 1914, xxvi.
[6] Williams, Whitridge. *Surg., Gyn., and Obst.*, 1915, xxi, pp. 541-554.

(*October* 5, 1916.)

Acute Toxæmia of Pregnancy, with Acute Nephritis and Accidental Hæmorrhage; Cæsarean Hysterectomy; Recovery.

By CARLTON OLDFIELD, F.R.C.S., and REGINALD G. HANN.

A WOMAN, aged 38, married nine years, expected her first confinement about August 8. Her pregnancy was uneventful until a few days before the sudden onset of her illness, when she had noticed transient swelling of the feet and face and some abdominal pain. She attached no importance to these symptoms and went about as usual, and was not seen until urgent abdominal pain began about 9.30 p.m. on June 16. The pain was severe and continuous and was localized to the region of the urinary bladder. She did not appear ill and the uterine muscle was slack, painless, and apparently unaffected. No other signs or symptoms were present. At 11 p.m. her condition had altered seriously. The pain was very severe and involved the whole uterus, which was tender and tense; it never relaxed. She had lost half a pint of blood from the vagina. There was no trace of dilatation of the os. Accidental hæmorrhage was diagnosed. At 2 p.m. she was seen by both of us. There had been no further loss of blood. Her face was blanched and a little puffy. She was restless and had sighing respirations; the pulse was 110 and of good quality. She complained of abdominal pain and she had headache and almost complete blindness. The extremities were rather cold.

Abdominal examination: The uterus was very tender and firm. No fœtal outline could be detected, but the muscles of the abdominal wall became rigid whenever the uterus was palpated. Now and again the patient cried out on account of exacerbations of the abdominal pain and it was noticed that she made no bearing down efforts and that the uterus did not become harder or alter in any way during these attacks.

Vaginal examination: There was blood-stained vaginal discharge continuously dribbling away. It was watery blood rather than blood-stained fluid and contained a few small clots. The cervix was quite closed and no fœtal part could be made out through the fornices.

Urine: By means of a catheter 1½ oz. of blood-stained urine were withdrawn. It was loaded with albumin.[1]

Diagnosis and Treatment.

From the shock, severe abdominal pain, the firm and tender uterus, which had increased in size since first seen by one of us four hours before, and the bleeding from the vagina, we felt fairly confident that the case was one of accidental hæmorrhage. The blindness, headache, scanty urine, accompanied by slight œdema of the face, however, pointed no less clearly to a toxæmic condition.

With regard to the treatment, we were in some doubt as to the best course to pursue. While we had been convinced long ago from a careful consideration of the subject that the graver cases of accidental hæmorrhage, in which the cervix is closed, are, generally speaking, best treated by Cæsarean section, we hesitated to carry out the operation immediately because of the severe toxæmia which co-existed with the accidental hæmorrhage and also because the patient did not appear to be in danger from the hæmorrhage alone. We came to this conclusion because her pulse was not very rapid and because we thought the pallor and restlessness were probably due as much to shock as to hæmorrhage, for the uterus was not much enlarged and no great amount of blood had been passed by the vagina. We decided, mainly because of the toxæmia, to watch the patient for a while and we did not interfere with her in any way. A binder was applied to the abdomen, hot bottles were put in the bed, and drinks of water were freely given to satisfy the thirst.

We remained in the house and saw her every hour or so during the night. The abdominal pain persisted, and blood continued to trickle steadily from the vagina. The toxæmic symptoms did not alter, but there were no convulsions nor marked drowsiness. The pulse varied from 100 to 120 and did not deteriorate in quality. The skin became warm and moist and at 7 a.m. her general condition was considered, if changed at all, a little better. The external os was still quite closed and no urine had been passed. We now came to the conclusion that the least dangerous course to pursue was to deliver by Cæsarean section, for while the hæmorrhage and toxæmia remained the same the shock was less severe.

The patient was accordingly removed to a neighbouring nursing

[1] The urine examined at intervals during the first three months of pregnancy was free from albumin.

home and at 9 a.m., on June 17—i.e., about twelve hours after the first symptoms appeared—the operation was done. When the abdomen was opened the uterus presented a remarkable appearance. It was purple in colour, and there were innumerable small subperitoneal ecchymoses. When the uterine wall was incised, the cut surface was also purple in colour. There was very little oozing from it and no spurting vessels. The uterine cavity contained a good deal of old clotted blood in some watery fluid, the dead child (eighth month) and the placenta entirely detached from the uterine wall and also from its membranes. After these mixed-up contents had been evacuated the uterus was drawn out of the abdomen. It was now observed that the ecchymoses not only involved the whole of the uterus, including the cut surfaces, but the appendages and broad ligaments extending on to the bladder and outwards as far as the cæcum on the right and the mesosigmoid on the left side. The uterine muscle was so broken up by hæmorrhages that it seemed very unlikely material for suturing and on this account in addition to the well-known danger of *post-partum* hæmorrhage occurring, it was decided to remove the uterus. This was done just above the level of the internal os. The cervical stump was sutured by interrupted sutures and the peritoneal surfaces apposed in a similar manner. The abdomen was closed in the usual way. For the hysterectomy part of the operation the patient had been in the Trendelenburg position.

Her appearance now was satisfactory and her pulse not different from what it was before operation. A small quantity of ether had been sufficient to maintain a satisfactory anæsthesia. She was kept in the Trendelenburg position on the heated table of the operating room and 3 pints of saline solution were injected under the breasts. Her condition remained good for the five hours she was kept in this position in the theatre. She was now transferred to bed. In a short time she became collapsed with marked restlessness and with uncountable and often imperceptible pulse. In spite of the usual restorative treatment this condition continued for eight hours, after which she rallied a little. She had a good night after being given morphia, and next day, June 18, had a pulse of 140 and still looked very ill. No urine was passed spontaneously and less than 1 dr. withdrawn by catheter. Her face was œdematous, but the legs and feet were clear. She was almost completely blind. She now complained of an uncommon symptom which we do not remember having met with before and which we cannot explain. Couvelaire, who has described a very similar case to this,

records the same symptom. It was severe epigastric pain, apparently like cramp, and brought on whenever she had a drink. The patient nevertheless took large quantities of fluid. Four drams of urine were withdrawn by catheter in the evening.

June 19: A little better; epigastric pain nearly gone; sight almost normal; perspires freely. No urine passed, but constant dribbling of fluid fæces due to saline purgative. Marked œdema of face still present; feet and legs became œdematous.

June 20: Restless night; had no morphia; sweating very freely; passed urine for first time since illness began, 2 oz.; considerable œdema of lower limbs noticed.

The following report on the patient's urine was received from Dr. Gruner:—

"The deposit in this specimen contains a very large number of squamous epithelial cells and cellular detritus, also much amorphous granular matter. The casts present are all of the granular or hyalo-granular variety. Small epithelial cells of various forms are also numerous; these include tailed cells belonging to stratified epithelium, presumably of either the renal pelvis or the ureter. They are too small for the lower urinary passages, although some larger forms also occur which may be derived herefrom. There is hardly any urea present, and the phosphate and chloride excretion is very low. There is no indican. (Albumin copious.)"

June 21: 3 oz. of urine withdrawn by catheter; respirations shallow; patient slept all yesterday and to-day.

June 22: Sweating; less œdema; blood in urine.

June 23: Eyes examined under homatropine. No details in retina to be distinguished—nothing beyond red homogeneous field; disks themselves very small; edges ill-defined as though encroached upon by swollen retina. No hæmorrhages; no detachments. Sight apparently almost normal again. Urine less deposit.

June 24: First marked improvement. Œdema of face in the morning soon goes off; considerable œdema of feet and legs continues.

June 25: Urine copious, colourless, and opalescent; albumin, pus and detritus.

June 27: Considerable bruising about the abdominal incision, dating from operation; patient constantly drowsy. Very little œdema left.

July 2: Urine, specific gravity 1008; trace of albumin; no œdema anywhere.

July 5 : No albumin.

[From this date convalescence was steady and uninterrupted.]

August 10 : Looks perfectly well and has no symptoms. Ophthalmoscopic appearances normal.

Remarks on the Case.

There are three points to which we wish to draw attention :—

(1) The association of the acute and severe renal affection with the accidental hæmorrhage.

(2) The apoplectic condition of the uterus, the appendages and broad ligaments.

(3) The treatment by Cæsarean hysterectomy.

(1) *The Association of the Acute and Severe Renal Affection with the Accidental Hæmorrhage.*—It is now well recognized that albumin is frequently found in the urine of patients who have accidental hæmorrhage. The case we have described is exceptional in that the renal lesion was so acute and severe that the patient had symptoms resembling the premonitory symptoms of eclampsia and almost complete suppression of urine for thirty-six hours, followed by a period of three or four days during which the urine was scanty, colourless and nearly free from urea. An exceptional case of this character not only emphasizes the connexion between accidental hæmorrhage and albuminuria but leads to speculation on the relationship between the two conditions. Is albuminuria a cause of accidental hæmorrhage? Is it the result of accidental hæmorrhage or are both conditions manifestations of a general toxæmia? We are inclined to favour the latter explanation and think that our case lends support to that view. Accordingly we would suggest that accidental hæmorrhage be added to the list of the clinical forms of toxæmia of pregnancy.

(2) *The Apoplectic Condition of the Uterus and of the Appendages and Broad Ligaments.*—This lesion suggests to us that the hæmorrhage commenced as a capillary hæmorrhage due to the toxæmia, and that from the eruption of the subplacental tissues lacerations in the large blood sinuses were produced ending in free hæmorrhage between the uterus and the placenta, with detachment of the placenta.

(3) *The Treatment by Cæsarean Hysterectomy.*—Many cases of accidental hæmorrhage are very slight and are not diagnosed until after labour. In the common type of case the symptoms are not severe and puncture of the membranes will usually stop the bleeding.

In the severer and rarer forms of the disease the treatment depends largely on the condition of the cervix. If the cervix is dilated or dilatable the problem of the rapid evacuation of the uterus is simple. When, however, the cervix is closed in a severe case of accidental hæmorrhage, surgical intervention seems to be called for. In primiparæ, especially when the pregnancy is near full term, vaginal Cæsarean section is often difficult to carry out satisfactorily. The abdominal operation is a better procedure because the principles of surgery—viz., hæmostasis and asepsis can be more completely carried out; and further, when hysterectomy follows, the raw surfaces can be more thoroughly closed in by the abdominal than by the vaginal operation.

With regard to the question as to whether the uterus should be removed in severe cases of accidental hæmorrhage, probably no rule can be formulated, but we can hardly imagine that any surgeon would hesitate about removing a uterus which presented such a disorganized appearance as the organ with which we had to deal.

Description of Specimens Demonstrated.

Uterus and the appendages: They are of normal size and conformation. Patchy effusions of blood are seen under the peritoneum. Cut surface of uterus is black from similar ecchymoses.

Placenta: Without membranes; half of it is blood stained; half is not; no infarcts.

Fœtus: Eighth month; the only abnormalities observed are chocolate colour of intestines, effusion of blood into subperitoneal tissue and of blood-stained fluid into peritoneal cavity. Liver and spleen appear to be normal.

Microscopic sections of (1) uterine wall: Extravasation of blood into the muscular wall separating and breaking up the muscle bundles. (2) Placenta: Nothing abnormal observed. (3) Liver of child: Some round celled infiltration of perivascular tissues.

(*October* 5, 1916.)

Cæsarean Section and Hysterectomy for Accidental Hæmorrhage.

By W. Fletcher Shaw, M.D.

Cases of accidental hæmorrhage are classified according to their symptoms as "concealed" or "apparent," but a much more practical classification, so far as treatment is concerned, would be into two groups with "contractile" or "non-contractile" uteri. Fortunately the former class is much the more common, and includes not only the cases with a comparatively small amount of hæmorrhage, but those in which the hæmorrhage is severe but responds to treatment. If the vagina is tightly plugged or the membranes are ruptured, a tight binder applied, and ergot or pituitrin given, the uterus in this class of case will contract and prevent any further serious loss of blood, but in the second class, fortunately comparatively rare, no treatment has the slightest effect on the uterine muscle which remains flabby and bleeding. Extraction of the child by the vagina or by Cæsarean section still leaves the uterus relaxed, and the only hope of saving the woman is the rapid performance of hysterectomy. Every experienced obstetrician must have had many of these cases, and must have recognized the utter futility of treating them on ordinary lines, the patient rapidly sinking from loss of blood and often dying while the operator still continues his manipulations.

Targett, in 1905, described a case of this nature which he had saved by Cæsarean hysterectomy, and he also mentions the two points I wish to emphasize, the presence of albuminuria and the occurrence of hæmorrhages under the serous coat of the uterus.

Amand Routh, in his exhaustive paper on "Cæsarean Section in the United Kingdom," published in 1911, mentions three cases treated by Cæsarean section and hysterectomy, and considers that hysterectomy should follow Cæsarean section in those cases sufficiently severe to require this operation.

Lequeux, in 1910, advocated hysterectomy after Cæsarean section for accidental hæmorrhage in those cases "in which the uterus is a

potential danger, either as a cause of secondary hæmorrhage or possible infection, however slight."

Phillips, in 1912, published an account of successful Cæsarean section and hysterectomy carried out in a severe case of accidental hæmorrhage.

Shannon, in 1912, published an interesting case in which he performed this operation, and found multiple small ruptures of the uterus, though he does not state whether there was free blood in the peritoneal cavity or no. He considers these ruptures to be due to over-distension.

Clifford, in 1914, published a case of accidental hæmorrhage treated by Cæsarean section and hysterectomy, in which he found free blood in the peritoneal cavity. This case and two of the cases I now publish, with free blood in the peritoneal cavity, are, so far as I can find in the literature, unique in this respect.

I have now performed this operation upon six patients, in none of whom did the uterus make the slightest attempt to contract after Cæsarean section had been performed, and, in five of these cases, inspection of the uterus showed such extensive extravasation of blood that we could hardly expect complete recovery of the uterus even if the hæmorrhage had been controlled.

Most of the authors I have quoted advocate hysterectomy for concealed accidental hæmorrhage, but of my six cases only one was a case of pure concealed accidental hæmorrhage, the other five all having a large amount of apparent hæmorrhage.

Case I.—This patient, aged 40, was admitted to St. Mary's Hospital, Manchester, on April 22, 1914. She was nine months pregnant with her thirteenth child, and the urine was solid with albumin. She was sent to the hospital because of severe hæmorrhage, for which a doctor had plugged the vagina with gauze. On admission the gauze was removed, the membrane ruptured, and a tight binder applied. Labour pain soon commenced; there was no further severe hæmorrhage, and in three hours a dead male child was delivered. The placenta was delivered very soon after the child. This was followed by some large clots and then free hæmorrhage. Hot intra-uterine douche, ergot and pituitary extract were given, and the excessive flow of blood arrested, though a slow trickle of blood persisted. The patient was now very collapsed, and the pulse-rate had risen to 120. Three hours after delivery the resident obstetrical surgeon gave another intra-uterine douche, packed the uterus with gauze, injected another dose of pituitary extract, but still this trickle of blood persisted. I was sent for seven hours after delivery, and found the patient very collapsed, with an uncountable flickering pulse and a persistent

trickle of blood from the vagina. I gave another hot intra-uterine douche but this had no effect upon the hæmorrhage, though the uterus was apparently contracted. The cervix was not lacerated and from the whole of its internal surface, as far as it could be seen, blood was oozing, and presumably this was happening all over the uterine cavity. I had the patient taken to the theatre and opened the abdomen, in which I found about half a pint of free unclotted blood; both broad ligaments were very much swollen and almost black in colour from very large effusions of blood in the loose cellular tissue, the one on the right being the most extensive and reaching above the brim of the pelvis. The back of the uterus had several slight abrasions on it from which the free blood in the peritoneal cavity had come. I did a panhysterectomy, sutured the opening in the vagina, the round and infundibulo-pelvic ligaments, and covered them with peritoneum. I had great difficulty on the right side in controlling the hæmorrhage from some small vessels ruptured high in the pelvic tissue, as the ends were lost amongst the disorganized cellular tissue, and I had to insert several ligatures round bunches of this tissue before the vessels were satisfactorily controlled. At the end of the operation I found I was perilously near the ureter, but the patient's general condition was so serious that I considered it better to run this risk than to spend any more time over the operation. After the operation we kept the patient in the theatre in the Trendelenburg position for some hours and gave intravenous saline. The patient picked up wonderfully, but only passed 9 oz. of urine in the first twenty-four hours, though the amount of albumin had considerably diminished. In the next fourteen hours she passed 20 oz. of urine, and after this a normal quantity each day, albumin not being present after the fourth day. Three days after the operation diarrhœa commenced, consisting of very offensive watery material containing undigested milk. This diarrhœa continued in spite of treatment until death took place from exhaustion three days later—that is, six days after the operation. During the whole of this time the patient was bright, had no headache or visual disturbances, passed a normal quantity of urine, with a gradually diminishing amount of albumin which completely disappeared after the third day. The temperature was intermittent to 100° F. and once to 101·4° F., and the pulse varied from 100 to 140. There was never any rigidity of the abdomen. At the post-mortem examination there was a small amount of clear fluid in the peritoneal cavity. The intestines were much injected and contained very foul, thin, watery yellow material. The right ureter was caught in a ligature and was distended, as was also the hilum of the kidney. Both kidneys were apparently healthy, both on macroscopic and microscopic examination. The abdominal and pelvic wounds were quite healthy. The lungs were very œdematous but there was no consolidation. The heart was normal.

Specimen: The uterus is enlarged and has been completely removed by panhysterectomy. It is of a deep red, almost black, colour all over except for a V-shaped area on the anterior and posterior walls. This discoloration, and that of the broad ligaments, which have shrunk very much in the preservative

solution, is due to extravasated blood. On the posterior surfaces are several cracks not extending much deeper than through the peritoneal coat. The section through the tissues shows several patches of extravasated blood; these are chiefly in the superficial layers under the peritoneal coat. There is a considerable increase of fibrous tissue but very little increase in the amount of elastic tissue.

[This case was published in the *Transactions of the North of England Obstetrical and Gynæcological Society*, 1914.]

Case II.—M. E. B., aged 41, was admitted to St. Mary's Hospital on February 3, 1915, for severe hæmorrhage. She was seven months pregnant with her seventh child, and there was a considerable amount of albuminuria. The hæmorrhage commenced suddenly at 7.10 a.m., but she was not sent to the hospital until almost midnight. She was then very collapsed, with a pulse of 144 and temperature of 96° F., and the uterus was the size of full-term pregnancy and very tender. She was immediately taken to the theatre and Cæsarean section with hysterectomy was performed. There was about half a pint of free blood in the abdominal cavity and a very large hæmatoma in the left broad ligament. The right broad ligament contained no blood. The uterus was filled with clotted blood, amongst which was the completely detached placenta and a dead child. She made a perfectly good recovery.

Case III.—M. E. H., aged 38, eight and half months pregnant with her eleventh child, was admitted to St. Mary's Hospital on August 28, 1915. There was a considerable amount of albuminuria. This patient was seized with sudden severe abdominal pain at 2 a.m. whilst in bed. She sent for the midwife but though in a collapsed condition she was not sent to St. Mary's Hospital until ten hours later. On admission the patient was very blanched and collapsed, with sighing respiration, and a thin uncountable pulse. The uterus was distended, very tender and there was no sign of external bleeding. She was immediately taken to the theatre and Cæsarean hysterectomy performed, while at the same time intravenous injection of saline was given. The operation only occupied fifteen minutes, but the patient died fifteen minutes later. The uterus was distended with blood and the placenta was completely detached. Both broad ligaments were enormously distended with large hæmatomata, but there was no free blood in the peritoneal cavity. This patient was moribund on admission, but it was decided to perform the operation and give her the only chance, though a slight one, of recovery.

Case IV.—S. N., aged 32, eight months pregnant with her ninth child, was admitted to St. Mary's Hospital on September 15, 1915. There was a large amount of albumin in the urine. Severe hæmorrhage commenced suddenly while she was in bed early in the morning, and she felt faint. The hæmorrhage slackened a little, but continued all day, and she was sent to the

hospital at 4.30 p.m. On admission she was very pale, pulse 140. The uterus was the size of full-term pregnancy, hard and very tender. The cervix admitted two fingers, but the external hæmorrhage was not severe and no placenta could be felt. Cæsarean hysterectomy was performed. The uterus was full of clotted blood; the placenta was entirely detached and the child was dead. The woman was so anæmic that there was no bleeding from the uterine incision. There was no free blood in the abdominal cavity, but there was a very large hæmatoma in the left broad ligament and a smaller one in the right. It was only with the greatest difficulty that the bleeding could be controlled on the left side, as the vessels were lost in the blood-clot, and the tissue as high as the third lumbar vertebræ had to be ligatured in the hope of catching all the vessels. The operation was done as rapidly as possible, but the patient's pulse could not be felt at the end of the operation. She was kept on the operating table in front of a large fire, intravenous infusion of saline given, and also pituitary extract. She gradually recovered, and eventually made a perfectly good recovery.

Case V.—H. H., aged 41, seven months pregnant with her ninth child, was admitted to St. Mary's Hospital on September 15, 1915. There was a large amount of albuminuria. Hæmorrhage commenced suddenly at 4 p.m. with severe abdominal pain, and she was admitted to the hospital three hours later. There was then free external hæmorrhage; the cervix admitted one finger; the uterus was hard, large and very tender; the patient was collapsed and the pulse uncountable. Cæsarean hysterectomy was performed soon after admission. The uterus was filled with masses of blood-clot, the placenta completely detached, and the child was dead. There was a large hæmatoma in the right broad ligament, but none in the left, though the hæmatoma on the right side did not extend over the pelvic brim as occurred in some of the other cases; there was much difficulty in controlling the oozing of the thin watery blood, but the operation on the whole was much easier than some of those just reported. This patient made an uninterrupted recovery.

Case VI.—M. R., aged 39, eight months pregnant with her thirteenth child, was admitted to St. Mary's Hospital on September 23, 1915. There was much albuminuria. Severe external hæmorrhage commenced in the afternoon, and a district nurse who saw her sent her to the hospital at once. On admission her general condition was fairly good, pulse 126, and temperature 98·2° F. The resident obstetrical surgeon ruptured the membrane, applied a tight binder, and gave pituitary extract, but no contractions of the uterus occurred, and the hæmorrhage continued. Later in the evening I was sent for, and did Cæsarean hysterectomy. There was no free blood in the peritoneal cavity, and no hæmatoma of the broad ligaments. The uterus was full of blood-clots and the placenta was completely detached. The uterus made no attempt to contract after extraction of the child. This operation was performed much more methodically than any of the foregoing as the patient was in a much

better general condition, and two ligatures were placed round each uterine and ovarian artery. At the end of the operation she was in a fairly good condition, with a pulse of 120. Six hours later she was restless and cold, with sighing respiration, but her pulse was only 110, so we looked upon her condition as one of shock. Very soon afterwards she died. Post-mortem examination showed that the ligatures round the right uterine artery had slipped and the patient had bled to death, though we had been misled as to her condition by the pulse-rate not increasing.

The operation performed in each case, except the first, was Cæsarean section through a longitudinal median abdominal and uterine incision. After extraction of the child, the round and infundibulo-pelvic ligaments on both sides were seized with pressure forceps and cut, the utero-vesical peritoneum incised, the uterine arteries on both sides seized and cut and then the cervix cut across about the level of the internal os. After the cervix had been sutured and the uterine arteries and the round and infundibulo-pelvic ligament ligatured, the cut edges of the peritoneum were drawn over the raw surface of the cervix and broad ligaments, and the abdominal wound closed in three layers. This is one of the operations in which time is an essential factor, every minute saved, in some of the cases, being of the greatest importance: one of the operations was completed in fifteen minutes, though needless to say, no time was lost in peritonizing the pelvic wound and in stitching up the abdomen in layers.

All these six cases were of the severest type of accidental hæmorrhage, and would undoubtedly have died if they had not been operated upon. They were all suffering very severely from the effects of the hæmorrhage, and in none of the cases did the uterus show the slightest sign of contraction after removal of the child, so there was no option but to follow the Cæsarean section by hysterectomy. The final result was three deaths and three recoveries, but one of the deaths was due to the accident of a slipped ligature, which has only once previously occurred to me, and this case I believe would have been saved if I had opened the abdomen again. I thought, however, she was suffering from shock, as her pulse-rate did not increase; at the end of the operation this patient was in a much better general condition than any of the others.

With regard to the other two deaths, one patient was practically moribund at the time of admission, and I only did the operation in the very faint hope that she might be saved; she survived the operation only a quarter of an hour. This patient was sent to the hospital ten

hours after the onset of symptoms and her life would almost certainly have been saved if the midwife had realized the gravity of her condition earlier. The other death took place six days after the operation with the symptoms of toxæmia. A mortality of 50 per cent. is extremely high, but the patients representing the other 50 per cent. would undoubtedly have died if the operation had not been performed. As was to be expected all the children were dead, the placentæ being completely detached.

The outstanding feature of these cases was the condition of the uterus; all, but one, containing large hæmatomata of one or both broad ligaments, in some cases extending far above the pelvic brim and, in two of the cases, the condition had extended so far that the peritoneum had cracked and free blood had been poured into the peritoneal cavity. In the first case—the patient operated upon for hæmorrhage after the birth of the child—the uterus had been douched and massaged frequently, and it is possible the lacerations on its surface and the hæmatomata in the broad ligaments may have been caused by trauma, though it had not been treated more vigorously than many uteri which have symptoms of *post-partum* hæmorrhage. Of the remaining five cases, four had large hæmatomata in the broad ligament and one of these also had free blood in the peritoneal cavity; no treatment had been attempted in the case of these patients before the abdomen was opened, so the hæmorrhage must have been due to some local condition of the uterus or some general condition of the patient.

None of the six cases I now record had the slightest chance of surviving had not the operation been performed, as not one of the uteri made the slightest effort to contract, except the first, and that maintained a persistent trickle of blood which almost killed the patient. But the condition found at the operation, hæmatomata of the broad ligament and free blood in the peritoneal cavity, quite unsuspected previously, should induce us to consider whether some of the less severe cases would not be better treated by Cæsarean section, followed by hysterectomy if necessary, than by the more conservative methods.

Pathology.

All these uteri show an increased amount of elastic and fibrous tissue, especially around the blood-vessels, but not more than is usually found in multiparous uteri. Apart from this I can find no change,

except extravasated blood in the uterine wall, more marked in the superficial layers, especially under the peritoneal covering. Couvelaire considers this a constant feature in accidental hæmorrhage, but we must look upon it as the result of the condition, not the cause. In these cases the condition had gone much further; not only was the blood extravasated into the uterine wall, but it had extended into the broad ligaments to form large hæmatomata, and in two cases had broken through the peritoneal covering of the uterus, and had trickled into the peritoneal cavity.

Another constant feature was the presence of albuminuria. This occurs in the great majority of cases of accidental hæmorrhage, and I find it exceptional to meet with a case, slight or severe, in which this condition is not present.

The older teaching was that accidental hæmorrhage was due to an accident, and I well remember, as a student, being warned not to allow pregnant women to do any work which necessitated the arms being raised above the head, such as the lifting of plates from a shelf, &c. It is very exceptional to see a case even remotely connected with an accident, and a large proportion commence to bleed during the night while in bed.

We must look much deeper to find the cause of this condition. In my opinion it will be found in some general condition of the patient, probably a toxæmia. My chief reason for this belief is founded on the large number of patients with accidental hæmorrhage who have albuminuria which entirely disappears a few days after confinement, not persisting as it does in cases of chronic nephritis.

This, of course, opens up a wide field for research, but when the toxin or toxins are found which produce eclampsia, acute yellow atrophy of the liver, hyperemesis gravidarum and chorea gravidum, it is more than likely we shall find them present in cases of accidental hæmorrhage.

Note.—I wish to emphasize that I only advocate Cæsarean hysterectomy for those severe cases which will not respond to ordinary methods of treatment, but remain flabby, relaxed and bleeding.

BIBLIOGRAPHY.

CLIFFORD, H. "Concealed Accidental Hæmorrhage accompanied by ntraperitoneal Hæmorrhage," *Journ. Obst. and Gyn. Brit. Emp.*, 1914, xxv, p. 48.

COUVELAIRE, A. "Traitement chirurgical des Hémorrhagies utéro-placentaires avec décollement du placenta normalement inséré," *Ann. de Gyn. et d'Obstét.*, 1911, 2me sér., viii, pp. 591-608.

KING, W. W. "A Uterus infiltrated with Blood removed for Concealed Accidental Hæmorrhage," *Trans. North of England Obst. and Gyn. Soc.*, 1914, p. 93.

LEQUEUX. "Hémorrhagie rétro-placentaire," *Bull. de la Soc. d'Obstét. de Par.*, 1910, xiii, p. 412.

PHILLIPS, MILES H. "Case of Concealed Accidental Hæmorrhage treated by Cæsarean Section and Hysterectomy," *Journ. Obst. and Gyn. Brit. Emp.*, 1912, xxi, p. 163.

ROUTH, AMAND. "On Cæsarean Section in the United Kingdom," *Journ. Obst. and Gyn. Brit. Emp.*, 1911, xix, pp. 1-233.

SHANNON. "Some Observations on Concealed Accidental Hæmorrhage," *Glasgow Med. Journ.*, 1912, lxxvii, p. 290.

SHAW, W. FLETCHER. "Accidental Hæmorrhage with Free Blood in the Abdominal Cavity," *Trans. North of England Obst. and Gyn. Soc.*, 1914, pp. 80.

TARGETT, J. H. "Abdominal Hysterectomy for Severe Concealed Accidental Hæmorrhage," *Journ. Obst. and Gyn. Brit. Emp.*, 1905, vii, p. 344.

DISCUSSION.

Dr. AMAND ROUTH: It is interesting to note that, in 1,280 cases of Cæsarean section collected by me from obstetricians living in Great Britain and Ireland in 1910, only four such operations had been performed for concealed accidental hæmorrhage—namely, by Bagot of Dublin, Briggs of Liverpool, Targett of London, and Savage of Birmingham, with two recoveries. I think if there is evidence of continued concealed hæmorrhage which does not yield to treatment, the abdomen should be opened and Cæsarean section performed, and if, owing to absence of retraction, hæmorrhage persists, subtotal hysterectomy should be performed. I believe that the epigastric pain to which Dr. Oldfield has referred is due to the associated toxæmia, of which condition it is a frequent symptom in severe cases.

Dr. HERBERT SPENCER: This meeting will probably mark a stage in the history of accidental hæmorrhage, the communications on intraperitoneal and intraligamentary hæmorrhage being of great importance. The ecchymoses and intramuscular hæmorrhages are not at all rare; I have seen them at post-mortem examinations of puerperal uteri of toxæmic patients and others. Some of them appear to be due to pressure on the uterus by the attendant; in some of the cases shown they seem to be due to overstretching of the uterus. Pure concealed accidental hæmorrhage (in which not a drop of blood escapes from the vagina) is extremely rare; in thirty years I have only seen one instance of it. In that case I did conservative Cæsarean section, notwithstanding the presence of ecchymoses and intramuscular hæmorrhages, and the patient made

a simple recovery. These slight hæmorrhages in aseptic cases form no indication for the removal of the uterus, and even the more extensive intra-peritoneal and broad ligament hæmorrhages might, perhaps, be treated more quickly and simply by drainage than by hysterectomy. If, however, the uterus is removed—and I do not go so far as to say it is never necessary—the organ should be removed by total hysterectomy and the vagina left open for drainage. I think Dr. Oldfield's case, in which severe epigastric pain occurred after food, may be regarded as an exaggerated example of the epigastric pain so frequently met with in the toxæmia of pregnancy.

Mr. H. BRIGGS (Liverpool): I share, with others present, the keen appreciation of the papers just read. The pathological evidence, still incomplete, enforces an open mind as to treatment. The possible distribution of the hæmorrhage encourages abdominal section, but hardly justifies the removal of an organ itself only participating in a morbid manifestation. The broad ligament hæmorrhage is not due to extraneous pressure as I thought possible in 1904, when I had its naked-eye appearances copied by an artist after the death of a patient treated by a tight abdominal binder and vaginal tampon (the water-colour drawing is produced). The intraperitoneal hæmorrhage, apart from actual uterine rupture, has been associated with uterine peritoneal fissures. Is it conceivable that high intra-uterine tension may establish a Fallopian tube as a conduit of blood from a concealed uterine accidental hæmorrhage? Early this year, when an acute abdomen complicated a labour with a dead fœtus near the full term, there developed an intraperitoneal effusion of 3 pints of liquid blood, which were drained by abdominal section twelve days after the labour. The extraneous side of the question was revealed in the patient's previous history of treatment for gastric ulcer in a general hospital. The matting of the undilated left Fallopian tube on the lower portion of the retro-omental blood sac was suggestive, but too inconclusive, for an obstetrical debate. Whitridge Williams has recently fully discussed the eclamptic state as a cause of accidental hæmorrhage. In the treatment of severe cases of accidental hæmorrhage I have been driven to regard active surgical effort as promoting the tide against the patient. In the light of to-night's proceedings further trials must be made; more work and more evidence are required.

Mr. G. F. DARWALL SMITH: I have come across one case of free intra-peritoneal bleeding associated with concealed accidental hæmorrhage, but before I relate it perhaps I may be allowed to state the reasons which influenced me in treating it in the way I did. When I was a student I was struck with the fact that in more than one case of concealed accidental hæmorrhage death occurred a few hours after delivery *per vias naturales* when delivery had been hastened, even though the uterus retracted well and there was no *post-partum* hæmorrhage. Later, I was also struck by the fact that a woman may be in a profound state of shock and collapse, due to concealed accidental hæmorrhage, even though there is no more than about half a pint of blood in the uterus, and scarcely any has been lost externally.

These considerations seemed to point to the woman's condition being due, not only to collapse from loss of blood, but also to nervous shock associated possibly with the sudden distension of the uterus. A patient in a condition of shock, as distinguished from collapse due to bleeding which is still going on, is not in a good condition for a severe abdominal operation. Hence, it seemed that, if the bleeding could be controlled temporarily while she recovered from the shock to some extent, it would be safest to allow her to deliver herself, either naturally or with artificial help, when her general condition had improved. In one case I attempted to eliminate the element of shock by spinal anæsthesia, but unfortunately, as sometimes happens with this method, the shock was definitely increased for a short time, though the woman eventually delivered herself naturally and safely. Generally speaking, I have adopted the method of combating the initial shock, which was, I think, first proposed and practised in Canada, by the use of morphia and saline infusion, at the same time attempting to control the hæmorrhage by vaginal plugging and pituitary extract or ergot, with or without rupture of the membranes. And my efforts have been attended generally with success, but I have come to realize that there are two conditions which may let one down badly when this method is adopted: one is intraperitoneal bleeding, and the other is extensive extravasation of blood into the wall of the uterus, in the presence of which nothing on earth will make it contract. Hence, I feel sure that in all severe cases in the future I shall open the abdomen without delay, but I would suggest that even then it is important to avoid adding to the shock already present by making use of local as well as general anæsthesia, which can be effected very rapidly. The history of the particular case I have to relate is as follows: The patient was a married woman who had had nine previous labours, all of which had been natural. At 11 a.m. on December 1, 1915, when near full time, she had some moderate hæmorrhage *per vaginam* and severe pain in the abdomen and she fainted. There were no labour pains. At 4 p.m. she was admitted into St. George's Hospital with a pulse of 120 and the typical signs of concealed accidental hæmorrhage. There was practically no external hæmorrhage. She was not in labour. She was treated at first with vaginal plugging, a tight binder and a T-bandage, and with morphia, pituitary extract, and subcutaneous saline, but her general condition steadily deteriorated, so I removed the uterus, unopened with the child inside it, in order to make the operation as bloodless as possible on account of her very serious condition. Normal saline was infused into the veins during the operation. The peritoneal cavity contained from 8 to 10 oz. of free blood, which had apparently come from two vertical cracks from 3 to 4 in. in length, extending through the peritoneum and into, but not through, the muscle of the posterior wall of the uterus near the fundus. There was about 1 pint of retroplacental blood-clot in the uterine cavity. The concealed blood was entirely behind the placenta, and the partial uterine ruptures were on the peritoneal surface over the placental site. There was no interstitial hæmorrhage in the wall of the uterus. The patient's condition was desperate at the end of the operation, and she unfortunately died nine hours

later from hæmorrhage from a small vessel in the flap of peritoneum near the bladder, which was not oozing when the abdomen was closed but must have started to bleed when the patient's pulse improved shortly after operation.

Professor MUNRO KERR: My opinion is that while a considerable number of cases of "concealed accidental hæmorrhage" are toxæmic in origin a very large number are not. Concealed accidental hæmorrhage of a pure type is hardly ever found in primiparæ—the individuals peculiarly liable to toxæmia. Of the cases in which I have performed abdominal section I have seen two types: one, where the uterus, tubes, and broad ligaments contained numerous hæmorrhages such as have been described by the readers of to-night's papers; and another where the blood was entirely pent up between the placenta and uterine wall. It would be most unfortunate if an extreme position were adopted, and all cases of "concealed accidental hæmorrhage" labelled "toxæmic." Regarding the treatment, I am surprised that none of the authors of the papers has referred to the most important contribution to the subject made by Professor Essen Möller of Lund (Sweden). This author has shown by his recorded cases that "hysterectomy" is not necessary even although the uterine wall contains many hæmorrhages. The question of "conservative" Cæsarean section versus hysterectomy in this connexion is not, however, of much importance—it is really almost entirely academic; for after all a woman who has given birth to twelve or thirteen children, as was the case in some of the examples referred to to-night, has done so well by the family and State that the removal of the uterus is not of the same moment to her as it would be to a young woman.

Dr. HASTINGS TWEEDY: It has been a pleasure to listen to the reading of three such suggestive papers. Professor Müller first drew attention to the association of accidental hæmorrhage with the toxæmias of pregnancy. I have no doubt that hæmorrhage is often due to toxæmias, and in last year's clinical reports of the Rotunda Hospital the two conditions were associated. I agree with the writers of the different communications that concealed accidental hæmorrhage not uncommonly arises from rupture of vessels in the broad ligament and vessels on the peritoneal surface of the uterus, and when these regions are the source of bleeding there is almost always a toxic origin forthcoming. As regards treatment, I should consider these forms of toxic hæmorrhage as indications for abdominal Cæsarean section, but for all other accidental hæmorrhage I should prefer treatment by the vaginal plug, no matter whether the membranes have or have not ruptured, or whether the hæmorrhage is external or concealed. The objections to the vaginal plug arise from a faulty conception as to its mode of action. It is stated in text-books that its purpose is to well back the hæmorrhage, and to retain it within the cavity of the uterus until the pressure there equalizes the pressure in the blood-vessels. No one has ever seen the plug acting in this way, nor on its removal has more retained blood been found than can be accounted for by the amount poured out during its application. It is certain that the plug when properly applied exercises direct pressure on the uterine vessels. These vessels, running on the surface

of the large spherical uterus, come into close contact with the lateral fornices. The fornices are unyielding in the unimpregnated state, due to the support they receive from the so-called Mackenrodt's ligaments. These ligaments are in reality portion of the tendinous extremities of the uterine muscle fibres. When the internal os is closed they are tense, but when the os opens in labour they become flaccid and permit the fornices to be pushed high up into the abdomen. Under the circumstances the plug can be felt above Poupart's ligament, and in this situation it is impossible to prevent it obstructing the uterine circulation. The mortality of treatment by abdominal section is at present very high, and there does not seem any immediate prospect that it will be materially lessened in the future. It at present compares very unfavourably with that obtained under use of the vaginal tampon.

Mr. T. G. STEVENS: Real cases of concealed accidental hæmorrhage are always desperate conditions, and cannot be dealt with in the same way as cases of external accidental hæmorrhage. The uterine muscle in concealed hæmorrhage is paralysed, and will continue to distend as long as blood is being poured out into it. Possibly such a uterus may gradually recover and retract after the emptying of its contents, but in the meantime further bleeding will occur which will probably cause death. I consider that in any case such a uterus is a menace to the future health of the individual, and that there need be no hesitation in performing hysterectomy, if it can be done by a shockless method of operating. It is practically impossible to guarantee that there will not be a loss of at least one pint of blood during the operation of Cæsarean section, and this amount may well turn the scale against the patient. It is my opinion, therefore, that the safest treatment is to perform hysterectomy at once, without opening the uterus at all. In one case I performed the operation with complete success. As the child is always dead in these cases the conservative operation need not be considered on its account.

Dr. FLETCHER SHAW (in reply): From the discussion upon my paper I find there has been some misunderstanding of it, as some speakers evidently thought I was advocating Cæsarean hysterectomy as a routine treatment for accidental hæmorrhage. Nothing was further from my mind. I only performed it for those cases which would not respond to the ordinary methods of treatment. Rupture of the membranes, accompanied by hypodermic injections of pituitrin or ergot, or packing the vagina tightly with gauze, cure the great majority of cases of accidental hæmorrhage by causing uterine contraction, but there remain a few cases upon which these methods of treatment have not the slightest effect; the only chance of saving the patient is Cæsarean hysterectomy. After the last case reported in this paper, eleven months elapsed before I saw another case of accidental hæmorrhage which did not respond to routine treatment, and for which I had to do hysterectomy, although I had seen many cases of accidental hæmorrhage during this time. I do not agree with Dr. Hastings Tweedy that packing the vagina is the best

routine method of treating accidental hæmorrhage. I prefer rupture of the membranes, with hypodermic injections of pituitrin, but this is beside the present discussion: to avoid this side issue, and to keep the discussion entirely upon the subject of the paper, I mentioned these two methods of treatment together and as of equal importance. Dr. Herbert Spencer has raised the question of panhysterectomy as opposed to subtotal hysterectomy: this again hardly comes within the scope of the discussion, though I much prefer the latter operation as being more rapid and free from many of the objections of the former, but this would be better treated in a separate discussion. I quite agree with Dr. Spencer that pure concealed accidental hæmorrhage is a very rare condition: one of the six cases now published was of this variety; but altogether I have only seen a very few cases.

Section of Obstetrics and Gynæcology.

President—Dr. G. F. Blacker.

(*November* 2, 1916.)

SPECIAL DISCUSSION ON THE NEED FOR IMPROVEMENT IN THE CARE OF PREGNANT WOMEN.

The Need for Improvement in the Care of Pregnant Women, and a Direct Means to that End.

By S. G. Moore, M.D. (Huddersfield).

I have taken the liberty of slightly extending the scope of what I wish to lay before you this evening. Permit me in the first place to direct attention to the fact that this subject is of national importance, that it is of such magnitude that it may, and ought to be, considered on national lines, and hence that we should avoid and disregard questions of local interest.

It may be the case that physicians and surgeons, whose daily work it is to deal with cases of diseases and injuries, come unconsciously to regard their presence in a community as unavoidable. But in reality they are preventable. In the absence of violent death the normal human life ought to progress from infancy, through youth, middle, and old-age, to senile decay and death. So also in the case of pregnancy. Considered strictly, discomfort, disease and death associated with childbearing are abnormal. The reproduction of the race is a physiological—not a pathological—process, but under existing conditions, in industrial England, to-day the unfortunate paido-poietic[1] expects to suffer distress and pain, and to risk death. Hence, in part

[1] *Paidos* = a child ; *poico* = I make.

at least, the reduction of the birth-rate. This aspect of the subject is important, and will repay more detailed consideration. How much of the unwillingness to bear children revealed by the falling birth-rate, is due to the fear among women of distress and suffering, and the risk of death? It cannot be measured; no figures can be given—obviously. In a volume issued by a women's social and political organization having numerous members, hundreds of "horrible examples" are furnished of the unendurable lot of pregnant women among the industrial classes. The main object of the volume is to secure amelioration, but defence of various nefarious and criminal practices is thinly veiled. Are the "horrible examples" well founded? Unquestionably so. Are they sufficiently numerous to constitute valid argument or a serious menace? The answer is doubtful; yet it is significant that the volume was issued; its source is *more* significant; and perhaps *most* significant of all is the fact that such an investigation as formed its basis was felt to be called for by those who carried it out. Examine the case from whatever aspect we may, I feel that there is substantial ground for the belief that some of the reduction in the birth-rate results from this cause.

A certain number of women seek medical help for conditions resulting from pregnancy. How many suffer but do not seek such aid? How many regard their sufferings as inevitable? Again, of all the causes and forms of death, among the most lamentable and calamitous is that of the young mother. Her removal from her family calls forth commiseration from all. When we are made aware that on an average 3,500 of such cases occur each year in England and Wales, we find that the matter is of no small moment. It is to be remembered in addition that a far greater number of women endure preventable suffering, and disablement—sometimes life-long.

In the report on the subject of "Maternal Mortality in Child-bearing," by the principal medical officer of the Local Government Board (Dr. Newsholme), several considerations are set forth which demonstrate that many of these deaths are preventable. In the same report a recommendation occurs that intensive investigation of the causes thereof should be made. In the accompanying table (*see* p. 39), details of the mortality in childbed in Huddersfield are shown. The return is a compilation of the entries in the death returns for Huddersfield for *ten years and ten months*, from January, 1906, to October, 1916, inclusive. The entries in the death certificates are classified according as they fall into groups, and these groups are

DEATHS FROM PARTURITION IN THE COUNTY BOROUGH OF HUDDERSFIELD DURING THE PERIOD JANUARY, 1906, TO OCTOBER, 1916.

	1906	1907	1908	1909	1910	1911	1912	1913	1914	1915	1916	Total	
Albuminuria and eclampsia	1	1	1	—	—	—	—	—	—	—	—	3	36
Albuminuria	—	—	—	—	—	1	1	—	—	1	—	3	
Eclampsia	—	1	7	7	1	1	3	2	5	1	2	30	
Puerperal fever	3	2	2	4	3	1	1	3	1	—	1	21	32
Septic absorption	1	—	—	3	—	1	1	1	—	2	—	9	
Peritonitis—convulsions	1	—	—	—	—	—	—	—	—	—	—	1	
Pneumonia	—	—	—	—	—	—	—	—	1	—	—	1	
Hæmorrhage	1	—	1	—	3	—	—	2	2	1	2	12	24
Placenta prævia	—	1	1	1	1	2	1	—	—	1	1	9	
Ante-partum hæmorrhage	—	—	—	—	1	—	—	—	1	—	—	2	
Retained placenta	—	1	—	—	—	—	—	—	—	—	—	1	
Cæsarean section	—	2	1	—	—	1	—	—	—	—	—	4	11
Contracted pelvis	—	—	—	1	—	—	—	—	1	—	1	3	
Parturition	2	—	—	—	—	—	—	—	—	—	—	2	
Parturition—difficult craniotomy	1	—	—	—	—	—	—	—	—	—	—	1	
Induced labour	—	—	—	—	—	1	—	—	—	—	—	1	
Embolism—pulmonary	2	—	—	—	—	—	1	—	3	1	—	7	10
Embolism—cerebral	1	—	—	—	—	—	—	—	2	—	—	3	
Ectopic gestation	—	—	—	—	—	2	—	—	—	1	—	3	3
Vomiting of pregnancy	—	1	—	—	—	—	—	—	1	—	—	2	13
Prematurity—endocarditis	1	—	—	—	—	—	—	—	—	—	—	1	
Heart disease—organic	—	—	—	—	—	—	—	—	—	1	—	1	
Purpura hæmorrhagica—abortion	1	—	—	—	—	—	—	—	—	—	—	1	
Phlegmasia alba dolens	—	1	—	—	—	—	—	—	—	—	—	1	
Ovarian tumour—ruptured uterus	—	—	1	—	—	—	—	—	—	—	—	1	
Meningitis—convulsions	—	—	—	1	—	—	—	—	—	—	—	1	
Malnutrition	—	—	—	—	—	—	—	—	—	—	1	1	
Anæmia	—	—	—	—	1	—	—	—	—	—	—	1	
Gangrenous hæmorrhoids after child-birth	—	—	—	—	—	—	—	1	—	—	—	1	
Cystitis, acute	—	—	—	—	—	—	1	—	—	—	—	1	
Mania	—	—	—	—	—	1	—	—	—	—	—	1	
Totals	15	10	14	17	10	11	9	9	17	9	8	129	

arranged in order of importance. The first group represents the deaths from albuminuric conditions; the second from septic infections; the third from hæmorrhages; the fourth from pelvic conditions; and the fifth from embolism. Thereafter the numbers become inconsiderable.

I think that the members of the Section will agree that some at least of the first and largest group need not have died had the paidopoietic come under the care of a skilled physician even a couple of months before the end of her period, while the second group, almost as important numerically, we know by experience to be entirely eliminable. How many of the cases in the third group could have been prevented it is more difficult to say, while the fourth group, though perhaps less so, still presents some doubt. The remainder are so small that they need not detain us.

I trust that enough has been said to establish the fact that many of the deaths in childbed are preventable, not theoretically, but practically and actually, as I hope to show later.

One's natural tendency when one arrives at this stage of consideration of the problem is to ascertain the influence of the nature of attendance at confinements—this is suggested in the Local Government Report: Thus if a ratio could be established between the total births attended by doctors, and the total births attended by midwives, and the number of deaths in childbed among the practice in each, it seems that some useful information might emerge. For example, if it could be shown that the death-rate was substantially heavier among persons attended by midwives than by doctors that would be an argument, perhaps, for the elimination of the former, or at least for the taking of much greater care in their training. It is not feasible to find this ratio in Huddersfield at present, and probably it is equally difficult elsewhere. Moreover I very much doubt whether it is worth the while, because I think it will be agreed that when a woman is actually in childbed, except where septic conditions are concerned, and frequently also in these cases, it is too late to save her life. Again, with the head impacted in a contracted pelvis, or in the presence of eclampsia, the condition is likely to be desperate.

A Direct Means to secure Improvement in the Care of Pregnant Women.

Proceeding on the assumption of the need for an improvement, let us consider how that can best be effected. I submit that this is not an appropriate subject for partial, tentative, and ineffective measures; the best ought to be adopted.

Some twelve years ago I saw a paragraph in one of the medical journals to the effect that in a Commune in France for ten years the infant mortality figure had been zero. I investigated the paragraph, and there emerged what seemed to me to be a plain, simple, and *unobjectionable* need—namely, that the sanitary authority should be made aware of the existence of members of the community who were dying in large numbers before their very existence in the community was known to the authorities whose duty it was to take means to prevent their deaths. I refer, of course, to infants. I introduced such a system in Huddersfield in 1905-06, and in the following year Parliament saw fit to apply that system to the whole country. To my complete surprise and substantial regret my professional brothers objected. Yet I feel sure that the historian who deals with the matter, after time has enabled an unbiased judgment to be formed, will express astonishment, not that this measure was taken, but that its absence had escaped the attention of a civilized community for so long.

In my judgment the notification of pregnancy is a strictly comparable proposition. The circumstances are parallel, mothers are dying in excessive numbers just as children died, and still die in excessive numbers, from preventable causes. The majority of them do not come under the care of a duly qualified, and legally registered, medical practitioner until their condition is desperate, and until the opportunity of taking measures to prevent catastrophe is overpast.

One hears tell of "violation of the sanctity of the home," and of "trespass on the holiest feelings of a woman." To my mind this is merely a survival of mediæval conceptions. Pregnancy cannot be concealed, and ought not to be concealed, whether it be legitimate or illegitimate, and since it cannot be concealed what valid objection is there to comply with a provision which has for its sole and exclusive object the welfare of the woman and her baby?

Let us contemplate for a moment two classes of hewers of stone, one erecting those edifices which, dedicated to the glory of God, yet remain monuments of the glorious labour of man—I refer to the temples and fanes of this and other countries; the other excavating stone in the quarries, or hewing paving stones. To which category ought medical men to belong? Voluntarily, willingly, vauntingly joining in the good work of preventing this family calamity, this source of national weakness, this discredit to medicine; or merely complying with the law of self-preservation by uncongenial labour.

I trust that the medical profession will not in this matter repeat the

mistake—it was undoubtedly a mistake—into which it fell in connexion with the notification of births. Given certain conditions which are sane and reasonable, and will be agreed to by everyone—namely, that on receipt of a notification of pregnancy, if the woman be visited or examined at all she shall be visited and be examined by a duly qualified, and legally registered, medical practitioner, and secondly, given that no treatment shall be afforded by the sanitary authority, that it confine itself to its true function, the prevention of disease and death, but that each case be referred for treatment to the family doctor—I see no reason whatever why the profession of medicine should not accept generally, willingly and cordially the proposition for the notification of pregnancy.

A voluntary system of notification of pregnancy has been in operation in Huddersfield since January 1, 1916. A fee of 2s. 6d. is paid to doctor or midwife (not to others) for each notification, subject to the consent of the woman having been obtained beforehand. Each case is visited by a duly qualified and legally registered medical practitioner. No treatment is undertaken. Suitable cases are referred to the family doctor. Material aid is obtained from philanthropic persons or organizations wherever necessary. It is not furnished by the sanitary authority.

It is too soon to report conclusively on the matter, but it may be of interest to set forth the experience so far:—

Period January 1 to October 31, 1916.

Number of births notified	1,536
Number of pregnancies notified	156
Percentage of total	10·1
Notified by doctors	8
Notified by midwives	148
Number of uncomplicated pregnancies	130
Number of complicated pregnancies	26
Percentage of complicated cases	20

Types of Complications.

Varicose veins	7
Extreme anæmia	6
Vomiting (excessive)	2
Albuminuric symptoms	2
Phthisis	2
Serious domestic difficulties	2
Hæmorrhoids	1
Contracted pelvis	1
Deformities	1
Prolapse of uterus	1
Sepsis	1

COMPARISON OF TWO CASES OF PREGNANCY SHOWING ALBUMINURIC SYMPTOMS.

Case I notified under pregnancy scheme.
Case II not so notified.

Case I.—S. M., notified under pregnancy scheme by midwife, February 2, 1916. Condition: Marked œdema of feet and legs; headache past fortnight; eyesight defective; occasional dizziness; vomiting of pregnancy troublesome. Advice: Doctor to be called in at once. February 10: Under doctor; better. March 31: Confined; healthy, full time child; placenta adherent. April 11: Mother up, anæmic; Child well, breast-fed. July 10: Mother and child well.

Case II.—H. M., not notified under pregnancy scheme; same midwife engaged. May 8, 1916: Child born; mother seized with eclamptic fit and died in infirmary; had had swollen legs, acute vomiting, and headache some weeks before confinement; "thought it normal with pregnancy and sought no advice."

One word in conclusion. The birth-rate is falling to an alarming extent. The death-rate also has fallen substantially during the past generation. There is no limit below which the former cannot be reduced, but with respect to the latter, nature does set an inexorable limit. We must remember that the death-rate in England and Wales has approached such a figure that if it falls two or three more per thousand the mean age at death will be 100 years.

The position is rapidly approaching when the population of this country will commence to decline. But the German, Austrian and Turkish nations, as well as others, are more prolific, and in the absence of war, pestilence and famine, increase prodigiously. If it be our desire and intention to prevent our descendants becoming slaves, then we must seriously take steps to prevent loss of life in every direction. No doubt these statements are familiar to all, but I think that they cannot be repeated too frequently.

The mother dying in childbed presents not only her own sad case, but from the national point of view—and I hope that no other will be considered—the surviving members of her family are rendered less efficient, they have less chance of surviving, and she has ceased to be a source, a fount and origin of defenders of the Homeland.

The Importance of getting a Pregnant Woman under Medical Supervision and affording Her the Necessary Treatment.

By Amand Routh, M.D.

Soon after our Council asked me to read a short paper on this section of to-night's subject I read in the *British Medical Journal* (July 8, 1916, p. 33), an important monograph on "The Care of the Pregnant Woman" by Dr. Archibald Donald of Manchester. Personally I agree with almost everything Dr. Donald said in that paper except his three summing-up conclusions, which seem to me to oppose some of his arguments. As these conclusions appear to discountenance universal medical supervision of pregnant women, I propose to use them as a text upon which to base my remarks, for his conclusions show that the question of such supervision needs more discussion than I thought was necessary.

His first conclusion is "That the supervision of all pregnant women would mean a great deal of unnecessary trouble, as in the majority of cases in which danger threatens during pregnancy, the patient will voluntarily apply for help."

His second conclusion is that "even if supervision were greatly increased, the results in the saving of infant life would be comparatively small."

His third conclusion is that "there are more important causes of fœtal death than the diseases of pregnancy, and these would not be dealt with in a scheme of supervision during pregnancy. Under these he would mention particularly abortions and stillbirths caused during delivery."

Surely the fact that stillbirths are often due to "accidents of childbirth" as the Registrar General calls them, is no argument against the supervision of pregnant women, for such accidents are often due to undiscovered pelvic abnormalities. Nor do I see that even if "greatly increased supervision" would only result in "comparatively few" fœtal lives being saved, it is any argument against such increase of supervision, rather the contrary. Such "few lives" might include a Shakespeare or a Kitchener.

Dr. Donald's first conclusion that such increase of supervision would cause unnecessary trouble because women would come voluntarily for

help in cases where danger threatens, does not seem borne out by one's own experience or by Dr. Donald's own statements. Many complications are subjectively unsuspected. Perhaps the expression "medical supervision" needs defining. It does not necessarily mean continuous or frequent interviews between doctor and patient. Primarily it means one interview and a preliminary examination of urine. Subsequent interviews and urine tests take place as the doctor thinks desirable, or when the patient notices some sign or symptom which disturbs her.

If Dr. Donald is right in saying that increased supervision is unnecessary, because its results would be comparatively small, I would ask how it is that cases for emergency Cæsarean operations for unrecognized pelvic contractions continue to be sent to our hospitals. Some of these cases, owing to previous attempts at delivery, are already septic and their mortality is high.

How is it that among the poorer classes emergency admissions to our hospitals of cases of eclampsia are still so numerous? Thus at St. Mary's Hospital, Manchester, Dr. Fletcher Shaw[1] tells us that forty-five cases of eclampsia were admitted in 1914, and at Queen Charlotte's there were nine cases in the same year. Private cases of eclampsia are rare because of the routine examination of urine, and though exceptional cases of sudden albuminuria with almost immediate eclampsia may occur, most cases have a warning albuminuria for some weeks beforehand. At Queen Charlotte's Hospital in 1914 there were 557 cases of albuminuria during labour—i.e., in 30 per cent. of the total labours, and of course many of these cases must have had albuminuria during part of their pregnancies (Queen Charlotte's Hospital Report, p. 13).

If the medical supervision of pregnant women is already sufficient, and its extension unnecessary, why should 3 per cent. of stillbirths now be notified under the compulsory extension of the Notification of Births Act? Why especially should there be so many macerated stillbirths?

In the Report of Queen Charlotte's Hospital for 1914, p. 39, it is stated that there were 100 stillbirths (5·5 per cent. of total births) of which twenty-six were macerated. By some curious error maceration appears as the *cause* of death in these twenty-six cases, and no mention is made of syphilis as a cause of death in the whole number of stillbirths whether macerated or not. Five stillbirths are stated to have been

[1] *Brit. Med. Journ.*, October 14, 1916.

due to albuminuria and three to eclampsia. It is, I think, agreed that the large majority of macerated fœtuses are due either to syphilis or maternal toxæmia.

Sir Francis Champneys, who is of course in favour of medical supervision, in a letter on "Health Visitors and Birth Inquiry Cards" in the *British Medical Journal* for September 23, 1916, p. 438, says: "It is a mistake to suppose that ante-natal care has only been discovered in the twentieth century. All careful doctors have practised it for a very long time and all careful midwives have done the same." Dr. J. W. Ballantyne's work on ante-natal pathology, and ante-natal hygiene really was a "discovery" from the fœtal point of view, although Sir Francis Champneys does not think so, and it happened to be made in the twentieth century, for Dr. Ballantyne began his prematernity work in the Edinburgh Royal Infirmary in November, 1901. In England there were no Infant Welfare or Ante-natal Clinics before 1906. Apart from hospital routine no systematic ante-natal work was started till 1911 when it was commenced in Birmingham and Leeds, and that was mainly the home visiting of expectant mothers.

There are of course many careful doctors, but even careful doctors do not always extend their care, unasked, to pregnant women or to unborn babes. Doctors must not only be experienced in recognizing the complications and diseases of pregnancy but in anticipating and treating the accidents of childbirth as well.

Midwives, although not paid for visiting their patients during pregnancy, are encouraged by the new (1916) Central Midwives Board Rules, to extend their ante-natal care to the "unborn child," for in the subjects for examination for their certificate there is now included the "Hygiene of pregnancy and its diseases and complications, including abortion, both in relation (*a*) to the mother and (*b*) the unborn child." Previously it was worded: "Pregnancy and its principal complications including abortion." This extension of interest to the unborn child will greatly increase the usefulness of the midwife, and appears to imply that she is to be trained in diseases of pregnancy, and encouraged to visit and supervise the patient and the child, during pregnancy, as well as during labour and the puerperium, though curiously enough on p. 22 of the "Rules," where the midwife's "Duties to patient" are defined, her "Duties" even now appear to begin only during the patient's labour. It is a possible danger that if midwives are to be trained in and examined on the diseases and complications of pregnancy, it may be difficult to prevent them treating such conditions, at all events

for a time, instead of at once sending for medical assistance as the rules require.

Some evidence which I was able to give in an address on "Ante-natal Hygiene" early in 1914[1] pointed out that the death-rate of infants during their mothers' pregnancies was as great, or probably greater, than the death-rate amongst their survivors in the first year of life. This evidence was based on the notification of three stillbirths per 100 births, and on the estimate made by many experts that abortions and miscarriages are four times as numerous as stillbirths. If so, it means about 150,000 deaths *in utero* or during childbirth in England and Wales every year. How many of these would be saved if we could always detect syphilis and toxæmia in early pregnancy.

The fact is that the life of the infant and of the unborn babe, like silver in the time of Solomon, was of small account with a high birth-rate and a prolonged peace. Now with a small birth-rate and when mothers are mourning the loss of "only sons," and have an added and often expressed sorrow that they might have had others, the life of the child is of enormous importance, and steps to ensure maternal and infant welfare are being initiated at conception instead of being delayed till childbirth, to the great advantage of both the mother and the child. As a result of this increased interest in the unborn child a large number of ante-natal clinics and maternity centres have been instituted all over the country with the primary object of trying to save the child, but, of course, also indirectly benefiting the mother. Where the clinic has been initiated by "The Association for Preventing Infantile Mortality," presided over by Sir Thomas Barlow, or that for encouraging "Infant Welfare," or as part of the organization of Hospitals for Children, &c., it is natural that the nomenclature should be from the child's point of view rather than that of the mother, and the phrase preferred at such centres is ante-natal rather than ante-partum. There are now 750 such centres in Great Britain and Ireland.

Till a few years ago women who came to be registered for their confinement in the indoor or extern departments of many general hospitals had their names and addresses taken down by the obstetric house physician, who may have had no previous experience of the diseases of pregnancy, and often had very little spare time. It was optional to him to examine or omit to examine the patient, or to test her urine. No students used to attend this registration, for there was

[1] *Brit. Med. Journ.*, 1914, i, p. 355.

rarely any clinical knowledge to be gained. Now opportunity is taken to utilize the registration of expectant mothers by making it, and sometimes calling it, an Ante-partum or Ante-natal Clinic, greatly to the advantage of patients and students, and the department is usually in charge of the Obstetric Registrar or Tutor, or even of the Assistant Obstetric Physician or Surgeon.

Notwithstanding the conclusions which Dr. Donald drew in his paper, and which I fear may tend to check enthusiasm in ante-partum and ante-natal hygiene and care, I feel sure that he really does not disagree with me, for a most interesting account is given in the *British Medical Journal* for October 14, 1916, by Dr. William Fletcher Shaw, on "Ante-partum Clinics," as conducted at St. Mary's Hospital, Manchester, where Dr. Donald has done such admirable and well-recognized work. Every pregnant woman going there to be registered for indoor or outdoor parturition is seen by the resident obstetrical officer, who has always had previous obstetrical experience. Every primigravida and every woman with a history of a previous difficult confinement is examined by him. This is ideal supervision, and so also is the routine examination of urine as insisted upon by Dr. Donald. But Dr. Shaw adds: "It is not sufficient to have well-equipped and well-staffed maternity hospitals; the general practitioner must have practical training in the whole department of midwifery." This is the right attitude to adopt as regards supervision.

The doctor must be able efficiently to supervise his pregnant patients. Routine urine tests should always be made, and further tests made, if there is albuminuria, to decide upon its toxic origin. In addition to the necessity of ascertaining whether the pelvis is contracted, whether there are clinical evidences of toxæmia, or whether the woman is syphilitic, I will merely allude to such maternal conditions as malnutrition, Bright's disease, diabetes, heart disease, and tuberculosis, any of which should, if present, be discovered and treated. Early this year I saw a lady who was only six weeks pregnant, and thought herself well, but was already showing evidences of cardiac insufficiency. Sir James Mackenzie's advice, and he is not a pessimist, was "immediate operation for the removal of the ovum." Such cases, rare though they be, cannot be seen too early. Plumbism, perhaps due to diachylon, asthma, Graves's disease, malaria, chronic colitis, or chronic bacilluria, may also be discovered, and the mother's health safeguarded. The acute specific fevers, as well as acute pneumonia or bronchitis, or gonorrhœa acquired during pregnancy, or acute coli pyelo-nephritis, need not be here con-

sidered, for such conditions would necessarily lead to the patient coming under medical care. Mechanical abnormalities, such as retroversion of the gravid uterus, could be detected before symptoms have appeared, and sequelæ, such as impaction of the fundus uteri, abortion, retention of urine, cystitis, or sepsis, prevented. In addition to pelvic contractions and deformities, potentially obstructing fibromyomata or ovarian tumours, or cicatricial stenosis of the cervix or vagina, may each be unsuspected by the patient, but easily discoverable during early pregnancy, and may be either treated at once or left to be dealt with at a later date as may be indicated. Ante-partum hæmorrhage need not be considered, for the woman would in most cases seek medical advice, so that ectopic gestation, mucous polypi or cancer of the cervix, placental detachments, &c., would become known. Similarly, hæmorrhage due to hydatidiform degeneration of the chorion or to the presence of a carneous or blood mole would lead to a doctor being consulted.

In the later months fœtal malpositions, malformations, such as hydrocephalus, or relatively large children, all of which may be causes of "accidents of childbirth," may sometimes be discovered. The recognition, for instance, of a transverse lie early in labour or during late pregnancy, followed by appropriate treatment, would minimize risk to mother and child.

It is true that quite a large proportion of pregnant women may be seen and found healthy, and their urine may also be found to be normal, so that many of our investigations may have negative results. Even in these cases, however, a doctor rarely sees a primigravida without being able to give her some useful advice as regards general hygiene, exercise, rest, suckling, care of nipples, &c. Women also can be encouraged to attend lectures or demonstrations on the feeding, clothing, and bathing of infants, and can be usefully helped by a word of sympathy and by being assured that all is well and that there is nothing to dread. If she is poor, and is looking forward with anxiety to the financial outlook of the period of her confinement and lying-in, some practical advice, or the possibility of some assistance from charitable or municipal sources could be brought to her notice, or be definitely obtained for her.

The next question is as to how such supervision can be universally ensured. In private practice this is easy to arrange. In panel practice it can also be arranged if the panel doctor is going to attend the patient. Registration at a general hospital, infirmary, or lying-in institution will secure supervision. Midwives can encourage their patients to attend schools for mothers or ante-natal clinics in their neighbourhood, for

a midwife would soon realize that she will be saved much anxiety, effort, and time, if her patient can be kept in such health that the parturition will be a normal one. The force of example will, in time, make supervision general.

Prematernity Beds.

Every maternity centre and ante-natal clinic should be linked up with hospitals where so-called "prematernity beds" or "wards" are available. Observation by experts, and medical or surgical treatment can then be carried out.

Notification of Pregnancy.

If all midwives could be encouraged to take their patients to a maternity or ante-natal clinic the patients would, by such a visit, voluntarily and automatically notify their pregnancy to the doctor in charge, without any publicity whatever. This is a very different thing to *compulsory notification* of the pregnancy to the local health authorities, which could not be carried out at present owing to the ignorance of the value of obstetric hygiene among women, and because of the certain resistance of the women most concerned. Any attempt to enforce compulsory notification to the health officer or his representative would, in many cases, result in the woman putting off notification till very late, or perhaps not till her confinement, and would arrest the good progress now being made in securing medical supervision.

Research.

Research work with regard to ante-natal pathology must be associated with medical supervision of pregnant women, especially as regards syphilis and toxic albuminuria, and the effect of these complications upon the mother and the fœtus. For these and many other reasons a pathological and chemical laboratory should be provided within easy access of all groups of maternity centres and clinics in large towns. This could either be in a general or lying-in hospital, or be one of the laboratories recommended in the report of the Venereal Commission. Every fœtus, and especially every macerated fœtus, whether born before or after viability, and every ovum, however early, expelled from a woman who has had other abortions or stillbirths, should be sent to a pathological expert for post-mortem examination, and search should be made for the *Spirochæta pallida* or other cause of death. In the new (1916)

rules of the Central Midwives Board midwives are instructed to keep the body of every stillborn child till the doctor has seen it.

There seems to be a tendency for some pathologists to consider that syphilis in the parents only causes fœtal disease or death in the later months. This appears to be founded upon the fact that the *Spirochæta pallida* is not found in the early fœtuses, and yet syphilitic women often have alternating abortions and stillbirths. Dr. Mott showed this conclusively in his interesting epidiascope demonstration at the discussion at this Section in April, 1914, following an address by myself on "The need for Research in Ante-natal Pathology," and those who doubt that syphilis causes abortions should read his remarks in our *Proceedings* for that month. Dr. Mott gives a table showing[1] that the wives of fifty-six tabetic men had 154 living children, seventy-three who died after being born alive, twenty-two stillbirths, and thirty-six abortions; and that of twenty-two tabetic women, four were sterile, and the remaining eighteen had sixteen living children, twelve born alive but died later, fourteen stillbirths, and thirty-two abortions. The following are two typical cases: Father contracted syphilis during wife's second pregnancy after two healthy children. She then had four abortions, an infant who lived three months, another who lived six months, then a child who became a general paralytic at the age of 14, afterwards a daughter who was then aged 12, then another abortion, and then a frail girl then aged 8. A tabetic woman (father not syphilitic) had six pregnancies, one child born dead, followed by five abortions.

If, in these cases, stillbirths are proved to be syphilitic, surely it is logical to assume that the abortions in the same women are due to the same cause, even though search for the spirochæta prove negative. Those who hold these new views admit that toxic albuminuria in pregnancy may cause fœtal death during both early and later pregnancy, and that in the later months the fœtus may become macerated. The evidence of a positive Wassermann reaction in the case of the mother, known to have syphilis, does not seem to me to be different from the evidence in the case of a woman who is known to be suffering from a toxic albuminuria. In both cases the mother has a disease which may destroy fœtal life at all stages. I cannot help thinking that the existence of spores in the life history of the *Spirochæta pallida*, as described by McDonagh, may be a possible explanation of the failure to find spirochætæ in abortions, the spirochætæ being destroyed by the

[1] *Proc. Roy. Soc. Med.* (Sect. Obst. and Gyr.), 1914, p. 288.

ferments of the chorionic villi, whilst the spores with their stronger envelope escape.

Whether, therefore, the question of medical supervision during pregnancy be considered from the point of view of the welfare of the mother and unborn child, or as an educational stimulus to the nation, or from the standpoint of the increase of pathological, clinical, and therapeutical knowledge for the profession, there can surely be no real difference of opinion that every pregnant woman should be seen by a doctor, and then have such supervision as her condition requires.

The Importance of getting Medical Practitioners and Midwives to co-operate with the Local Health Authorities.

By Comyns Berkeley, M.D.

If a reference is made to the title of the subject being discussed this evening, and to the text of the three subsidiary subjects dealing with the different sections of the subject, it is evident that the members of your Council, in framing them, determined that, whatever else might happen, this should be an "evening out" for the pregnant woman, and that, so far as they could help it, her claim for better care and treatment should not escape adequate presentation and discussion for want of repetition.

I am led to make this observation from my personal opinion that, had Dr. Moore so chosen, he could have covered legitimately in his introductory remarks all the ground that is covered by the three speakers that follow him, who in their turn can hardly help enlarging on his subject. Further, I have failed to find any encouragement in the remark of several of my friends that they should be sorry in such circumstances to have to follow the medical officer of health for Huddersfield. I suppose, therefore, I must consider myself fortunate in not being placed fourth on the list, although I quite expect to find that when Lady Barrett has introduced her subject, she will be in no need of my commiseration, but need rather that I should have congratulated the Council of this Section on having the foresight to place her in this position.

The title of the subject chosen for Dr. S. G. Moore for this evening's discussion, "The Need for Improvement in the Care of Pregnant Women," suggests to my mind, among other things, that it is thought

that doctors and midwives do not pay as much attention to the health of their pregnant patients as they might or can do under present circumstances. Again, the subject chosen for Dr. Amand Routh, "The Importance of getting all Pregnant Women under Supervision, and affording them Necessary Treatment," suggests that at present pregnant women are suffering at the hands of their medical attendants or midwives with respect to supervision and treatment. In either case, therefore, not having had the advantage of knowing what points these two speakers were going to emphasize, I do not deem it unnatural that I should assume, in thinking over what I was going to say, that they would be certain to touch on many points which I could have elaborated, and that, unless I was very careful, I should only be repeating many things which they had already said, and said better.

I have come to the conclusion, rightly or wrongly, after a careful consideration of the title of the subject that has been allotted to me—namely, "The Importance of getting Medical Practitioners and Midwives to co-operate with the Local Health Authorities," that I am supposed to deal with the subject of "The Maternity Centre" and its relation to the private medical attendant and the midwife. If so, I shall confine my attention principally to the ante-natal side of it.

The question of the "Maternity Centre" first came prominently before the medical profession on the passing of the "Notification of Births (Extension) Act, 1915," when the Local Government Board addressed a circular to the County Councils and Sanitary Authorities pointing out the provisions of this Act. This circular, among other things, drew the attention of these bodies to Section 2 of the Act, which enables them to make arrangements for the care of pregnant women, and the Board states that it is confident that it can rely upon the Local Authorities to make the fullest use of the powers conferred upon them in this respect, and, indeed, assumes that they will do so.

In order to encourage those Local Authorities already engaged in this work, and to stimulate others who have not yet commenced, the Government has agreed to finance any such scheme up to one half of its cost by means of annual grants. In offering to make such grants, the Board is careful to state that it will expect medical advice, and, where necessary treatment, to be continuously and systematically available to the pregnant mother, and that to enable this to be done efficiently, the Local Authorities will have to start maternity centres in populous districts.

Now, I am confident that every doctor and midwife will agree that

a pregnant woman is justified in maintaining that she has a right to the greatest possible care and to the best possible treatment that medical science can give her during this important and trying time of her life, and this not only for her own sake, but also for the sake of the nation, since the child which she brings forth will, if it lives, become a citizen of that nation, and its usefulness to the State may be greatly impaired or nullified from want of care of its mother during its intra-uterine life.

The British Medical Association has passed a resolution that it is entirely in favour of any scheme which will encourage prospective mothers to arrange in advance for their nursing and medical attendance during the period of pregnancy, but evidently it is doubtful as to whether the scheme suggested by the Local Government Board is the best means of securing it. Let us see what this scheme is.

Having decided that maternity centres must be set up, it suggests that their personnel and procedure shall be as follows:—

Personnel.—The staff of the Centre is to consist of medical men or women and non-medical women, working under the direction of the local medical officer of health. With regard to the medical workers, the Local Government Board, as far as I can gather, considers that the best possible medical officer to have charge of such a centre would be a whole-time paid official.

This impression has led to the conclusion "that there would be a tendency under such a scheme to the formation of a class of practitioner who would be wholly concerned with the treatment of disease in expectant mothers and in young children, and that almost the whole of actual practice among such patients may be withdrawn from the general practitioner." Further, "if the medical and surgical treatment of the public is to continue to be (as it should be) in the hands of general medical practitioners, aided by suitable opportunities for consultative and pathological assistance, the effect of this must be seriously detrimental to public and profession alike."

If a local authority takes the line of trying to persuade all pregnant women that they need the help of some specialist (and the specialist employed by a local authority is in very many cases certain to be some young woman doctor at a salary of about £300 a year), I do not think that it is at all likely that the doctors will co-operate, but on the contrary there will lurk the danger of a tendency to make things as difficult as possible. Quite apart from the financial point of view, which must appeal to every man, even to a general medical practitioner, his

exclusion from such a scheme would most certainly lessen his usefulness to the community; and it has been pointed out that if the duty is to be undertaken by a whole-time officer, such a one will lack not only the healthy experience of free practice with the stimulating competition of ideas, but also, which is more important, will be unable to obtain that free entrance to the homes of the workers which is possible to the general practitioner.

The Board is, however, very anxious that medical practitioners and midwives should co-operate in its scheme, and, quite rightly, if for no other reason than because such lack of co-operation would mean its ruin. The whole success of this scheme depends upon it pleasing the doctors and midwives. With this object in view, therefore, the Board suggests as alternatives that the head of the Maternity Centre should be a practitioner from another district—or, and this is the arrangement which it considers least satisfactory—a rota of practitioners from the neighbourhood.

The Local Government Board agrees that the work of the Maternity Centre would be most effective if co-operation is secured with midwives of the district, and thinks that the value of such a centre would be most enhanced by the co-operation of the medical practitioners, and that they should be consulted in regard to organization, and that any arrangements made should, as far as practicable, secure their approval.

In addition to the doctor or doctors in charge of the maternity centre, there are to be non-medical health visitors, and the Local Government Board lays great stress upon the importance of these workers, if the scheme is to be a success. It appears that the best possible women, according to the Local Government Board, for such posts, are trained nurses who have gained the certificate of the Central Midwives Board, and have been certified as sanitary inspectors. It is suggested that in some cases, at any rate, these health visitors should act as inspectors of midwives, and thus bring into line the hygienic advice given to the mother during or after the lying-in period. In addition, it is hoped that voluntary workers will be available.

The Work of the Centre.—The doctor will make a careful examination of the heart, lungs and kidneys of the expectant mother, and, when considered necessary, will measure her pelvis. If serious organic disease is detected, the patient will be advised to have medical aid at home, or, failing this, arrangements will be made to send her into a hospital for in-patient treatment, and in all cases, if the patient has engaged a midwife, the midwife will be informed of the state of affairs.

Any immediate treatment will be given at the centre, or by the private doctor of the patient, or by the hospital, and no responsibility will be undertaken at the centre for continuous treatment.

As the work of the centre will only succeed in producing the best results when combined with home visiting, the health visitors, or in some cases special visitors appointed for this work, will visit the homes of the patients, as often as they consider necessary, to see that the advice given has been thoroughly understood, and is being carried out in the home. It will, therefore, be necessary for the health visitor to have been present at the medical consultation at the centre.

Now, such a scheme as this, if carried out as suggested, will not, as far as I can gather, please either the doctors or the midwives. It is futile to suggest, as is suggested by advocates of the Government scheme, that the pleasing of doctors and midwives has nothing to do with the matter, and that all we are concerned with is the lowering of the mortality or morbidity of the pregnant woman. The Local Government Board is at pains to impress upon the medical profession that it is anxious to satisfy its legitimate demands. Again, what right, I should like to know, have those people who insist that the Board's scheme is the only scheme that should be considered, to assume that they are the only people who know what is good for the community or who have its interests at heart?

The scheme of the Local Government Board, unless it is properly safeguarded, will in the long run vitally alter the status of the profession, and the relation between doctor and patient, to the detriment of both. Such a scheme would have a tendency to eliminate the "family doctor," who is the best person to give advice to his patients and to treat them. Again, the tendency of the Insurance Acts to provide every worker with medical attendance on the lines of private practice may be interfered with.

Now, as regards the midwives, about 50 per cent. of the total number of pregnant women and 75 per cent. of working women engage midwives to attend them, and perhaps this scheme will affect the midwives more than the doctors. The chief cause of the opposition of the midwives to this scheme is due to the proposal to take their supervision out of the hands of the Councils and County Boroughs and transfer them to the Borough Councils. The midwives seem very certain that it would be a move in a backward direction to take away the supervision of themselves from those authorities. Such a transference of supervision has been tried, but was so unsatisfactory that the Counties

who had delayed their powers had to revoke their delegation. A striking instance of this was Kent, where the administration of the Midwives Act was delegated to sixty-five different District Councils. Little or nothing was done to carry out the Act, and the difficulty in obtaining any trustworthy returns from the various authorities was overwhelming. Moreover, a Departmental Committee which met to consider the working of the Midwives Act, passed a resolution that "it is the emphatic opinion of the Committee that this power (of delegation) should be withdrawn, and that in cases where it is still exercised it should be revoked."

It is a great advantage for a midwife to be responsible to one inspector only, whereas under the proposed scheme, a midwife working in London, for instance, might be responsible to the inspectors of as many as five or six Borough Councils.

It is contended that the health visitor of a District Council could not devote the whole of her time to inspecting midwives, and the duties and payments of such a post would not be likely to attract to the service such women as are now commanded—women who combine high technical qualifications with wide general experience of midwifery, and who at present inspire confidence and respect among every class of midwife.

Midwives are to be encouraged to accompany their clients to a centre or to send a note to the doctor concerning them. But who is to pay the midwives for this extra work which they would be called upon to do? There is no mention in the scheme of this aspect of the case.

The work at the Maternity Centre will be, primarily, one of examination, but supposing a pregnant woman is examined by her doctor as she should be, will she not be very likely to resent a further detailed and somewhat prolonged examination? Now, let us examine a scheme that is proposed in place of that advocated by the Local Government Board.

The Representative Meeting of the British Medical Association in May last passed the two following resolutions:—

(A) That in the public interest the employment of private practitioners would be the best system for adoption.

(B) That such a system would be beneficial and attractive to the medical profession itself, and therefore its adoption would not only be the best in the public interest, but would also be the line of least resistance.

And the scheme it proposed is as follows:—

(1) General supervision, including responsibility for records and statistics and the following up of cases to ensure that adequate treatment is obtained, to be undertaken by a supervising authority, preferably the medical officer of health assisted by nurses or health visitors.

(2) Attendance at a centre or at such places as may be arranged for the purpose of giving advice and deciding if treatment is necessary, and the treatment at this centre of such selected classes of cases as may be determined upon by the local authority. This work to be undertaken by all those local practitioners who are prepared to do it. Such practitioners would be required to attend at the centre or arranged place for a specified time on certain days at fixed intervals.

(3) Treatment of all cases requiring ordinary medical attendance, to be undertaken either at the centre, at the doctor's surgery, at the patient's house or at an institution.

(4) All persons referred to the centre to be allowed to choose whichever doctor on the list they prefer to consult.

The work of this rota of doctors would consist in the examination and advice to expectant mothers, and the keeping of such records and the giving of such treatment as is included within the scope of the scheme. The midwives also suggest that the practising midwife should be included in either scheme by enlisting her sympathies and securing her services. It is pointed out by those who speak on their behalf that the practising midwives would get in touch with the pregnant woman earlier than any official, especially as there is certain to be great resistance among the people to notify pregnancy to any public authority. The midwife is often the only confidant of the single pregnant woman, and such knowledge is of very great importance in regard to the question of abortion.

Midwives are now trained to recognize conditions requiring medical advice, and by the rules of the Central Midwives Board are bound, when they discover that the patient is in need of such advice, to refer her to a doctor.

The midwives could quite well work in with the scheme proposed by the British Medical Association. It is more than probable that if suitable arrangements could be made with the doctors we should get a more educated class of women practising as midwives. At present, a large number of the women who have passed the examination of the Central Midwives Board, and who would be most suitable for midwives, do not practise as such. Now, it is impossible to do without midwives, the statistics I have already quoted prove this, and yet the majority of

practising midwives are not of the class one would wish. Neither the Local Government Board nor anyone else will ever get proper ante-natal care of the poorer pregnant women until the midwife is made to feel her full responsibility. Educate the midwife well, make her position a decent one, and with her help, under the guidance and direction of the general medical practitioners, such a scheme as that outlined by the British Medical Association could be made the greatest success.

It is quite certain that the Local Government Board has in mind the desirability also of the local authorities starting a "Notification of Pregnancy Scheme," since it is evident that the elaborate machinery set up for a Maternity Centre would lose a vast amount of its utility unless such a scheme was started, and in fact the memorandum states that one of the chief objects of the Centre is to secure the medical supervision of the expectant mother, which of course could not be done unless the mother notified that she was expecting, or someone did it for her.

Both the general medical practitioners and midwives have no doubt that it would be all for the good of the expectant mother and the nation if ante-natal care and treatment were more seriously considered than up to the present has been possible, but to do so by a notification of pregnancy would only appeal to these bodies if this was really voluntary. The main objection is that some of the local authorities who have adopted a so-called voluntary system appear to have tried their best to make it look as much like a compulsory system as possible.

There are many practitioners in favour of the voluntary notification of pregnancy and there are some against it, but I think I may say that at the present time the vast majority of practitioners in the United Kingdom are dead against "compulsory notification." Now, although the Local Government Board is at pains to indicate that it does not approve of compulsion, and distinctly states that such notifications should not be made unless the express consent of the expected mother has been previously obtained, a large number of doctors, with the recollection of what has happened before under somewhat similar circumstances, can only see the thin edge of the wedge in this insistence of the advantages of voluntary notification. Their fears in this respect are not, therefore, allayed by the action of the authorities of the City of Nottingham, which is issuing to the midwives in its districts formidable-looking documents surmounted by the Arms of the City, asking the midwives to notify upon these documents the names of their pregnant patients, and also the answers to a number of questions of a very personal and private nature concerning them. Which documents, be it noted, have apparently been

regarded by some midwives as a demand and which can be returned through the post, unsealed, for the cost of a halfpenny. Again, the Huddersfield authorities, by offering half-a-crown to the doctor or midwife for each patient that is notified, do not intend to let the grass grow under their feet.

Expectant mothers used to make the necessary arrangements for their confinement much earlier in pregnancy, but now, since the advent of the Maternity Benefit, because they know this money is coming in, the present tendency is for them to put off engaging their attendant till on the average about two months before the confinement.

It is contended that the notification of pregnancy, if compulsory, will postpone the necessary arrangement being made for the confinement to an even later date, when the great object of this scheme is to induce them to notify as early in pregnancy as possible.

Then the patient herself is concerned in this notification of pregnancy in more ways than one, and personally will have a good deal to say to it. Many women will consider the visits of the health visitor, resulting on notification, as an intrusion, and will, on that account alone, delay engaging a doctor or midwife till the last moment. Until notification becomes compulsory, single women will certainly postpone engaging an attendant till the last possible moment, and in many cases these are the very women one would wish to get hold of. And besides that, working women nowadays are not going to stand class legislation, and they will see to it that the same regulations apply to the pregnant duchess as to one of themselves.

The Local Government Board has professed a platonic respect for the proposal that the general practitioner, wherever possible, should be employed in the ante-natal schemes, but past experience inclines the practitioner to believe that the Board will practically give its weight to the whole-time scheme. Town Councils like to have the full control which they can exercise over a whole-timer, whilst by such schemes the status of their medical officers of health is enhanced, with the result that the employment of general practitioners never gets a fair chance.

Members of the civil community, when they or the members of their family fall ill, have to employ the general practitioner. If the Local Authorities gradually take out of his hands all the work which really belongs to him, he will become a mere guide to the nearest specialist or institution, and his status and capacity will be gradually undermined.

In the London school clinics such a method as that proposed by the British Medical Association has been a great success, but no local

authority has yet given this method a trial with respect to ante-natal clinics. The Local Government Board's help probably means that it will stand by and will not actively interfere.

The Board thinks that a rota of doctors is the least satisfactory method of dealing with the subject, but the British Medical Association's proposal does not imply a rota in the strict sense of the word. The word "rota" has a sinister meaning, giving the impression that patients seen at one visit by one doctor may very well be seen at the next by another. The British Medical Association's plan means nothing of the sort. It aims at these men serving for stated periods, which shall be long enough to enable them to become expert at the work. They must attend regularly once or twice a week at a stated place, and the patient on her return visit will be seen by the same doctor. Such a scheme would have the great advantage that, within a certain time all the younger men in a locality would have passed through the maternity clinics, to the great benefit of the community and the greatest benefit to themselves.

It is not true to say that general medical practitioners are hostile to the proposed scheme of the Local Government Board: refer to the resolution of the Council of the British Medical Association, and later endorsed by the representative meeting, which I have previously quoted. If the officials will consult the local profession, as surely they should do if they are anxious to make it a success, they will find them entirely sympathetic. The idea that the profession is hostile comes from a body of whole-time officials, whose ideal is a discipline of the race and its proper subordination to a class of super-officials. The profession, however, is hostile to any attempt to apportion work to whole-time officials which it believes can be done as well or even better by the general practitioner.

One cannot have a specialist at every woman's door, but Heaven help her! if the doctor who *must* attend her in her emergency or for her so-called trivial complaints, has had all the backbone and interest in his work taken out of him by having the really interesting medical work handed over to a hierarchy of official specialists.

Against such a scheme as is outlined by the British Medical Association it is suggested that the practitioner presiding at the Centre may be seeing and advising the patient of another doctor; but if the woman's doctor is on the staff of the Centre she would presumably go to him, whilst if every man had his chance of being on the staff the risk would be the same for all.

Then as to the guarantee of the competency of the members of the medical staff. There are his qualifications, and it must not be forgotten that many so-called specialists in tuberculosis, for instance, are youths and maidens, with a high qualification if you like, but with an experience that, in many cases, extends to only a few months in the subject in which they are posing as specialists.

Is not the qualified doctor with some years' experience of midwifery and doctoring infants *prima facie* as likely to be as competent as the kind of young woman who would probably be appointed to most of the new posts. If more practitioners apply than are required, the Local Government Board must make a choice, just as it does in the London school clinics.

The highly specialized members of the profession are not doing their duty to their profession, or to the community, unless they interest as large a number of general practitioners as possible in such a scheme as that suggested by the British Medical Association.

General practitioners, like medical officers of health and consultants, are good, bad, and indifferent; but it behoves those of us who have the interests of the nation at heart, to try to improve their position and insist on their taking a part in these communal schemes, not only because they are members of our profession, but because the community cannot dispense with that experience or that knowledge of human nature which they acquire by virtue of their daily work.

The Importance of linking up all Organizations for Maternity and Child Welfare in Local Health Districts.

By Lady Barrett, M.D.

In all organization to secure a normal motherhood and infancy we have to remember that the central factor is the mother, and mothers are self-respecting human beings, thoroughly British in resenting interference as to the best way of managing their own affairs, and particularly sensitive in regard to the subjects with which we are concerned, their homes and their children. I suggest, therefore, that the linking up of organizations for maternal and child welfare must be planned with the mother, her wishes, her prejudices, and her disabilities in the focus of our attention, for, without her co-operation, our schemes, however

excellent, are foredoomed to failure. It is true that the mother does not yet know what she needs. So accustomed is she to a maximum of suffering and a minimum of comfort that she does not dream it possible that even the luxury of having time to be ill could ever come her way.

Let us therefore consider for her what her need is. I propose to discuss very briefly: (*a*) The need of the mother—medical, educational, and social; and (*b*) existing organizations, in order to clear the way for some suggestions in regard to (*c*) co-ordination.

(*a*) (1) The *medical need* is threefold. It must include medical supervision during pregnancy, skilled attention at labour, and medical supervision during lactation. We ought to bring within the reach of every mother in the land skilled obstetric assistance at labour, that is, such skill and knowledge as can only be acquired by special study. It is obviously impossible that every mother can be attended by an obstetric specialist, but an obstetric specialist should be available when necessary for every woman in time for the best results to be obtained. To secure this in town or country, arrangements must be made whereby every midwife has a doctor provided for each case should it prove to be beyond her powers, and every doctor has a specialist to call in for cases of difficulty. The possible weak links in this chain would be if the midwife did not recognize early signs of abnormal labour, or if the general practitioner failed to realize that operative procedure beyond his experience or resource were necessary in the best interests of mother and child. Modern teaching of midwives and medical students should, however, minimize or eliminate these risks. But the medical responsibility with regard to maternal and infant welfare does not begin or end with suitable conduct of labour. Medical supervision throughout pregnancy, and I suggest throughout lactation, are necessary if the ideal of prevention of ill-health is to be secured rather than the cure of ailments when they arise. It is better to provide a fence at the top of a cliff than an ambulance at the bottom. I hope the term "ante-natal clinic" now growing up through the demand for supervision during pregnancy will be replaced by some such term as "maternity clinic," which shall include in its work post-natal as well as ante-natal care. This, of course, involves a considerable amount of medical work which has not hitherto been given, and I fear we must recognize that supervision by visits to the houses of the women would be quite impossible with the present supply of doctors even after the War. The suggestion of maternity centres, where pregnant and nursing women would attend to see the

doctor periodically, materially lessens the work while securing continual touch between doctor and mother.

(2) *Educational Need.* — Most mothers need education in all the activities which pertain to the making of a home and the bringing up of children, for, while it is quite true that all the education in the world will not teach a woman to mother, yet a mother's love for her children without knowledge has in the past led to many harmful practices, and I suppose we all recognize that better education does not destroy any useful quality of mind or heart. Education in things pertaining to health should be, if possible, first suggested to the mother by a doctor, but must be taught over and over again in the home by someone who is regarded as a friend, yet is qualified for such work, if good results are to be obtained. Education in many household affairs is best taught at first outside the home in maternity centres where practical and not theoretical methods are employed, and where the stimulus of competition in learning and other elements of club life make education a pleasure.

(3) *Social.* — But medical skill and educational advantages alone would still leave much to be desired. Suitable houses in which to rear children, as well as suitable environment for parturition, are necessary if mortality and morbidity of mother and child are to reach the lowest possible level. I suppose that we all agree that the homes in which much of our hospital out-patient maternity work is carried on are wholly unsuitable. The technique of midwifery is the technique of surgery, and no surgeon would dream of doing surgical work under conditions passed as good enough for midwifery. We need hospital conditions for all cases in which surgical interference is required, and for all patients living in insanitary dwellings or who cannot provide one well-ventilated room for the mother's use alone during the lying-in. This is surely the minimum demand. In the case of mothers who are confined in their own homes two further needs arise, closely associated with efficient medical aid—viz., nursing for the mother, and cooking and housework for herself and family. Work which has employed the mother's whole time cannot suddenly cease, and we find that mothers wish to be confined in their own homes rather than in the comfort of hospital in order that they may do the work of the house as best they can during the lying-in. Some provision for her housework to be carried on is as necessary as nursing for herself. But the homes are not only unsuitable for midwifery; many involve work for pregnant women more injurious than paid outdoor work which calls forth such condemnation. I refer particularly to the inefficient arrangements for water, coal, food, &c.,

involving much carrying of heavy weights upstairs and difficulty in obtaining any ordinary standard of cleanliness for house and children. The sanitary arrangements of many dwelling-places of the poor are so bad that it is almost incredible that they have been tolerated in homes which rear the citizens of the richest empire in the world.

(*b*) The existing organizations are: medical, municipal, and voluntary.

Medical.—Provision for midwifery in any urban district includes the work of midwives, medical practitioners, hospitals, and Poor Law infirmaries.

Municipal work for the care of mother and child is carried on by the Medical Officer of Health and Public Health Committee, with the aid of the sanitary inspectors and health visitors. In some districts municipal payment in part is made to midwives and doctors, and municipal maternity and infant welfare centres are provided, including in some cases dining-rooms for expectant and nursing mothers.

Voluntary philanthropic agencies are numerous. It is only just to remember that the interest aroused in the whole subject is largely due to the work and influence of these agencies. The more important are schools for mothers and infant welfare centres with various activities, including infant consultations, health visiting, classes, crèches, dinners for nursing and expectant mothers, and more recently—ante-natal clinics and dental clinics. Other useful societies provide day nurseries, invalid cookery and home helps. Such are the various organizations more or less in touch with each other which are at present attempting to supply the needs of maternity and infant welfare. Clearly a closer co-ordination as well as extension of these agencies is necessary.

(*c*) *Co-ordination.*—The memorandum of the Local Government Board has given a centre around which these various agencies may crystallize. We are introduced by it to the idea of a "maternity centre" and its possible activities, which may include: Medical supervision of pregnant and nursing mothers, attendance at confinement, pathological investigations, medical supervision of infants, medical supervision of children aged from 1 to 5, health visiting, educational facilities, and dinners for pregnant and nursing women. Such activities involve at least two types of institution—the hospital, and the infant welfare centre—together with visits in the homes by health visitors, and midwifery attendance whether by doctor or midwife.

In venturing to open a discussion on the question of co-ordination, three principles seem to be of paramount importance.

(1) That all sanitation, health visiting, statistical work, &c., must

centre in the Medical Officer of Health of the District and Public Health Committee.

(2) That all medical work of the district, medical, surgical and pathological, should centre in the hospital.

(3) That all voluntary work, which is necessarily supplementary, should be organized in relation to the activities of these two main centres and should supply the social and educational part of the whole.

With regard to the first point the reasons are fairly obvious :—

(*a*) Only an organization controlled by the State and municipality can cover the whole ground; voluntary work, and what I may call voluntary municipal work, is always patchy in character. Only control by a State department can make municipal work approach uniformity of standard, and only municipal control can bring the health of a whole district to the highest level. We hope that such a State department may one day be set up under a Minister of Public Health, and deal exclusively with all that pertains to the health of the nation.

(*b*) Only a national organization can supply adequate funds for this work. .

(*c*) The annoyance of a multiplicity of visits and other overlapping can only be avoided by central control.

The advantages of the second point—namely, that the hospital should be the centre of the maternity work of the district are many :—

(i) In such a scheme as that suggested by the Local Government Board it must necessarily be a hospital which provides beds for observation of diseases of pregnancy, for difficult labour, for normal labours where social conditions render the home unsuitable, and for cases of sepsis or other puerperal complications which may arise in the midwifery practice in the district.

(ii) The hospital should therefore naturally provide for the pathological work, not only of the cases within the walls, but also for all cases in the district which it serves.

(iii) If cases requiring in-patient treatment and pathological investigations are to flow towards the hospital as a centre, its activities should also include: Out-patient consultative maternity clinics, out-patient consultative infant clinics, out-patient consultative children's clinics. Cases would be referred to these clinics for advice by doctors, by midwives, or by the smaller type of maternity centre.

(iv) Research work in obstetrics done at a hospital in such intimate and friendly relations with the whole maternity work of its district will command at once material necessary for such research.

(v) Where a teaching school is attached, the advantage of such concentration of work is obvious, and I venture to suggest that in the future it may be desirable to abolish the teaching of midwifery in the external districts of our hospitals and so disarm any feeling there may be in the minds of doctors and midwives that the hospital competes unfairly with them in their own field.

If such a concession should lead to all complications finding their way to the hospital and all normal cases unable to provide for themselves—the teaching of practical midwifery would have nothing to lose and much to gain.

If the maternity hospital of any district fulfils all these functions, Poor Law infirmary midwifery would cease to exist, and I venture to suggest that this would be entirely beneficial to mother and child, providing as well many additional normal cases for study in the lying-in hospital wards which might otherwise tend to have too large a proportion of cases of difficult labour.

Such a hospital constitutes one type of maternity centre—the consultative type; but two types of centre are necessary to fulfil all requirements. If we are to have the facilities of fully equipped hospitals and pathological departments, with the latest appliances for routine work and research, they cannot suitably include such work as classes and demonstrations for the women, with nurseries for the children while their mothers are taught, or dining rooms for necessitous cases, &c., and it would be unnecessary to multiply such institutions so as to be near the homes of all concerned.

But facilities on behalf of mother and child, to be attractive to the mother, must be within reach of her home and must not involve long hours of waiting.

Small centres are needed in sufficient numbers to serve all working class areas within easy reach of the mothers. Just as there are gradations in obstetric aid so there may be two kinds of obstetric clinics, and all such small centres therefore should include maternal consultations, ante-partum and post-partum, and infant consultations, and children's and dental clinics.

The majority of cases supervised at the smaller clinic would need no further medical care, but more serious cases would be at once referred to the consultative centre serving that district, either directly or through a medical practitioner. Two points seem to be of special importance in regard to maternity clinics in these welfare centres:—

(1) They enable every midwife's patient to have medical supervision during pregnancy, a great advantage to midwives.

(2) They make it possible for every case of pregnancy illness to be referred to the doctor who will be present at labour.

The smaller centre would include under the same roof all other activities unsuitable for a hospital, which are now carried on by schools for mothers and which constitute the centre a sort of maternity club as well as a medical agency. All this work might continue to be done by voluntary help even if the centres become municipal in finance and control. This type of minor maternity centre, with its clinics, bristles with debatable points and difficulties, the most important of which is their relation to the medical officer of health and to the midwives and doctors practising in the district. The agent of the medical officer of health in the homes of the women and at the centre is the health visitor, or sanitary inspector, or both, and if all sanitation, health visiting, &c., is to centre, as it must do, in the medical officer of health, the qualifications and duties of this officer, call her what you will, are of great importance.

One of the many trials of the working-class mother must be the desire of so many people to do her good, and their determination to invade her home for that purpose. Organization of maternity help might safeguard mothers from such indiscriminate visiting and yet secure that every home shall have one visitor with the necessary knowledge to observe sanitary and structural defects and to teach the mother about simple laws of hygiene and infant care, and who has also sufficient tact and sympathy to win the mother's confidence and interest, and attract her to share in the activities of the centre. Such a visitor needs special training, which should include practical and theoretical work in sanitation, tuberculosis, health visiting, infant care and the nursing of children, and also some knowledge of midwifery and of sociology.

A health visitor with these qualifications, working under the medical officers of health, would bring to the notice of the authorities the need for reforms in the housing and sanitary arrangements of her district; she would put out-of-work husbands in touch with the labour exchanges and other agencies, she would report cases of suspected tuberculosis, &c., and women pregnant would be invited to the maternity centre where they would at once be in touch with medical and social care. There would be no need for any other official or visitor to enter the home save the midwife or doctor engaged for the confinement.

At the present time the lack of sufficient municipal health visitors is supplemented by the work of women paid by voluntary Infant Welfare Centres whose qualifications vary. It is most desirable that even if for a while voluntary societies have to supply some of these workers, that a uniform qualification should be established and that the societies should instruct their visitors to report to their medical officer of health and to receive instructions from him. Just as the Health Visitor would send women to the Infant Welfare Centre, so in turn would the centre send cases to the doctors and midwives. To this end it would be necessary to keep a register of all practitioners and midwives willing to do midwifery in the areas concerned, with the fees for which they would work recorded at the centre, and in the case of the doctors the fee also for which they would be willing to attend a case in which the midwife was in difficulty. Every mother could then choose whether she would be attended by midwife or doctor. She would have free choice of both, knowing the fee she would have to pay.

In the case of the women choosing midwives it will be essential for more skilled advice to be at hand should the case prove to be in any way abnormal, and it is suggested that the mother should herself choose in such cases one of the doctors on the list, provided his fee be known. The maternity centre would be furnished with complete information, and the mother would be directed to go to the address of midwife or doctor as the case may be, to be seen as early as possible in pregnancy.

In most poor quarters the patient will be unable to pay the fee of a doctor called in for difficulty, in addition to the already arranged midwife's fee. Yet it is essential that such help should be available because midwives are not only unable but are forbidden to deal with such cases. This difficulty has been met by the Local Government Board if the Local Health Committee is willing to be responsible for half the expense.

It is equally desirable that the doctor should be able to send for hospital aid should the case be beyond his resource. The hospital might thus in three ways become the free Consultative Centre for the district with advantage to both hospital and doctors.

(1) Every practitioner in the district would be invited to send cases of illness in pregnancy or after the lying-in period to hospital for consultation or an opinion, which should be written to him (if unable to meet in consultation), together with any necessary pathological report.

(2) Any practitioner in difficulty during labour might send to the maternity department of the hospital for assistance, or for admission to the beds of the hospital.

(3) Any practitioner might send material for examination, or patients for the Wassermann or other reactions, to the Obstetric Pathology Department, a report of which should be given; the cost of such outside work would naturally be paid by the Public Health Authorities.

With regard to the question whether general practitioners or whole-time officers are to do the work at the small centres it may be pointed out that it would not tend to win the confidence of the women if a succession of different doctors attended the clinic, nor would it tend to efficient work. If the general practitioners practising midwifery in the district would elect one of their number to do this work this grave difficulty would be avoided, but I fancy in most districts it might be thought that this would give the chosen medical officer an unfair advantage over his fellow practitioners. If so the difficulty would probably best be solved by the appointment of a whole time medical officer.

In closing this inadequate survey I venture to submit that some such method of co-ordination of work, starting with the routine visiting of every home by a competent visitor and securing the medical supervision of all pregnant, parturient and puerperal women, would obviate the necessity for anything so repugnant to the feelings of the mother as the compulsory notification of pregnancy, yet would secure the same end.

Professor L. A. Krivsky, M.D. (Petrograd).

A fortunate occurrence enables me to give to you some information as to the manner in which the question under discussion is dealt with in Russia. I have recently received the Russian *Journal of Obstetrics and Diseases of Women*, and I discovered in it a paper by Professor A. A. Redlich, entitled "The War and the Care of Maternity and Child Welfare." This was an address delivered by him on March 6, 1916, at the Congress of the Pan-Russian Institution for the Care of Maternity and Child Welfare, under the august patronage of Her Imperial Majesty Alexandra Empress of Russia. As I think

that the views expressed in this address will be interesting to English physicians, because they will give some idea of the state of this question in Russia, I intend to give a short extract from this paper of Professor Redlich.

The birth-rate in the fifty central provinces of European Russia is decreasing: between the years 1894-96 it was 49·9, but between 1909-11 it decreased to 44·8 per 1,000 of the population.

The death-rate is also decreasing, but very slowly: from 36·3 between 1892-95 to 29·2 between 1908-11.

In the year 1913 in the same provinces of Russia there were 5,249,677 births and 3,338,938 deaths, which makes 43·1 per 1,000 births, 27·4 deaths, and 15·7 natural increase of population. The general death-rate in Russia is very high and due to the enormous mortality among children up to one year; the latter is 27·2 per cent., while the minimal mortality is between 7·1 and 6·7 (Sweden and Norway) for each hundred children born.

According to the figures of the lying-in hospitals, the number of still-born children must be estimated at 5 per cent., and the number of miscarriages at 10 per cent. Therefore, from the 5,249,677 births in the year 1913 in the provinces mentioned there were 870,000 miscarriages and stillborn children.

As one of the most important causes of mortality amongst children up to 1 year appears to be the absence of mother's milk, or, at least, of rational artificial feeding, the public generosity in Russia displayed itself in the founding of two societies—(1) "The Society for the Prevention of Mortality amongst Children," and (2) "The Drop of Milk," organized in a manner similar to the French societies (Goutte du Lait).

A little later the Russian Government assisted the cause by the passing of a Maternity and Sick Fund Bill on June 23, 1912, which enables women to claim a weekly sum of money equalling from the half to the whole of their usual wages during a period of two weeks before confinement and four weeks after it, provided they have been working no less a period than three months. Unfortunately, however, this Bill only applies to the women working in factories employing no less than twenty hands, and worked by mechanical motors. According to the provision of the Act, the employee subscribes a certain percentage into the fund, the employer paying the rest. Up to January 1, 1914, there had been opened in Russia 773 sick funds with 561,803 members.

The much too small amount received by the member before and after confinement points, in my opinion, to the necessity of financial help from the Government, as is the case in England and Norway. At last, on May 31, 1913, there was founded by the edict of the Emperor the Pan-Russian Institution for the Care of Maternity and Child Welfare, under the august patronage of Her Imperial Majesty Alexandra Empress of Russia, and a special body, the aim of which is to unite all the different efforts working in the direction of the prevention of infant mortality and the care of maternity and child welfare.

Besides this, a Special Commission for the Revision of Medical and Sanitary Legislation perfected a programme for the extension of the lying-in hospitals necessary for securing to the population contemporary obstetrical assistance. To accomplish this end the Russian Empire needs 37,000 beds and 60,000 midwives, which would guarantee hospital accommodation in 50 per cent. of the cases of confinement.

The real number of midwives at present in Russia, is 14,361, and the number of confinements which could be assisted by them is about 700,000. In the year 1912, out of 7,033,507 children born alive, only 25,240 were born in the lying-in hospitals; if we added to this number the stillborn children and the miscarriages, then 95 per cent. of the confinements occur without any obstetrical help whatever. Up to the present, the care of the health of the people has been chiefly in the hands of the municipal authorities of the towns and Zemstwos. These latter gradually improving, the medical assistance reached in some provinces the ideal, which was the object of the Commission mentioned.

Quite recently (September, 1916) there was instituted in Russia the chief Board of National Health, which is upon the same footing as a Ministry.

We can hope that it will also assist in the development of legislation and medical help in cases of maternity and in the diminution of morbidity and mortality among lying-in women and the mortality among the children.

Professor MUNRO KERR, M.D.

All of us interested in obstetrics and gynæcology must feel grateful to those who initiated this discussion, and particularly to these speakers who have so ably opened it. It may seem hypercritical, but to my mind a more suitable word might have been substituted for "need." The need is now admitted by every one. Every one admits that hundreds of lives, both fœtal and maternal, might be saved if pregnant women were better cared for—deaths from septicæmia, the toxæmias of pregnancy, forceps delivery, craniotomy, and from many other complications of pregnancy and parturition, could in great part be avoided. I do not say they could be absolutely abolished, but I am perfectly certain that 90 per cent. of the deaths that occur from these complications should not occur if pregnant and parturient women were looked after as they should be looked after, and as they will ultimately be looked after as preventive obstetric medicine advances, and social conditions are attended to and improved. What we really want to recognize then, is not the need for improvement, but the means to be employed for securing an improvement in the care of pregnant women.

There are two bodies of people concerned in this problem: (*a*) *Those who require to be cared for;* (*b*) *those whose duty is to help those who require care; and until each of these groups of people work together willingly and harmoniously ideal conditions will not be reached.*

In the remarks I have to make I desire to consider the subject under these two heads mentioned.

(*a*) *Those who require to be cared for.*—All pregnant women require to be cared for, but you will admit, I think, that as one passes down the social scale the amount of care pregnant women receive steadily declines. At the one extreme are the people in comfortable circumstances who place themselves under specialists or properly skilled and careful general practitioners. At the other extreme you have those who seek no medical advice during pregnancy unless they become extremely ill. The care of the former, those at the top of the social scale, is satisfactory, and improvement can only result as our knowledge of pregnancy and its complications increases. At the other end matters are very different, largely owing to ignorance and the casual-drifting, hand-to-mouth, existence of these individuals, upon whom many of the diseases

of pregnancy slowly and insidiously steal until they are brought to hospital dying or doomed to death or permanent ill-health.

But between the two I am sorry to say there is a large body of the lower middle-class who, partly from their own fault (ignorance, the desire not to incur expense, &c.), and partly because their medical attendants do not fully appreciate the necessity of giving special attention to the pregnant condition, also drift into a diseased condition, often serious, sometimes even fatal.

I repeat conditions in the first group are satisfactory. We can leave it alone. The lowest group is absolutely unsatisfactory, the middle group is not quite satisfactory; but the middle group can be readily made satisfactory, because it is a body of people amenable to reason and instruction, and attended by a body of medical practitioners who only require to be impressed with the necessity of caring for pregnant women more than they have done in the past. But the lowest class is very different. Appeal to them is futile; considerate but firm legislation is the only way in which you can secure an appreciable improvement in the present state of matters, which all of us are agreed is deplorable, and which we are determined shall not be allowed to continue.

You naturally ask: What legislation do you propose? My proposal is that an intimation of pregnancy should be made compulsory. Some years ago, at the International Congress on Infantile Mortality held in London, at which I was asked to read an address, I brought forward this view; it received no support, neither did it receive any support when the subject of Infantile Mortality was considered at the International Medical Congress in London in 1913. But I am more convinced every day that some such legislation is necessary if we are really to make appreciable progress. The women who elect to be attended by medical practitioners should intimate their condition to these practitioners at an early date. After the confinements are over the practitioners should send in their returns with the dates when the pregnancy was intimated to them and they confirmed the pregnancy. With the poorer classes, attended by midwives, the intimation should be made to the authorities, Public Health Authorities, or others who are now to be made responsible for the care of pregnant women, and the penalty for not making intimation at a reasonably early date might be that deduction from the maternity benefit would be made.

I should like very much to go into further details regarding this proposal, but time will not permit. This only would I say: that

I cannot see that any hardship would result from such a scheme, or that the sensitive and delicate feelings of prospective mothers would in any way whatever be outraged if the scheme were properly considered and arranged.

(*b*) *Those whose Duty it is to help those who require Care.*—There are three bodies of people who can help: (1) Obstetric and general practitioners engaged in midwifery practice; (2) Public authorities; (3) Private individuals interested in infantile mortality and the care of pregnant women.

Obstetricians can do an enormous amount. After all they are the expert advisers by instructing medical students, midwives and nurses, by establishing clinics in connexion with maternity hospitals. In many other ways, they can, and most of them are already doing invaluable work. Many practitioners in general practice, are helping also, but they could do more, they could encourage still further pregnant women to consult them, and they could employ the specialist more frequently by sending these poorer patients earlier to hospital.

Public Health Authorities.—It is for these to see that the pregnant women are cared for—they are responsible for the establishment of antenatal clinics in large towns or the proper supervision by local practitioners in smaller towns, villages and country districts, for the supply of skilled midwives and many other matters I have not time to mention. By the Notification of Births Extension Act, 1915, the responsibility not only for the care of infants, but for the care of prospective mothers, nursing mothers, infants and children up to 5 years is handed over to the Public Health Authorities.

Take my own city, the City of Glasgow: the Public Health Authorities propose to have an ante-natal clinic at the Maternity Hospital, and at such of the infant consultations and tuberculosis dispensaries as may be necessary. There is also a proposal, and there is every chance that it will materialize, to open wards for the indoor treatment of ante-natal conditions, the Corporation to contribute £1,000 per annum to its maintenance. Thus in Glasgow the obstetricians and the Public Health Authorities are mutually assisting each other and co-operating to establish a more satisfactory state of matters. Midwives will also have to be supplied by the Public Health Authorities, and probably some mutual agreement between the bodies mentioned will readily be established. In any case the more difficult and complicated cases will be drafted into the Glasgow Maternity Hospital; some-

what similar arrangements will be made regarding the post-natal department.

In the Maternity Hospital we have recently appointed an almoner and are erecting a clinical laboratory.

Private organization and individuals interested in infantile and maternal welfare have been given an enormous field upon which to expend their energy in the crèches, day nurseries, playgrounds, kindergartens, school of instruction for students, mothers and infants. The large hospitals supported by voluntary subscriptions will, I believe, soon be a thing of the past—it will be impossible to keep them going on voluntary subscriptions, but there are hundreds of schemes for bettering the domestic conditions of the people, which, for many years, must be supported by voluntary efforts and the generous public.

Professor Henry Briggs, F.R.C.S. (Liverpool).

I cordially approve of the manner in which the discussion has been introduced and opened. I came to the meeting chiefly to hear the views of others; my own remarks will be brief. For the widest possible practical application of a maternity and ante-natal scheme, co-operation is essential; co-operation amongst the many individuals and the many public bodies, voluntary and legal, moves ahead slowly. We have just heard what has been so well done in Glasgow. At the Central Midwives Board meetings I have been long familiar with the efficiency of administration in London, Manchester, and other popular centres. Dr. Hope, of Liverpool, suggested to me that I should ask that the example of Liverpool should be followed; in my own city co-operation underlies the welfare of the patient, the midwife and every one concerned. Amidst a steady general advance there has been trumpeting on the part of those who are afraid or already have caught the ante-natal fever more or less badly. Extension and widening of maternity and ante-natal welfare are certainties, and whether ultimately there is to be an "intimation" or "notification" of pregnancy, its adoption is to be expected when the moment is favourable. It was Mathews Duncan who said that the most difficult acquirement in midwifery practice was patience.

Miss Rosalind Paget.

We midwives quite realize that there is much room for improvement in regard to ante-natal care, and that we, like others, have not in time past paid enough attention to this branch of our work as we should have done; we have now for a considerable time been doing our best to remedy this defect. From our standpoint may we urge that improvement greatly depends on the following points being attended to:—

Early booking.

The provision of up-to-date instruction for midwives in regard to ante-natal questions.

The provision of facilities for adequate medical treatment when required for midwives' cases.

Encouraging the right class of woman to take up midwifery by improving her status, recognizing her responsibilities, and making it possible for her to earn a decent living.

Early Booking.—This is difficult to attain. The maternity benefit has considerably interfered with it. Any form of notification of pregnancy would prevent it, especially amongst the independent self-respecting women who form a large part of the patients in many midwives' practice, and who are the backbone of the nation. Many midwives' cases do book early, but great tact is required in regard to all investigations.

We midwives feel that our position as the chosen servant and friend of our patients gives us a unique opportunity for usefulness in regard to discovering early some of the causes of abortion, stillbirths, and premature labours (some, let us hope, preventable if taken in time). We get early cognizance of drug-taking, of the existence of constitutional disease, of the alcoholic habit, and of many other unhygienic conditions, and we are able to persuade our patients to obtain medical treatment, and here comes in the need for facilities for obtaining adequate medical treatment. If you will provide this for us, we will see that our patients make full use of it, and we will endeavour to enforce your orders.

We ask you not to ignore and waste the work of the midwife, but to include her in any scheme, because she attends 50 per cent. of all births and about 75 per cent. of working-class births, and this is increasing owing to war conditions. If she is competent to undertake

the conduct of labour she is surely able to give ante-natal care, and recognize when medical advice and treatment is required.

It is said that some midwives are not competent: this is sadly true, but the number is rapidly decreasing owing greatly to the admirable teaching of the inspectors, who are the midwives' truest friends. If midwives are not competent there is very efficient machinery to remove them from *their* register, and it should be used.

From every point of view it is advisable to recognize the work of the midwife and make use of her in any scheme. She is a necessity: she is in possession of from 50 to 80 per cent. of the mothers. Nothing is suggested to take her place, therefore she should be made as good as possible, and the best women encouraged to practise. Good candidates are decreasing in number every day. The ranks of the practising midwife are being depleted; they find work as health visitors, sanitary inspectors, lecturers—better paid and without the wearing anxiety of midwifery practice; as it also takes several years to make a good midwife this is a poor look out.

From the economical point of view also it is extravagant not to use material ready trained and on the spot. Since the future of the Empire depends entirely on the coming generation, to exterminate the good midwife would be a national calamity.

Sir Francis Champneys, Bt., M.D.

I came this evening to hear what was to be said on the subject of ante-natal care, and I have heard much to interest me. Dr. Moore referred to the question of comparative puerperal mortality in the hands of doctors and midwives. Some years ago the Central Midwives Board tried to persuade the Registrar-General to add to the certificate of birth the name of the person who actually delivered the patient, but were unsuccessful. This addition would have been very useful in many ways. Dr. Moore said that in his experience midwives usually called in the doctor too late. If this is the case, why has not the local supervising authority of Huddersfield referred such midwives to the Central Midwives Board? According to the Rules (E 20) a midwife is bound to advise medical help "as soon as she becomes aware" of the pathological condition, and she is liable to be removed from the Roll for any delay.

Dr. Routh seems to think that ante-natal care is a new discovery. No doubt the great interest recently taken in it is new; but surely Dr. Routh himself, as a careful physician, has looked after his pregnant patients long before the present movement, like other careful practitioners. He also said that by the Rules of the Board (p. 22), the care of the midwife seems to begin with labour. I hold the Rules in my hand, and the only thing which I can find to the point on p. 22 is Rule E 7, which runs as follows: "A midwife in charge of a case of labour must not leave the patient without giving an address by which she can be found without delay." To ask a midwife to leave her address with all her pregnant patients is surely not necessary, even if it were practicable, nor would the words "leave her patient" apply. I think we all agree that the care of pregnant women and unborn children is a work of great importance. In order to render any scheme successful, it is absolutely necessary to enlist the sympathy of all existing workers, especially of doctors and midwives. The doctors should be assured of their fees when they are called in to midwives' cases. For this omission they have largely to thank the direct representatives on the General Medical Council at the time of the consideration of the Midwives Bill, whose one object seems to have been to prevent any legislation. The difficulty was anticipated in Liverpool, where, by the wise forethought of Dr. Hope, an arrangement was made with the general practitioners at the very beginning, and no trouble, I believe, has been experienced.

As regards midwives, they are acknowledged to be a necessity. Doctors are quite unable to attend all the labours in the country, and many must be attended by midwives of some kind. The only choice is between certified midwives and handy women. The operation of the Midwives Act produced a sudden fall in puerperal mortality, which has been maintained since it came into force. If it is still higher than it should be, we must remember that doctors as well as midwives attend midwifery. If there is a shortage of midwives, and handy women return into practice, the puerperal mortality will certainly rise again. Practising midwives are getting steadily fewer. They are a necessity, and are valuable public servants. If all interest is taken from their work, they will become absolutely insufficient in numbers for the work of the country, and disaster will follow. The standard required of them is rising, and the incompetent midwives are being eliminated.

In any scheme the inspectors of midwives are of great importance.

As a body they are admirable; most of them are medical women. Health visitors are a difficulty. It is hard to find enough women with necessary experience and character. Compulsory notification of pregnancy will certainly at the present time defeat its own object. To impose it from above would lead to a reaction on the part of women and of their husbands, which would render it futile, and would certainly tend to diminish the birth-rate. If notification of pregnancy is ever to come in it must be by the wish, and not in spite of the protests of the mothers of England.

Section of Obstetrics and Gynæcology.

President—Dr. G. F. Blacker.

(*March* 1, 1917.)

A Uterus containing a Carcinoma and a Sarcoma.

By J. P. Hedley, M.C.

The specimen consists of the uterus and both appendages. The uterus is laid open from the front. The cavity of the body is distended by a somewhat pedunculated spheroidal tumour, 6·3 cm. (2½ in.) in chief diameter, which is attached to the posterior wall. Microscopically this exhibits the structure of a spindle-celled sarcoma, without any unusual characters; the free surface for a narrow zone is necrotic. There is no indication that the tumour has arisen by the sarcomatous transformation of a fibromyoma although this is not improbable. In the immediate neighbourhood of this, on its left-hand side, the uterine mucosa is the seat of a somewhat prominent papillary formation which is histologically a columnar-celled carcinoma of the adenoid type—i.e., the cell groups are provided with a glandular lumen bounded by a single series of columnar cells. Neither ovary is obviously enlarged, but both are secondarily diseased, and microscopically present an adenoid carcinomatous infiltration like that of the endometrical neoplasm. The left ovary has been torn in its removal, owing to its having contracted adhesions with the parts around. The Fallopian tubes are of normal diameter and present no indication of being affected either by direct extension or implantation.

I removed the specimen on August 5, 1916. The patient was aged 66, and a virgin. She had always enjoyed good health, and reached the menopause at about the age of 50, without marked disturbance of any kind. Her mother was alive and well. She was sent to me on account of the appearance of vaginal discharge: this

started in February, 1916, and was thin and watery but frequently blood stained. Six weeks before I saw her (in July) it had become brown, but had increased considerably in amount. On examination I could find no abdominal tumour, but, by vagina, enlargement of the uterus was obvious. The cervix was healthy.

I regarded the case as one of carcinoma of the body, and advised exploration of the uterus and hysterectomy if necessary. Under the anæsthetic I passed a sound into the uterus, which caused free bleeding, so I decided to go straight on to abdominal hysterectomy without spending time on exploring the uterus further. The operation presented no great difficulties, but fixation of the left ovary by adhesions to the back of the broad ligament and surrounding peritoneum caused some difficulty in separating the uterus on that side.

The subsequent progress was good and uneventful, except for slight solid œdema of the left leg, but there was no rise of temperature, and the patient was able to get up and be out of doors in a chair on September 7. She was apparently quite well when last I heard of her a month ago.

Cases of this nature are of considerable rarity and very few have been recorded in the *Proceedings* of this Section. In 1905 Dr. Spencer brought forward a case and gave short accounts of fifteen cases that he was able to find in the literature. In my case the involvement of the ovaries by carcinoma is interesting. It is possible that the sarcoma originated in a fibromyoma, and the fact of the menopause having been delayed until the age of 50 may be regarded as some slight evidence of the presence of a fibroid at that time.

Professor Shattock, who has kindly examined the specimen and furnished me with a description of it, can find no evidence of the transformation of a fibromyoma.[1]

(*March* 1, 1917.)

Carcino-sarcoma Uteri.

By Herbert R. Spencer, M.D.

Eleven years ago I showed at a meeting of the Obstetrical Society of London a specimen of "carcino-sarcoma uteri" in which an adeno-carcinoma and a round-cell sarcoma existed side by side in the same

[1] For report of Pathology Committee *see* p. 134.

uterus, and I gave a short *résumé* of fifteen cases of which I had been able to find the records.[1]

To-day I show another specimen in which a carcinoma and a spindle-cell sarcoma co-existed in the uterus; but in this case the typical adeno-carcinoma shows in places a sarcomatous stroma and the typical spindle-cell sarcoma shows carcinomatous changes in the involved glands: to these conditions the names of "carcinoma sarcomatodes" and "sarcoma carcinomatodes" have been given by Virchow, Ehrlich and others. These names are not very appropriate, and for the present it would appear to be better to speak of those cases in which the growths exist side by side simply as "co-existent carcinoma and sarcoma," and to limit the name "carcino-sarcoma" to cases in which the growths are intermingled, as in the specimen now shown. A tendency of the nomenclature to take this direction is seen in the publications of R. Meyer,[2] Forssner,[3] and Shattock.[4]

True "carcino-sarcoma," as thus defined, appears to be very rare; but this apparent rarity may be due to the fact that a sufficient number of sections have not been made to exclude intermingling of the growth. Should further research prove such intermingling to be commoner than is now believed (which I think is not unlikely), the term "carcino-sarcoma" may be used as convenient for indexing cases of co-existent carcinoma and sarcoma, whether the growths are separate or combined.

Briefly, the notes of the case are as follows:—

Mrs. H., aged 56, a nullipara (married four years ago), was seen by me on June 13, 1904. She complained of bleeding and a little offensive discharge for the last three months. Menstruation began at the age of 13, was always regular, occurring every four weeks and lasting seven days. It ceased three years ago. A polypoid growth had been found protruding from the cervix by her doctor, who removed it a fortnight before I saw her. A report by the Clinical Research Association declared it to be a spindle-cell sarcoma.

On examining the patient on June 13, I found the cervix opened to the size of a shilling and occupied by uneven masses which bled on examination. The body of the uterus was movable and apparently not enlarged. There was slight thickening of the cellular tissue to the left of the cervix.

[1] *Obst. Soc. Trans.*, xlvii (for 1905), p. 338.
[2] *Veit's Handbuch der Gyn.*, 1908, iii, p. 500.
[3] *Archiv f. Gyn.*, 1909, lxxxvii, p. 445.
[4] *Proc. Roy. Soc. Med.*, 1917, x (Sect. Path.), p. 48.

On June 18, 1904, the uterus was removed at the patient's house by vaginal hysterectomy with the Pacquelin's cautery. The operation was rendered difficult by adhesions and the growth was much more extensive than had been supposed, causing the uterus to tear during removal. The parts were extremely vascular, so that in addition to the ligatures three pairs of forceps were left on bleeding vessels for three days (the same extreme vascularity, rendering necessary the use of forceps to stop the bleeding, was found in my other case). The patient recovered from the immediate effects, but gradually failed, and died five weeks after the operation. A post-mortem examination was not made.

Description of the Specimen (fig. 1).—The uterus measures 9 cm. in length and is covered with adhesions. On its surface are seen two small fibroids of the size of a pea and bean. These are fibromyomata undergoing hyaline degeneration, under the microscope. The cut surface of the uterus is scorched in places by the cautery. The uterus has been laid open. The wall is 1 cm. thick. The rim of the external os, and the lower 5 mm. of the canal, are unaffected, but the whole of the rest of the cervical and lower corporal cavity up to within 2 cm. of the fundus is occupied and distended by a ragged growth, ulcerated in places, and in others occupied by polypoid projections. A section of the upper cervical wall shows that the growth has penetrated the wall to within 2 mm. of the outer surface. In the upper part of the body is seen a smooth polypus of the size of a haricot bean, and near it a smaller irregular shaped polypus of the size of a pea with a pedicle about 2 mm. in diameter. The mucous membrane of the body of the uterus elsewhere is atrophied and its surface smooth.

Microscopic Structure.—(*a*) *The smooth polypus* of the size of a haricot bean remaining in the body is a mucous polypus, covered in great part with columnar epithelium and containing glands lined with a single layer of columnar epithelium, some of which are dilated and occupied by shed epithelial cells. The stroma shows in places dense connective tissue, but there is no sign of malignant change in the glands or stroma.

(*b*) *The smaller irregular-shaped polypus* seen in the body of the uterus is similar in structure to the larger polypus, but at one spot there is definite sarcomatous change in the stroma, the cells being large and mostly spindle-shaped. Many of the glands in this polypus are surrounded by an area of hyaline degeneration which stains red with eosin. The stroma cells in this area are much enlarged and some are vacuolated: they might be mistaken for sarcoma cells, and I think they

may be the same as the "periglandular sarcoma" of Winter and Ruge; but there can be little doubt that the cells in my case are merely swollen and degenerated. The occurrence of these swollen hyaline cells around the glands suggests that the secretion or discharge sets up the degeneration and perhaps is the cause of the sarcoma also.

(*c*) *The main ragged growth* in the body is a carcinoma, consisting of tubules lined for the most part with a single layer of columnar epithelium which deeply penetrates the muscular wall. The epithelium



FIG. 1.

The uterus laid open showing the large ragged carcinoma in the lower body and upper cervix. On the right side is seen a dark raw surface, to which the spindle-cell sarcoma was probably attached. In the upper part of the body are seen a smooth mucous polypus of the size of a haricot bean, and a smaller irregular mucous polypus which shows sarcomatous change in the stroma.

in places shows some tendency to proliferate and form papillary ingrowth into dilated cavities (fig. 2). In some parts the tubules are embedded in a fibro-muscular stroma having the usual appearances; but in other parts the stroma consists of large spindle cells with

capillaries running between them, having the typical appearances of a spindle-cell sarcoma, and contrasting distinctly with the normal stroma in its immediate neighbourhood (fig. 2).

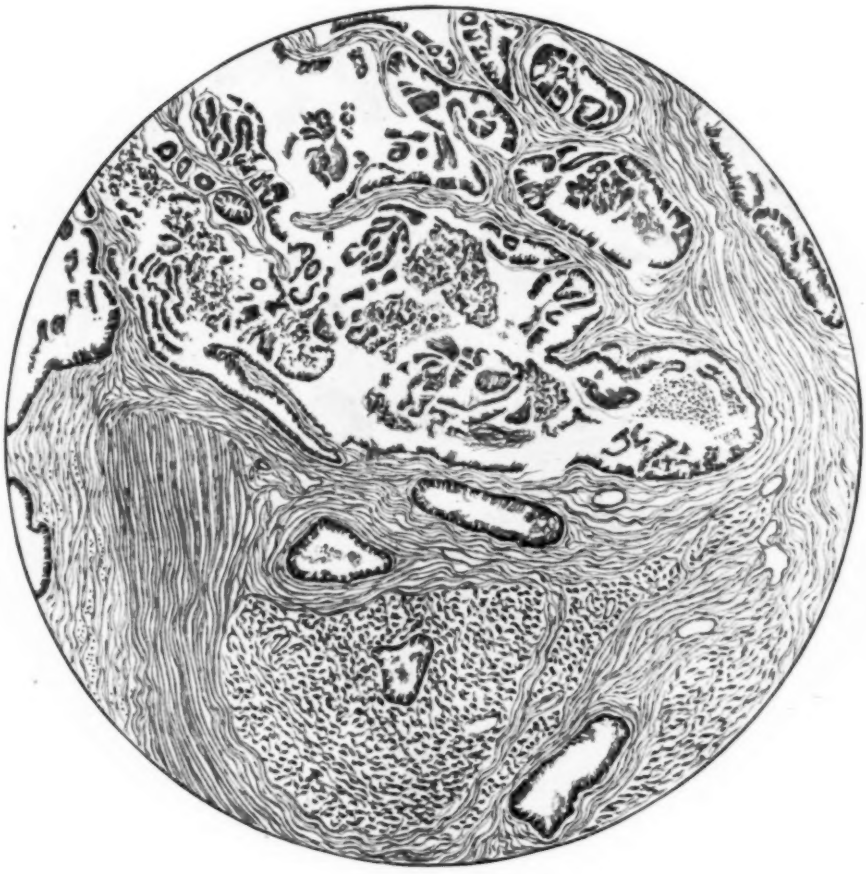


FIG. 2.

Section of the carcinomatous growth showing adeno-carcinoma with some papillary ingrowths, deeply invading the muscle. Below are seen two areas of spindle-cell sarcoma, in one of which is seen a gland lined with desquamating columnar epithelium. The sarcoma cells contrast distinctly with the fibro-muscular cells of the stroma. The great bulk of this tumour is adeno-carcinoma, but there are a few other patches of sarcoma. In only one patch is there an included gland, which has been figured as interesting in connexion with the microscopic structure of the small irregular-shaped polypus (*see b*, p. 84).

(*d*) *The polypus removed before the hysterectomy* shows the typical appearance of a large spindle-cell sarcoma (fig. 3); but scattered through the sarcoma are seen a few uterine glands the epithelium of which is commencing to proliferate and in two or three places has formed large masses of epithelial cells having the appearance of squamous cells. The glands are clearly carcinomatous (fig. 3).

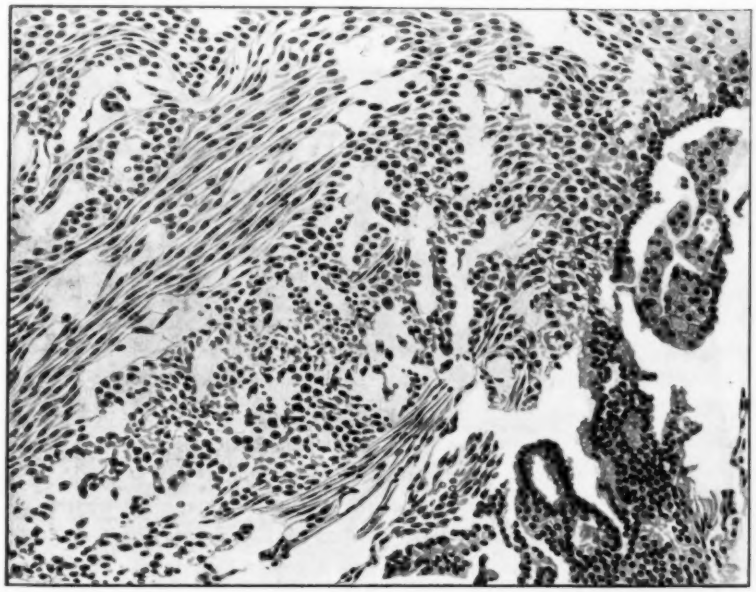


FIG. 3.

Section of the sarcomatous polypus removed before the hysterectomy. The growth is a large spindle-cell sarcoma. On the right is seen a cancerous area made up of the proliferation of glandular epithelium. Several of these carcinomatous areas and glands were found sparsely scattered through the sarcomatous growth.

[Photomicrographs of this growth, made in the laboratory of University College Hospital Medical School by Dr. Butterfield, were exhibited, showing the typical sarcoma and the carcinoma involved in it.]

The polypus removed before the hysterectomy was probably attached at the site of the rough area seen in the right side of the body (fig. 1).

It is impossible to say in this case whether the carcinoma gave rise to the sarcoma, or vice versa. From the more extensive growths of the carcinoma it is probable that it gave rise to the sarcoma, a probability increased by the proved possibility of producing sarcoma experimentally in animals by inoculation of successive strains of carcinoma.

The occurrence of definite sarcoma in the stroma of the little narrow-stalked fibro-adenomatous polypus, which shows no other sign of malignancy, is noteworthy.[1]

(*March* 1, 1917.)

Simultaneous Bilateral Tubal Pregnancy.

By Cuthbert Lockyer, M.D.

On the night of April 28, 1916, I received an urgent message to go into the country to see a lady who was very ill and who, five days previously, was thought to have had an abortion. On my arrival at midnight I found the patient suffering from very severe pain in the lower abdomen, both sides being equally affected. The pulse-rate was 140, the beats were scarcely perceptible, and the patient was blanched, restless, and evidently in extreme pain. On examination the abdomen was distended, rigid, and tender. There was a mass on the left side rising nearly to the level of the umbilicus. Its outline was indefinite below and towards the mid-line, but its upper limit was easily made out. On the right side there was extreme tenderness and rigidity, but no swelling could be felt. *Per vaginam* the os was patulous and the cervix soft. The uterine body was enlarged and anteverted. Behind the uterus, the pouch of Douglas was filled with a tender mass which on the left side felt solid, but on the right was less firm and more ill-defined.

On inquiry into the history of the case I learned the following facts: The patient was aged 26. She had been married fourteen months. The monthly periods had been regular until February. There had been no period in March, but a blood-stained discharge started about April 1 and had continued daily. For this reason the patient had kept to her bed during the whole month of April up to the time of my visit. On April 20 sharp colicky pains started, and on April 23 a membrane was passed *per vaginam*. At that time this was thought to constitute

[1] For report of Pathology Committee *see* p. 134.

an abortion. The next day, however, a swelling appeared in the left iliac fossa and the abdomen became greatly distended. There had been no sickness. On April 27-28 the patient became much worse. There was an exacerbation of pain, which was now generalized over the whole lower abdomen, and the doctor telephoned requesting me to come prepared to operate. Laparotomy 2 a.m., April 28, 1916. The abdomen was full of fluid blood and blood-clots. The bleeding was proceeding from the right side of the pelvis so that the right infundibulo-pelvic fold was speedily clamped and the clots cleared away. The right Fallopian tube was distended with clot, the latter projecting through its patulous abdominal ostium. The tube and ovary were matted together by clot and held by adhesions to the side and back of the uterus, so that the right appendages could only be separated and drawn up with great difficulty. The right tubo-ovarian mass was removed. There was still a large tumour to be dealt with on the left side of the pelvis and I found that the disturbance in drawing up the right prolapsed appendages had caused the left-sided mass to bleed afresh (the left side had appeared to be quiescent when the operation was begun). On investigation I found a large sac with very thick adventitious walls occupying the left half of the pelvis. It was very difficult to peel this structure off the rectum and sides of the pelvis; in fact, a portion of the sac-wall had to be left attached to the bowel. A large solid clot containing a central amnial cavity was found in the sac, but no embryo was seen. The left tube, which had apparently ruptured, was removed together with a small strand of adherent ovarian tissue. The uterus, which was enlarged, was not removed. The operation was long and tedious (seventy-five minutes) and the difficulties were aggravated by poor illumination, the nurse having fainted and the anæsthetist having to perform the double task of holding a hand-lamp and keeping up the narcosis. Saline injections were administered after the patient was put back to bed, and following a desperate struggle for life, the patient made a complete recovery. This case is a very tragic one, for the young lady had ardently expressed the noble wish to be a mother of twelve children.

Pathological Report.

The tissues removed consist of the right Fallopian tube and right ovary (fig. 1, **A** and **B**), also the left Fallopian tube and a segment of the left ovary (fig. 1, **C** and **D**).

The right tube is widely dilated, its outer surface is roughened by

adherent blood-clot and adhesions. It measures 9 cm. in length and 3 cm. in its transverse diameter. It is slightly bent upon itself and projecting through the abdominal ostium is a clot which covers the posterior aspect of the ampullary portion of the tube and also the upper half of the ovary. In the centre of this adherent clot is an irregular aperture leading to a rent in the wall of the tube at the junction of the floor and posterior surface. This rent is situated 1·5 cm. from the patent tubal ostium: it runs parallel with the long axis of the tube and is 1·2 cm. in length. It is not seen in fig. 1, **A**, which represents an *anterior* view of the right adnexa. On dividing the right tube longitudinally (fig. 1, **B**), its walls are seen to be greatly thinned out and its lumen is divided into three loculi by two transverse septa. The two proximal loculi are empty and show a smooth lining of yellow ochreous hue. The outer loculus is filled with blood-clot, the latter projecting through the ostium, which measured 0·75 cm. in diameter. Beneath this clot is the rent referred to, and which is seen to communicate also with the central loculus on the other side of the dividing septum.

The right ovary measures 4 cm. by 2·8 cm. It is matted to the Fallopian tube by the adhesions and blood-clot which lie around the rupture. A large part of its anterior surface is also covered with clot (fig. 1, **A**). On section the organ is seen to be œdematous and cystic. No recent corpus luteum can be seen.

The left Fallopian tube (which was in communication with the large adherent sac limiting the big hæmatocele) is smaller than its fellow of the opposite side; in length it is 6 cm., and measures 2 cm. in diameter. It forms an oval swelling, with its outer end closed and covered by blood-clot. Beneath the tube the mesosalpinx has been opened up by blood-clot (fig. 1, **C**), and on dividing the specimen longitudinally it is seen that the floor of the tube has given way so that the lumen, which is full of clot, communicates with the hæmatoma in the mesosalpinx. The latter has also been torn right across, leading no doubt to a communication with the adventitious sac, which reached up into the abdomen. The left ovary is represented by an irregular tag of tissue (fig. 1, **C** and **D**) adherent to the engorged mesosalpinx.

Microscopy.—The left Fallopian tube was sent to the Laboratories of Pathology, 38, New Cavendish Street. The right tube was cut and examined in the laboratories at Charing Cross Hospital.

The Right Fallopian Tube.—A vertical longitudinal section taken through the site of rupture shows the tube-wall to be thinned out and very degenerate, the nuclear staining is lost and the fibromuscular fibres

are only distinguishable by their arrangement—they are overlain by a lamina of blood-clot to which a few chorionic villi adhere. Within the distended lumen are seen a few plicæ, cut in longitudinal section, but these are not traceable to their attachment. There are many chorionic villi surrounded by syncytium (fig. 2), with deeply staining nuclei, and which gives off plasmodial buds. Lying free in the clot are also

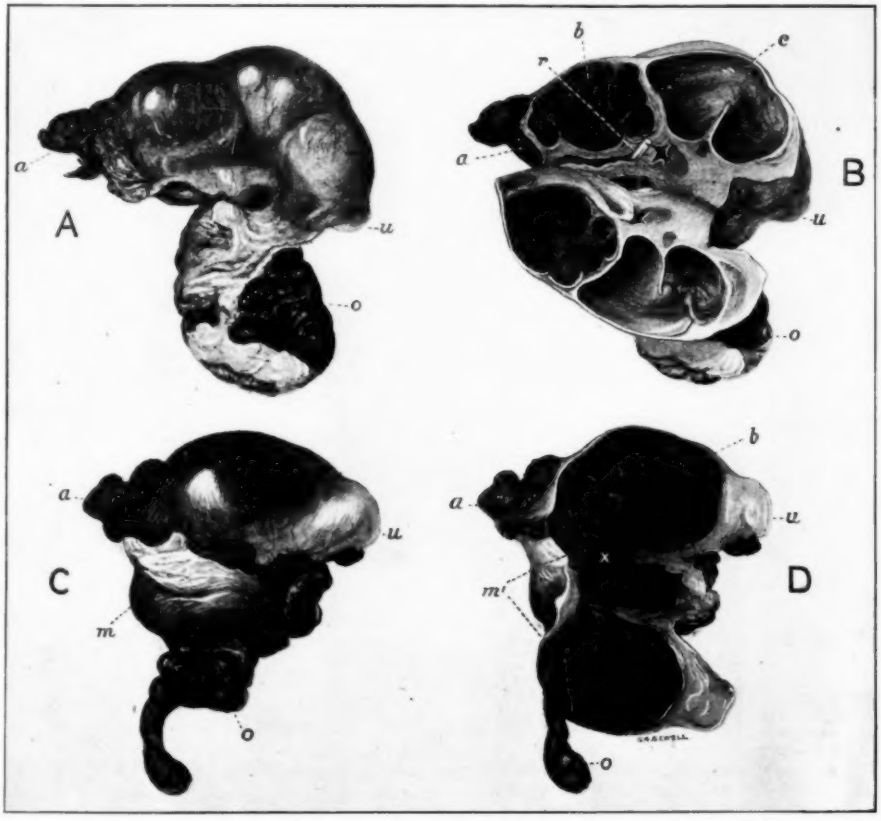


FIG. 1.

Simultaneous bilateral tubal pregnancy. **A**, right tube and ovary; **B**, right tube laid open longitudinally; **C**, left tube and portion of ovary. **D**, left tube opened longitudinally; *a*, abdominal end of tube; *u*, uterine end of tube; *o*, ovary; *b*, blood clot; ×, rent in floor of tube; *e*, cystic portion of right tube; *m*, mesosalpinx; *m'*, site of rupture of mesosalpinx; *r*, site of rupture of tube: approximately three-quarters natural size.

numerous syncytial derivatives in the form of large multinucleated and mononucleated cells.

The Left Fallopian Tube.—Sections were taken in the longitudinal direction from a portion of the tube-wall where the musculature was

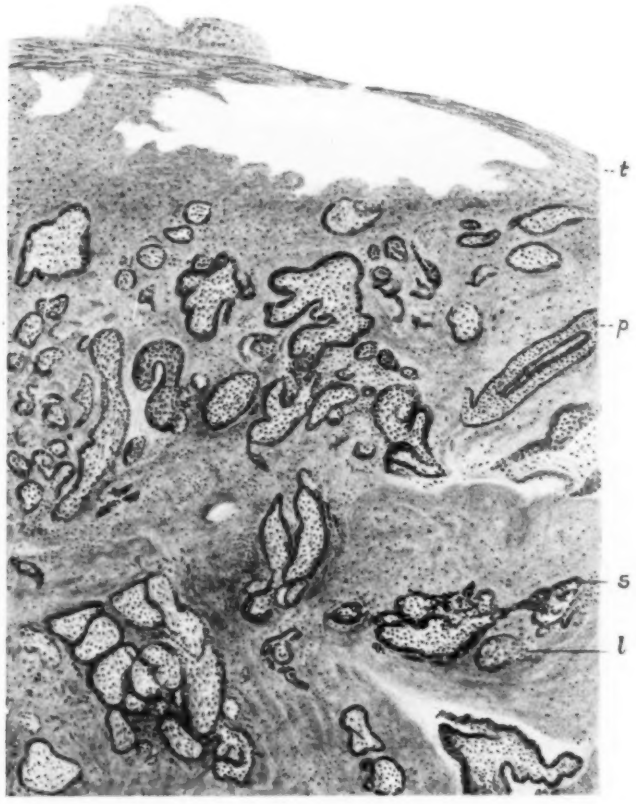


FIG. 2.

Simultaneous bilateral tubal pregnancy. Right tube near site of rupture. *t*, tube wall; *p*, plica; *s*, syncytial buds; *l*, cluster of Langhans' cells. (× 160.)

best preserved (fig. 3). They show the peritoneal coat to be thickened, and also in places covered with blood-clot. The intermuscular capillaries are engorged, and some are ruptured, causing interstitial hæmorrhages.

The plicæ have disappeared and their place is taken by branching chorionic villi which have attached themselves to the tube-wall. Deep to this attachment are seen other villi (divided in transverse section), and also many multinucleated cells and bands of syncytial protoplasm. The areas of invasion of the tube-wall show marked necrosis of muscle-tissue and blood-extravasations. Attached to a villus larger than the rest is a large mass of mononuclear cells with clear cytoplasm—evidently derivatives of the layer of Langhans. Judging from the

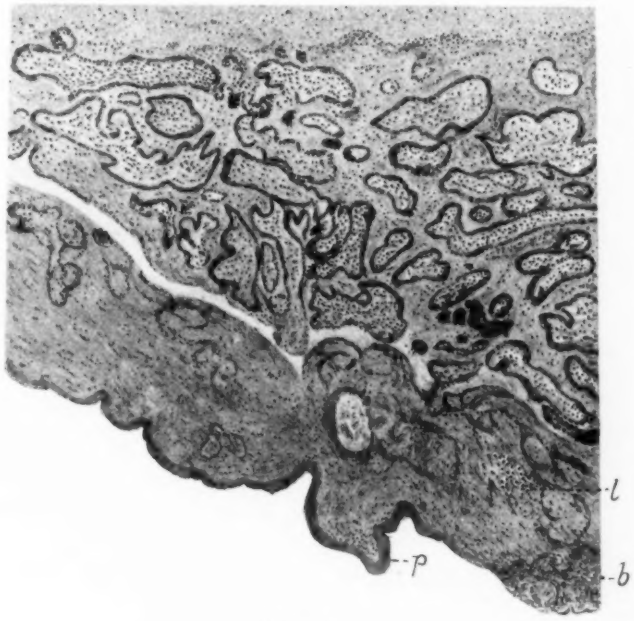


FIG. 3.

Simultaneous bilateral tubal pregnancy. Section through left tube, showing the villi invading muscular wall: *l*, Langhans' cells adherent to villus; *b*, adherent blood-clot; *p*, peritoneum (thickened). (× 160.)

similarity of the villi in each tube, both as regards their development and staining properties, it is fair to assume that conception occurred simultaneously in the right and left oviducts.

With intra-uterine gestation multiple pregnancy, occurring as it does in about one in eighty cases, is a fairly common incident; with

extra-uterine gestation multiple pregnancy is an event of sufficient rarity to justify it being recorded. The cases reported fall into three classes:—

(I) Co-incident intra- and extra-uterine pregnancy.

(II) Multiple pregnancy in a single Fallopian tube.

(III) Co-incident pregnancy in each tube: (*a*) Cases of successive or repeated gestation; (*b*) cases of simultaneous binovular gestation.

Class I is said to include the greatest number of cases and Class III the fewest. But for the latter part of this statement to be correct it is necessary to exclude from Class III (*a*) those cases in which the pregnancy of one tube is of older date than that of the other (a condition to which the term "twin-pregnancy" does not strictly apply, the pregnancy being successive or repeated). This reservation leaves in Class III only (*b*) the examples of simultaneous bilateral binovular pregnancy, which, from a careful survey of all the published cases, I have no doubt is the rarest form, not only of tubal pregnancy but of ectopic pregnancies in general—i.e., even when authentic ovarian gestation is included. I have satisfied myself that there are forty-one cases of genuine ovarian pregnancy in the literature, but can only find about thirty cases in which *simultaneous* pregnancies in both tubes have been proved. The question of abdominal pregnancy for the genus *homo* being unsettled, cases reported as such need not be considered. I can advance no figures to prove the relative frequency of the above three classes of multiple pregnancy; but in an excellent paper on the subject McCalla [15] states that he has found reported twenty-five cases in which *both* tubes were the seat of gestation, as compared with thirty-six, in which twins were situated in a *single* tube. The twenty-five bilateral cases in McCalla's list include both the repeated and the simultaneous varieties. It would appear from a survey of these same cases, made by two other authors (Proust and Buquet [21]), that only seven belong to the simultaneous type. It is to be hoped that McCalla will himself at some future date undertake to dissect these cases, and any subsequent ones, from the point of view of the rare *simultaneous* variety. The most remarkable case of Class II is that of Professor Treub of Amsterdam, in which a single tube contained quintuplets. But as we are not concerned with this class in the present paper further mention of *unilateral* multiple gestation will not occur. The literature devoted to bilateral tubal pregnancy is already fairly extensive. The subject attained considerable prominence in the British Empire in the years 1890-92 by the publications of Rowan [23], Doran [7], Savage [24],

and Walter [27]. Since this date it has only been dealt with at long intervals by British writers—i.e., by Haig Ferguson [8], 1899, Burford [3], 1905, McCann [16], 1906, Wilson [29], 1910, Tenison Collins [5], 1912, Lancaster and Moncrieff Barron [13], 1912 and Christopher Martin [18] 1913. The cases of Burford, Lancaster and Collins are the only *positive* examples of the rarest type of tubal pregnancy, simultaneous gestation in both tubes having been proved in each of these three cases. In Wilson's case [29] no chorionic villi were found, but the two tubes were ruptured and each showed decidual reaction. On this alone the case is accepted as positive by the French authorities (Proust and Buquet), who agree with the author in regarding it as the first authentic case ever published in Australia, thereby excluding Rowan's previously reported case, as I also have been obliged to do. My opinion about Wilson's case is that had the tissues been examined by a gynæcological pathologist probably villi would have been found; in point of fact they were examined by a veterinary surgeon. The earliest cases, those of Rowan and Savage, are not conclusive, and the report which Mr. Christopher Martin has been good enough to send me relating to his own case contains no histological record. Much to my regret, therefore, this case cannot be counted in my list (*see* remarks at end of this article). McCann's [16] case belongs to Class II—i.e., it was a case of uni-ovular twins with a single implantation. Ferguson's was a good example of *successive* or *repeated* tubal pregnancy, and the same applies to Walter's [27] most interesting case in which the clinical history showed that the patient missed a period in 1891 and was ill in bed for a month afterwards. A mass the size of an orange was found on the left side when she was examined in August, another period was missed in January, 1892, and after seven weeks' amenorrhœa a new "swelling" was found on the right side of the uterus. At the operation the pregnancy on the left side was found to be much more advanced than that on the right. In spite of these facts this case is included in eight "authentic" cases published by Labhardt [12] in 1909. This is the more remarkable because this writer notes that most authors do not insist on simultaneity, and stating that this ought to be done, he proceeds (quite rightly) to exclude the cases of Rowan [23], Doran [7], and Savage [24]. Similarly Launay and Séguinot [14] *accept* Rowan's case, which was clearly one of *unilateral* tubal pregnancy with hæmatosalpinx of the opposite side. In the same way Proust and Buquet retain only six of Jayle and Nandrot's twenty-nine cases. It should, however, be stated in this connexion that Jayle will

not admit the possibility of bilateral tubal gestation ever being simultaneous, and therefore did not, of course, attempt to prove that any of his cases were such. It is clear, therefore, that individual estimates are open to criticism, but as they afford an approximate estimate of the truth, they are not to be deprecated, especially as their compilation entails a vast amount of tedious labour such as only the few will undertake. I have carefully studied Doran's case in which the left tube, though ruptured through its ampullary portion with the corresponding ovary showing a ripe corpus luteum, no chorionic villi could be found in this tube; whilst on the right side the tube contained microscopic evidence of a molar pregnancy. I can well understand how it is that some writers include this case in their series. I agree with Proust and Buquet [21], however, in excluding it from a list purporting to contain authentic cases only. This is probably the right course to take in spite of the fact that I know of no other example of rupture of a hæmatosalpinx pure and simple (d'un hématosalpinx banal). My own case is therefore the fourth proved example of simultaneous bilateral tubal pregnancy to be published in British literature and the third example obtained in England.

I had assigned to myself the task of collecting all the hitherto unpublished examples of simultaneous bilateral tubal pregnancy when I found, with some relief, that the work had already been done up to 1914 by Proust and Buquet. These French authors reviewed eighty-two cases of bilateral tubal pregnancy and found among them thirty-three which they accept as genuine instances of simultaneous gestation. A complete bibliography is appended to their most valuable memoir, which forms the best account on record of this interesting and rare condition. Since this monograph appeared the War has intervened and very few cases have been published, but I find that the following are not included in Proust and Buquet's list, although they mention one of them (Hadden's) in the text.

(1) A case of double tubal pregnancy by David Hadden [10] is quoted in the *Zentralblatt für die gesamte Gynäkologie und Geburtshilfe*, November 5, 1913. The original article is only to be found in the *Californian State Journal*, and as this periodical is not to be obtained in London, I have been unable to test the validity of this case.

(2) Max Cheval [4], "Un cas de grossesse extrautérine simultanée des deux trompes avec avortivement tubaire bilatéral et formation d'hématoceles bilatérales." The patient, a quartipara, was aged 38; labours normal, the last one fourteen months previously; since then quite regular. Last period May

27, 1913. June 15: Uterine hæmorrhage, with pain in left side, followed by a brown discharge; tumour in left iliac fossa noticed four weeks later. Admitted August 20, with a tumour up to navel. Operation, August 23: Villi were demonstrated in the right tube and in the left hæmatocele.

(3) Crousse [6], "Deux cas de grossesses tubaires bilatérales." (*a*) Patient, aged 35, nullipara, taken ill eight days after last period. During the next month a large hæmatocele formed. Right tubal abortion and a left hæmatocele containing gestation products of older date; recovery. (*b*) Patient, aged 38, one partus seventeen years previously; one abortion fifteen years ago. Hæmorrhage continuous since last period. Frequent fainting and severe pain. Large retro-uterine hæmatocele caused by left tubal abortion. Right hæmatosalpinx with a dead ovum inside. Probably simultaneous age. Recovery.

(4) Mr. Christopher Martin [18] has kindly sent me the following letter: "I have looked up the notes of the case of bilateral tubal gestation I reported at the Midland Obstetrical and Gynæcological Society in December, 1913. The patient was a Mrs. Y., aged 38, of Walsall. She had had no children, previously, but three miscarriages. Her periods were regular till July, 1913. Then she had irregular hæmorrhages with one severe loss in August. I operated on her in a private home on October 30, 1913. I opened her abdomen and found she had (i) six small myomata of the fundus of the uterus; (ii) a cystic left ovary; (iii) double tubal gestation. Both tubes were full of old clot, there being a 'mole' in each tube. There was a large hæmatoma (localized intraperitoneal hæmatocele) in connexion with the left tube. There was no sign of a fœtus in either tube, but the mole had the ordinary placenta-like appearance, though no microscopical examination was made. I removed both tubes, the left ovary, and the body of the uterus (sub-total hysterectomy). The patient made a good recovery and left on November 25, 1913. I believe the specimen is in the Pathological Museum of the University of Birmingham. From the appearance of the tubes I believed that the pregnancies were simultaneous." (See note at the end of this article.)

(5) Phahl, F. [20]. "Gleichzeitiger Schwangerschaft beider Tuben.' Ruptured left tube and right-sided tubal hæmatoma with tubal abortion. Chorionic villi in each tube. Corpus luteum only in left ovary.

It is possible that all these five cases are genuine, but with scanty references, or in the absence of histological data, it is impossible to be certain of four. There need be no doubt, however, in accepting the case of Max Cheval. This would appear to bring the number of published cases of simultaneous bilateral tubal pregnancy up to thirty-four, making my own case the thirty-fifth. This estimate of "proved" cases is, in my opinion, too high, because I cannot bring myself to agree with Proust and Buquet in accepting as valid the proofs of simultaneity advanced in the cases of Launay [14], Robins [22],

Weinlechner [28], Savage [24], and Balleray [1], and I have doubts in the cases of Greenberg [9] and Sussmann [25]. This means that I am disposed to exclude at least five, if not seven cases of Proust and Buquet's thirty-three; in Launay's [14] case (which is interesting from the fact that on the right side there were two small fœtuses whilst on the left there was a three months' fœtus and its placenta) the evidence of simultaneity is not forthcoming, at any rate it is open to doubt. In Robins's [22] case there is no histological record, and Proust says, "We *suppose* that it was a simultaneous bilateral pregnancy"; the evidence being that the right tube, which was not ruptured, contained an ovum, and that the left was ruptured with an ovum adherent outside. What is the evidence of simultaneity?

Weinlechner [28] reported that the two tubes in his case were filled with clot, and that two corpora lutea were found, but he gives no histology of either tube and makes no mention of embryos. In the case of Savage there was no microscopic examination and no embryos were found. In Balleray's [1] case there was a fœtus in the right tube, and we are told that the left tube was the seat of an ectopic gestation, but, as Proust himself points out, Balleray gives no account of the examination of this tube. It should, therefore, be excluded from a list of simultaneous pregnancies. In the example of Greenberg [9] the left tube was filled with blood-clot and ruptured. The right tube was adherent to, and had perforated, the bladder, causing copious hæmaturia. No histological account is given. Proust and Buquet say "We are inclined to think that the gestation was simultaneous but the rupture perhaps successive," a conclusion evidently based on clinical grounds only; this may or may not be right. In Sussmann's [25] case the right tube showed, "without any doubt," that there had been a tubal abortion, so that a microscopic examination was not considered necessary, especially as in the clots, which were removed from the pelvis and which were subsequently lost; the debris of an ovum had been seen at operation. A few villi were found in the ruptured part of the opposite tube. Query as to evidence of simultaneity.

I wish to draw attention to these defects in the presentation of cases for publicity, because I hold that for the settling of the question of simultaneity, not the clinical bearing of the cases only, but also the macro- and microscopical details, should be fully reported. Not only is it impossible to decide this question on clinical data alone, but it is absolutely essential to base the final conclusion on histological evidence. Where this is wanting the cases may be dealt with as Whitridge

Williams disposes of those which purport to be examples of ovarian gestation—i.e., by classifying them as (*a*) highly probable, (*b*) probable, (*c*) doubtful. But for the present purpose I am content to exclude the above cases from the list of positive examples on the grounds that the proofs are incomplete. This leaves $33-7=26$ cases in Proust and Buquet's list; $5-4=1$ in the list I have made from cases subsequently published; and, finally, the case which forms the basis of this paper, thus making a total of twenty-eight cases of simultaneous bilateral tubal pregnancy to date.

Diagnosis.

It is practically impossible to make a *clinical* diagnosis of simultaneous bilateral tubal pregnancy. There is more chance of coming to a correct conclusion in successive bilateral cases as seen in the case of Walter [27], in which one tumour was detected some months before the other, and the appearance of each was preceded by a very suggestive illness.

Definite attacks of pain starting on the one side and then on the other have been noted, but in no case does the correct diagnosis appear to have been made, or even suggested, before operation. With the abdomen opened it may be easy to observe a tubal lesion on both sides; on the other hand there are instances where the bilateral nature of the lesion has been missed, necessitating a second operation (e.g., Boisleux's [2] case seven weeks later). When operating for extra-uterine gestation it is, therefore, most essential *to examine the appendages on both sides.* Even if the opposite tube be not the site of a gestation sac it is no infrequent occurrence to find it presents a hæmatosalpinx.

The coincidence of a hæmatosalpinx and tubal gestation has given rise to much speculation as to the causation of the tubal gestation. Doran [7] thought that it was caused, in his case, by a leak from the gravid tube, the blood reaching the opposite tube via the uterus. A more common acceptation is that the hæmatosalpinx (in the absence of torsion) is the result of intense congestion set up by pelvic hyperæmia due to pregnancy. Whatever the cause it is clear that a differential diagnosis between hæmatosalpinx and a gravid tube can only be made by the microscope, and it is to be remembered that this is often no easy task. In the case of Unterberger [26] the finding of chorionic villi in the left tube was extremely difficult in spite of the fact that when found they were well stained. The same difficulty arose in the case of Sussmann [25].

When the embryos are both preserved, and are of the same size, the diagnosis of "simultaneity" is assured.[1] But the mere presence of a fœtus on both sides is, of course, of no value in deciding as to simultaneous gestation — embryos of even date may show different developments, since one may die before the other—therefore actual simultaneity will, in some cases, be impossible of proof. Such cases should not be placed in the "positive" series. The existence of rupture, and the appearance of "evident abortion" (a term not infrequently used) when standing alone, only indicate a strong *probability* of bilateral tubal pregnancy. These lesions cannot of themselves decide the question of simultaneous gestation. The finding of two ripe corpora lutea has been advanced as proof of simultaneity (Weinlechner [28]), but, as in the majority of cases, only a single corpus luteum is found. I do not attach importance to this sign.

To sum up the question of diagnosis of simultaneity. It should be based upon thorough macro- and microscopic investigation, and the pathological findings should harmonize with the clinical history of a single impregnation ending in a typical crisis, or in two crises more or less contemporaneous, and in some cases it may be possible to make out that the pain is bilateral in a single abdominal attack.

When the onus of proof lies on the histological findings, the question of simultaneity is based on the character of the villi—i.e., on their relative size, development and staining properties. The villi in one tube should so closely correspond in all their features to the villi in the tube opposite that the sections might be interchanged without detection.

Treatment.

The mere fact of a bilateral tubal pregnancy being a possibility is an argument in favour of early surgical intervention during an acute abdominal attack. Although only one side may be ruptured, as in the cases of Johnson, Findley, Proust and Buquet, there may be an equally serious lesion on the opposite side in the shape of an abortion, and, in general terms, it may be said that a patient incurs a double risk from

[1] This was found in the case of Burford, in which the fœtus on the right side measured 38 mm., that on the left measured 41 mm. Each tube contained an embryo of about eight weeks' development in the case published by McCalla [15]. In Milligan's [19] case, each tube was said to contain a fœtus of three months' development. In Launay's [14] case there were twins on the right and a single fœtus of three months' development on the left side. In McDonald and Krieger's [17] case, each tube contained a fœtus. In Collins's [5] case in the left tube there was an amnial sac, 5 cm. by ½ cm., without a fœtus, and in the right tube there was a fœtus 5 cm. long.

the double lesion. In my own case, the patient was on the verge of death, and her extremity may have had to do as much with the additional pain as with the extensive hæmorrhage. The question of leaving or removing the uterus arises. It may be easier and quicker in certain cases to perform subtotal hysterectomy, removing both tubes and uterus *en masse*. I left the uterus and a portion of an ovary, and I feel sure that this was the right thing to do, especially as I hear by letter that the lady, who is now in America, is deliberating on the advice of a surgeon on the other side who wants to engraft the ovarian tissue into the uterine cornu with a view to favour conception. I do not think this effort, if made, would succeed, but there is no doubt that, from the patient's point of view, the possession of a menstruating uterus is a mental satisfaction as well as a physical asset.

REFERENCES.

[1] Balleray. *Med. News*, 1898, lxxx, p. 136.
[2] Boisleux. *Nouv. Arch. d'Obst. et de Gyn.*, May, 1892.
[3] Burford. *Brit. Gyn. Journ.*, 1905, xxi, No. 82, pp. 97-100.
[4] Cheval, Max. *Bull. de la Soc. belge de Gyn. et d'Obst.*, 1914, xxiv, No. 10, p. 456.
[5] Collins, Tenison. *Proc. Roy. Soc. Med.*, 1912, v (No. 9), (Obst. and Gyn. Sect.), p. 374.
[6] Crousse. *Bull. de la Soc. belge de Gyn. et d'Obst.*, 1913, xxiv, No. 5, pp. 316-322.
[7] Doran. *Brit. Med. Journ.*, 1891, ii, p. 789.
[8] Ferguson, Haig. *Edin. Med. Journ.*, 1899, xlvii (n.s. v), p. 145.
[9] Greenberg. *Journ. Amer. Med. Assoc.*, 1907, xlix, No. 7, p. 575.
[10] Hadden. *Californian State Journ. of Med.*, 1913, ii, p. 60.
[11] Jayle et Nandrot. *Rev. de Gyn. et de Chir. abdom.*, 1904, viii, p. 195.
[12] Labhardt. *Beitr. z. Geb. u. Gyn.*, 1909, xiv, pp. 155-161.
[13] Lancaster and M. Barron. *Austral. Med. Gaz.*, 1912, xxxii, p. 34.
[14] Launay et Séguinot. *Rev. de Chir.*, 1911, xliii, p. 401.
[15] McCalla. *Surg., Gyn., Obst.*, 1909, viii, pp. 248-254.
[16] McCann. *Journ. Obst. Gyn. Brit. Emp.*, 1906, x, pp. 628-631.
[17] McDonald and Krieger. *Journ. Amer. Med. Assoc.*, 1913, lx, pp. 1766-1769.
[18] Martin. *Journ. Obst. Gyn. Brit. Emp.*, 1913, xxiv, p. 331.
[19] Milligan. *Journ. Amer. Med. Assoc.*, 1912, lviii, p. 114.
[20] Phahl, F. *Gyn. Rundschau*, 8 Jahr. Heft 20.
[21] Proust et Buquet. *Rev. de Gyn. et de Chir. abdom.*, 1914, xxiii, pp. 353-404.
[22] Robins. *Atlanta Journ. Rec. of Med.*, 1901 ; *Frommel's Jahrb.*, 1901, p. 765.
[23] Rowan. *Austral. Med. Journ.*, 1890, n.s. xii, p. 265.
[24] Savage. *Brit. Med. Journ,,* 1892, i, p. 556.
[25] Sussmann. *Münch. med. Wochenschr.*, 1910, lvii, No. 25, p. 1341.
[26] Unterberger. *Monatschr. f. Geb. u. Gyn.*, 1913, xxxviii, pp. 247-251.
[27] Walter. *Brit. Med. Journ.*, 1892, vii, p. 732.
[28] Weinlechner. *Zentralbl. f. Gyn.*, 1905, xxix, No. 2, p. 46.
[29] Wilson. *Austral. Med. Gaz.*, 1910, xxix, p. 187.

DISCUSSION.

Dr. GRIFFITH: Has Dr. Lockyer taken into consideration another difficulty in determining, by examination of the specimens, "simultaneity" or not of double tubal pregnancy—namely, the changes in the chorion and sac wall which will arise in the event of the arrest of development of either embryo? The mere size of the sacs will be no guide even if both embryos are living, and may depend only on the amount of blood effused into them.

Dr. H. R. SPENCER: Does Dr. Lockyer claim that he can decide on the date of the pregnancy by microscopic examination of the villi? If so, it seems a large claim, in view of the remarkable way in which villi retain their vitality in some cases of retained ovum. I have published a case where a villus stained well after remaining thirteen months in the tube.

Dr. LOCKYER (in reply): The questions raised by Dr. W. S. A. Griffith and by Dr. Herbert Spencer will be found to have been answered in that part of the paper which I omitted to read for want of time. In simultaneous bilateral tubal pregnancy, when one embryo died before the other, it is impossible, after a considerable lapse of time, to *prove* simultaneity. Such cases should therefore be described simply as "bilateral tubal pregnancies" and the question of simultaneity should be left *sub judice*.

P.S.—At my request Mr. Beckwith Whitehouse has kindly sent me sections prepared from the two Fallopian tubes in the case of Mr. Christopher Martin. I find a few old villi in sections taken from "Tube No. 1" (presumably the left tube), but in "Tube No. 2" there are no microscopic evidences of gestation.—C. L.

(*March* 1, 1917.)

Metastatic Glioma (Neuro-epithelioma) of the Right Ovary in a Child aged 3.

By CUTHBERT LOCKYER, M.D.

I AM indebted to Mr. Norman Fleming for permission to show this extremely rare specimen. Being present at the autopsy of the patient, which was made in the Charing Cross Hospital, I was consulted as to the condition of the right ovary, and subsequently took charge of the pelvic viscera with a view to their preservation and for investigation.

A paper recording this case was read by Mr. Frank Taylor and Mr. Norman Fleming at the Section of Ophthalmology on November 1, 1916, but this has not yet appeared in the *Proceedings of the Royal Society of Medicine*. I was present at that meeting and here record such details of the paper written by the above authors as may prove of interest to this Section.

Mr. Norman Fleming stated in his clinical notes that the child, L. D., aged 3, was seen by Mr. E. W. Brewerton on February 14, 1916, who admitted her forthwith into the Royal Westminster Ophthalmic Hospital for a fungating growth protruding between the lids of the *left* eye. The right eye was described as being proptosed. The left orbit was exenterated and the right eye freely excised within an hour of admission. On March 11 the child was discharged, and on March 30 was re-admitted with a recurrence of the size of half a tennis-ball protruding from the right socket. This was cleared out on the day of admission, but the patient soon became very emaciated, the right socket filled up again, and death occurred on May 7.

At the autopsy about twenty rounded growths with raised edges, scarlet in colour, caused adhesion of the dura mater to the bones of the cranium; there was also a mass of growth in the position of the optic chiasma. Metastases were found in the mesenteric glands, on the anterior aspect of the body of the second lumbar vertebra and in the right ovary (*see* fig. 1). With the exception of the ovarian growth, the pelvic organs were quite normal. On naked-eye section of the right ovary its centre was seen to be occupied by a blood-clot measuring $\frac{5}{16}$ in. in diameter. This clot was surrounded by an area of new tissue forming a zone $\frac{3}{16}$ in. in thickness. Microscopic sections showed a normal ovarian cortex containing many primordial follicles deep to which came the tissue of the new growth, the characters of which are indistinguishable from those of a small round-celled sarcoma (*see* fig. 2), which I am informed by my colleague, Mr. Treacher Collins, is the usual appearance of this class of growth. Extensive areas of cell degeneration form a marked feature, and this again is said by Mr. Collins to be characteristic. Although resembling a sarcoma in its histology, Mr. Collins states that the growth is of epithelial origin and should be termed a neuro-epithelioma.

Mr. Frank Taylor, whose study of the literature of these cases formed his contribution to the paper read by Mr. Fleming at the Section of Ophthalmology, quotes Wintersteiner's[1] statistics based on

[1] Wintersteiner, "Das Neuro-Epithelioma Retinae," Leipz. u. Wien, 1897.

497 cases of glioma of the retina. As regards metastases in this series, those in distant organs are as follows: In skeletal bones, nine; liver, seven; ovary, two; kidney, two; lungs, one; spleen, one. These statistics appeared twenty years ago, and, according to Taylor, no other ovarian metastasis has since been recorded in cases of glioma of the retina.

I have myself looked up the literature on the subject, and can find only two cases of metastatic glioma of the ovary, these being the two to which Taylor referred. One was recorded by Heymann and Fiedler,[1] in 1869. The patient was a child, aged 3. The primary disease was unilateral, affecting only the left eye. The tumour was the size of a hen's egg. Four months after the onset of the disease the bulb was extirpated and the optic nerve resected. Recurrence followed two weeks later and death occurred two months after operation. Metastases were situated on the cranium and meninges, underneath the optic chiasma, in the *left ovary* and in the retroperitoneal glands. The age of this patient and distribution of the metastases in the case are practically identical with what has been stated above in reference to the case I now exhibit. The second case was published by Rusconi and Bizzozzero in 1871.[2] The patient was a child aged $1\frac{1}{2}$, who died two months after the growth was first noticed; no operation was performed. There was exophthalmos due to a *glioma retinæ*. Metastases were found between the cranial bones and the dura mater, in the liver, and in *both ovaries*.

Histological Details of Neuro-epithelioma.—The cells composing the growth are for the most part small (8 to 9 μ) and variable in shape, but round and oval cells predominate. They possess a large nucleus (6 to 7 μ) which monopolizes the greater part of the cell, the remaining protoplasm forming only a thin ring around the nucleus. Often the extranuclear cytoplasm is too scanty to be seen even under $\frac{1}{6}$ in. objective, and the nuclei appear to represent the entire cell (the "free nuclei" of Virchow and others). Here and there, larger cells with abundant cytoplasm are seen. These are commoner in metastases than in the primary retinal growth. These larger cells may be angular or fusiform

[1] "Ein Fall von Netzhautgliom mit zahlreichen Metastasen," *Arch. f. Ophthal.*, 1869, xv, No. 2, p. 173.

[2] Rusconi, "Caso di glioma della retina con nodi secondari nel fegato, nel reni negli ovari," *Rendiconti del R. Istituto Lombardo* Serie II, iv, fasc. 5; also, *Rivista clinica di Bologna*, Giugno, 1871, p. 169. Bizzozzero, "Sulla sviluppo del glioma secondario del fegato." *Giorn. dell' Accad. di Med. di Torino*, 10 Maggio, 1871.

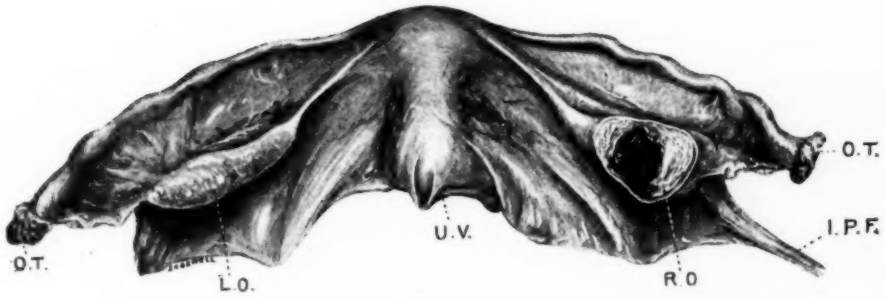


FIG. 1.

Secondary neuro-epithelioma of right ovary in a child, aged 3.

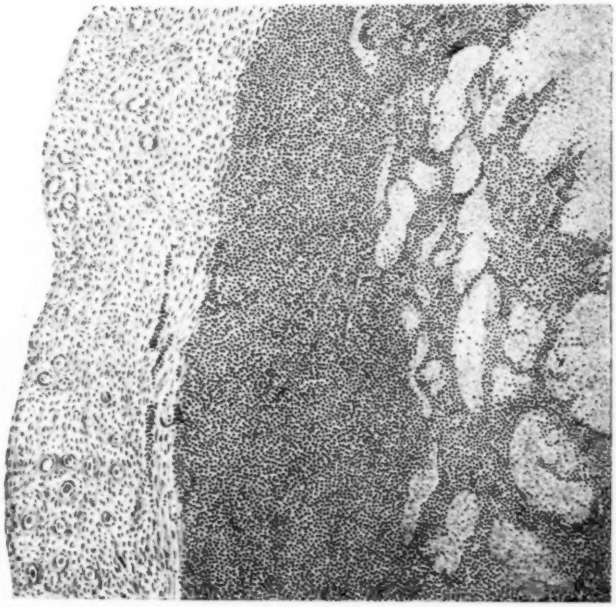


FIG. 2.

Secondary neuro-epithelioma of right ovary in a child, aged 3. (× 105.)

in shape and give off processes, they may also contain more than one nucleus. The intercellular substance is extremely scanty and is scarcely visible except in places where the delicate capillary channels are distended with blood-corpuscles. In the ovarian deposit these capillaries are very numerous and are often widely distended with blood. Their walls are composed of a single layer of flattened cells, which easily give way, leading to disruption of the growth by extensive interstitial hæmorrhages. As already stated cell degeneration is a marked feature of the growth. Fat droplets appear in the cytoplasm of the cells, and large masses of growth may undergo caseation and total necrosis, so that all nuclear staining of the cells is lost, and the vascular system of the tumour, sharing in the process, shows thrombosis and degeneration of blood-vessels.

(*March* 1, 1917.)

Curious Degeneration of a Cervical Tumour.

By C. Hubert Roberts, M.D.

The specimen exhibited was removed by the author from a patient, aged 35, at the Samaritan Free Hospital for Women on October 12, 1916. The patient had one child, aged 3, and states that since this confinement she has never been really well. She said that her chief trouble was extreme pain in the abdomen, mostly in the left side, and that for the last six months she had suffered with flooding and discharge. For two months before admission she had noticed a lump in the abdomen which was very painful and was rapidly getting larger. Other symptoms were great difficulty with micturition and sometimes retention of urine. On examination of the abdomen a semi-elastic swelling was found rising out of the pelvis to the level of the umbilicus. At its upper border there was a hard mass, (?) the fundus uteri, or a harder portion of the tumour. The whole mass was very tender to touch.

On vaginal examination the following curious condition was found. There was a mass in the roof of the vagina distending and protruding from a cervix which, in midwifery terms, might be designated as "fully dilated," in fact at one time this presenting mass was taken for the bulging membranes of an ovum in the process of extrusion.

On closer examination, however, the mass was found to be really in the cervix itself, the remainder of the cervix proper being thinned out and distended round the tumour. A sound passed 7½ in. laterally, and the hard mass at the summit of the abdominal tumour was then known to be the fundus uteri. Vaginal examination with a speculum found a soft bulging mass, dark bluish-red in colour, which distended the cervix, and which again could easily have been mistaken for the membranes of pregnancy. Finally the diagnosis of cervical fibroid was made and the tumour removed by panhysterectomy.

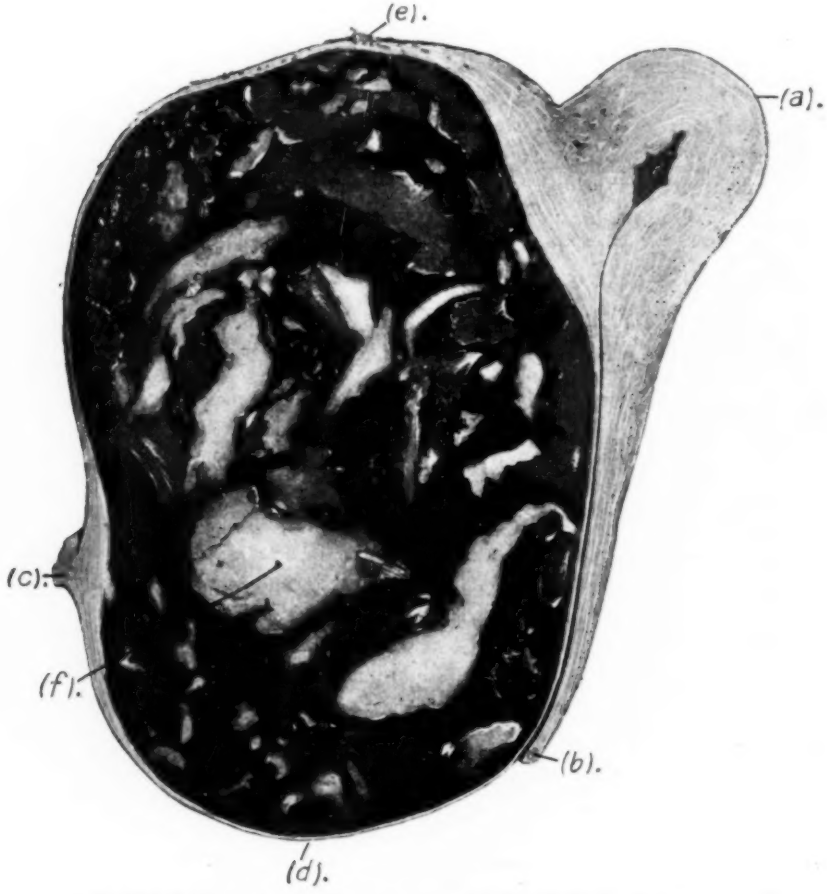
The operation was a difficult one as the cystic mass practically had to be enucleated. During the operation the tumour was accidentally punctured and a large amount of dark red débris and fluid escaped. The bladder relations were very difficult, but after stripping it down forwards I was able to reach the anterior vaginal fornix and open it, and after that the remaining stages of the operation were fairly easy. The right ureter was clearly seen to be much dilated and lifted up on the side of the tumour; in fact it might easily have been damaged.

The patient recovered satisfactorily except for a slight rise of temperature on the fourth and sixth day after operation. So far she remains well, but the prognosis is doubtful in view of the microscopical examination. She was carefully examined on February 28, 1917, when her condition was quite satisfactory.

The specimen exhibited is now only half its original size, as much of its contents were lost during its removal. It has been carefully hardened, and on section shows the curious appearances found in the accompanying picture (by Mr. Shiells). It resembles a cervical fibroid of the anterior lip, presenting the usual appearances of such tumours, but has undergone the most remarkable degeneration. The contents show the whole tumour to be necrotic, and of a dark bluish-red colour, with islands of "fibroid" tissue more or less well preserved in places. At the time of removal the contents were of course fluid. One of the striking features of this case is the extraordinary protrusion of the cystic mass into the dilated cervix, which measured 3 in. across. This might well justify such a case being taken for an ovum in the process of being expelled.

Microscopical Examination.—Sections of the islands of tissue from the centre of the tumour show marked degeneration everywhere, combined with hæmorrhage. The cellular elements of the tumour, however, do not resemble those of an ordinary fibroid of the cervix. There

is practically very little fibrous tissue present, the cells seen under high power are large and almost uniform in type amid an embryonic stroma, and are very suggestive of sarcoma. Many of the cells are clustered around blood-vessels, some of which are very thin walled, others, however, are thick and like those met with in fibroid tumours. The protoplasmic elements of the cells are too degenerate to determine the condition of the nuclei. Some areas of the tumour show cells apparently developing from the outer coats of minute



(*a*), Fundus uteri; (*b*), posterior lip of cervix; (*c*), anterior lip of cervix; (*d*), bulging mass into dilated cervix; (*e*), site of attachment of bladder; (*f*), island of semi-necrotic tissue (from which the sections were taken) in centre of degenerating tumour.

capillaries (or lymphatics), and yet the condition is not exactly like a perithelioma.

I should very much like to hear the opinion of members of the Section as to the nature of the tumour. The question arises: Is the tumour a sarcoma or a degenerate fibroid? If it be a sarcoma it is not typical on microscopical examination; if it be a fibroid the degeneration is unusual. Lastly, could it be described as sarcomatous degeneration of a fibroid?

Dr. Lockyer has very kindly examined the sections and believes it to be a sarcoma.

DISCUSSION.

Dr. H. R. SPENCER: I have removed two cystic cervical fibroids of about the same size as that shown by enucleation by the vagina, a very simple and safe operation. In both the fluid coagulated after removal. I think the specimen shown is a degenerated myoma, but the microscopic appearances are curious.

Dr. ROBERTS (in reply): I am quite agreeable that the specimen should be referred to the Pathology Committee. As regards the question of vaginal operation in this case I think that, considering the size of the tumour, its degeneration and the extent of uterine tissue involved, the dangers of sepsis and hæmorrhage would have been very great; and, further, that if the uterus had been saved it would have been quite useless as a reproductive organ. In view of the nature of the microscopical sections also, I feel certain that panhysterectomy was fully justified.

N.B.—The Pathology Committee have since reported on the sections and are of opinion that the tumour is sarcomatous (*see* p. 134).

(*March* 1, 1917.)

A Calcified Fibroid which caused Complete Axial Rotation of the Uterus.

By C. HUBERT ROBERTS, M.D.

THE specimen shown was removed at the Samaritan Hospital for Women from a patient aged 55, married; no pregnancies. The periods had ceased for ten years, but previously they had been very profuse.

The patient stated that she knew she had a tumour in the abdomen "for many years" but that it had given her very little trouble. About

three months before admission she began to suffer with acute attacks of abdominal pain, which of late had been very severe and accompanied with cramps and sickness; her general condition was good. On examination a large hard swelling could be easily felt in the abdomen, reaching to the level of the umbilicus, more marked on the left side than the right. It was very mobile and could be freely pushed about the abdomen.

On vaginal examination there was a small atrophic cervix; movements of the abdominal mass communicated themselves to the cervix. The fundus could not be determined. The diagnosis was made of a calcified fibroid, ovarian dermoid, or ovarian fibroma. There was no free fluid.

On November 16, 1916, the tumour was removed by abdominal section. It was easily lifted out of the abdominal cavity and was found to be densely hard and very heavy. The remarkable feature of the tumour was its pedicle, the length of which was 8½ in., with the thickness of that of two fingers. It consisted of an enormously elongated senile uterus which, at the level of the supravaginal cervix, had undergone an acute axial twist, two and a half times, the twist being from left to right, forwards. The appendages were included firmly in the twist. The torsion was easily undone and the tumour simply amputated from the fundus uteri, to which it was attached by a pedicle 1½ in. wide. The vessels below the twist were deeply engorged, but there were no adhesions anywhere and no free fluid. The senile uterus and appendages were left *in situ*.

The patient recovered completely, the temperature being normal throughout her stay in hospital.

The tumour exhibited is a well marked example of complete calcareous degeneration of a fibroid; when fresh it weighed 4½ lb. It was so hard that it took a considerable time to cut through, and spoilt two saws in the process.

The importance of the specimen lies in its causing axial rotation of the *uterus*. Axial rotation of the pedicle of a stalked fibroid is not uncommon and may lead to acute degeneration of the tumour, but complete torsion of the uterus itself is very rare. I have not met with a case in my own practice before. Most of the cases reported of axial rotation of the uterus by a fibroid have been only partial—i.e., say half a twist—possibly leading to acute abdominal pain, accumulation of blood or pus in the uterus, or necrosis of the tumour. In my case the twist was two and a half times, but as the uterus was very senile

and the pedicle very thin from long traction, such twisting was easily possible, just as occurs in pedunculated ovarian cysts.

Dr. Cuthbert Lockyer kindly permits me to show the picture exhibited.[1] It was drawn from my personal description of the case and accurately portrays the condition found at the time of operation.

(*March* 1, 1917.)

Case of Supravaginal Amputation of Uterus for Sarcoma mistaken for Myoma.

By G. Blacker, M.D.

The patient, a single woman, a nurse, Miss H. S., was admitted to University College Hospital on April 26, 1907, complaining of swelling of the abdomen and some œdema of the legs. The periods, which had never been excessive, lasting five to six days, and recurring every four weeks, had ceased six years previously at the age of 38. There was no abdominal pain nor difficulty in micturition and no vaginal discharge. The temperature was slightly elevated, reaching 101° F. at night.

Palpation of the abdomen revealed the presence of a mass reaching up to 1 in. above the level of the umbilicus. It was smooth on the surface, dull on percussion, and over a considerable area on the left side where the wall felt thin it fluctuated and gave a fluid thrill. On vaginal examination the cervix was small and healthy, and in Douglas's pouch there was a mass continuous with that felt in the abdomen and indistinguishable from the uterus. The sound passed the normal distance; there was no thickening of the cellular tissue. The ankles were somewhat œdematous. The diagnosis was thought to lie between a semisolid ovarian cyst adherent to the uterus and a subserous fibrocystic tumour of the uterus.

On May 1 the abdomen was opened and the tumour found to be uterine in origin. The abdominal cavity was packed off, and to facilitate removal the cystic portion of the tumour was tapped, over 2 pints of thick grumous fluid being evacuated. Supravaginal amputation of the uterus was then performed in the usual way, both ovaries being removed and the abdomen closed without drainage, a continuous

[1] The illustration will be found in "Fibroids and Allied Tumours," Macmillan and Co., 1917.

silk suture being used for the peritoneum, interrupted silk sutures for the fascia, and a subcuticular catgut suture for the skin.

The patient made a good recovery except that on one occasion she passed some bright red blood from the vagina. She was quite well when last heard of in May, 1911, four years after the operation.

The specimen shown consists of a slice of the right half of the uterus, together with the right ovary and a portion of the tumour. The ovary and tube are atrophied in appearance, but otherwise normal. On one surface of the slice is seen the uterine wall, measuring about 2 cm. in thickness, with the uterine cavity cut across and containing mucus. In the anterior wall is a small fibromyoma, 2 cm. by 1·5 cm. Growing from the posterior wall of the uterus is a growth waxy in section and breaking down on the surface. The greater part of the growth is occupied by an irregular cavity and posteriorly the wall is very thin.

On microscopic examination the endometrium is found to contain some distended glands and is infiltrated with small round cells. The fibromyoma presents no sign of any sarcomatous change, but is undergoing hyaline degeneration. The section of the tumour shows that it is made up mainly of round and spindle cells undergoing degenerative changes, and that there are present a large number of giant cells with deeply staining nuclei. Scattered throughout the growth are considerable numbers of leucocytes.

Sections of the cervix show perfectly normal structure.

(*March* 1, 1917.)

Two Cases of Supravaginal Amputation of the Uterus for Supposed Myoma found to be Sarcoma.

By Herbert R. Spencer, M.D.

In this short communication I wish to place on record two cases of supravaginal amputation of the uterus for sarcoma, mistaken for myoma. The operations were performed seventeen and twenty years ago. It was these two mistaken diagnoses (which I think could not have been avoided without a microscopic examination) which led me to study the literature and to consider the relative advantages of amputation and total hysterectomy, and to decide very strongly in favour

of the total operation, which accordingly I have employed in every case of abdominal hysterectomy for myoma, sarcoma or carcinoma which I have performed during the last sixteen years.

No one will deny that for sarcoma of the uterus total hysterectomy is better than amputation, nor that removal of the uterus whole is better than removal of the body and cervix separately; and no one of experience will deny that cases of myosarcoma occur which cannot be distinguished from myoma at the time of, or immediately after, the operation, but only after careful hardening and microscopic examination of the tumour, sometimes in many places.

These two cases of sarcoma occurred amongst thirty cases of supravaginal amputation for "myoma" (i.e., in 6·6 per cent.), a frequency which is somewhat greater than that generally met with. But sarcoma of the uterus, though rare, is much commoner than is thought by those who do not harden and examine all tumours removed. I have met with more than a dozen other cases. But these two cases now published are the only cases in which I have performed the operation of supravaginal amputation, and they point to one of the disadvantages of the partial operation and one of the points of superiority of the total operation to which I have called attention on many occasions, and which I will endeavour to illustrate in further detail in subsequent papers, one of which is now in the hands of the secretaries of the Section.

Case I.—A. I., aged 50 (children four, abortions nil), was admitted to University College Hospital on March 26, 1896, complaining of swelling of the abdomen, occasional attacks of pain and hæmorrhage. The enlargement of the abdomen was first noticed nine months ago: there was no pain at the time. About six weeks before admission the patient was seized with sudden severe pain across the abdomen, which had continued since and lately had shot down the left leg to the foot; severe pain had also been present in the back. Since the attack of pain the patient had been constipated and a discharge of blood had been present, profuse for the first month, but very slight during the last fortnight. The patient had previously had no severe illness; there was no sign of syphilis; her last child was born nine years ago, and since that time she had not been pregnant. There was no history of cancer or tubercle in the family. Menstruation began at the age of 15, was always regular, lasting seven days and requiring nine to ten diapers, and was very painful. Menstruation ceased two years ago. On admission the patient was pale, fairly well nourished. There was no œdema of the legs, in which venules were visible. The temperature was 100° F., the pulse 140, respiration 24. The heart and lungs appeared healthy. In the right half of the abdomen and slightly passing across the middle line, a well defined tumour was found which reached up to a height of $8\frac{1}{2}$ in. above the pubes and measured $7\frac{3}{4}$ in. transversely. It did not

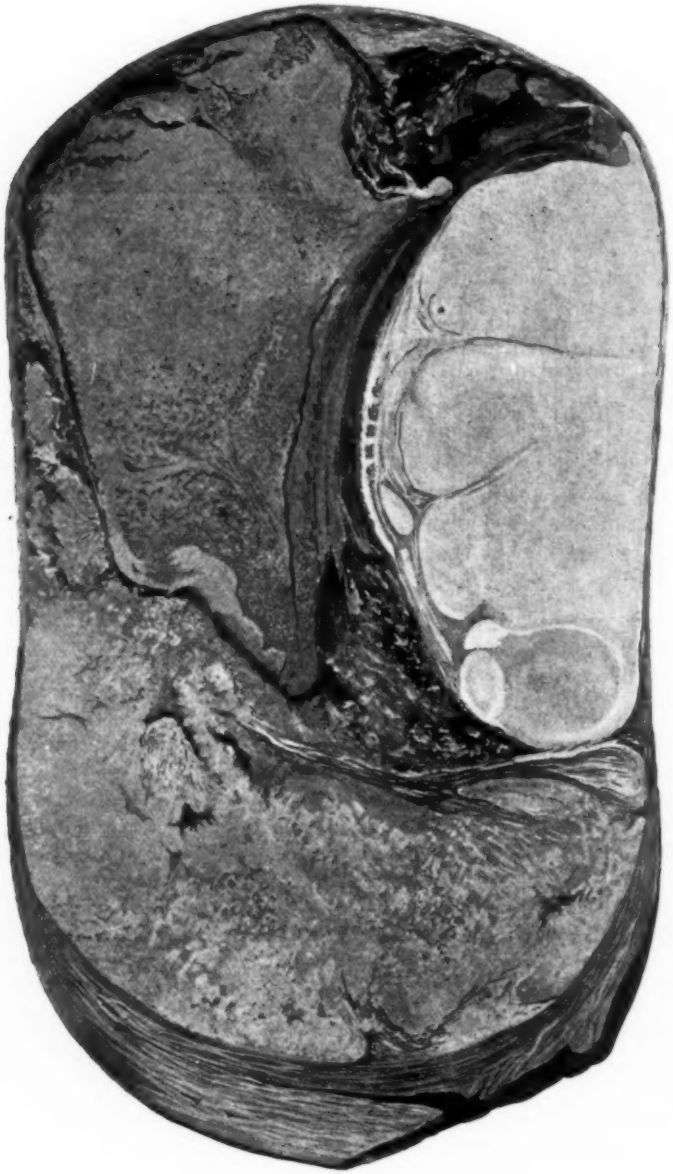


FIG. 1.

Sarcoma of the uterus (Case I), natural size.

move on respiration. The veins in the abdominal wall were enlarged; the umbilicus protruded. The tumour felt like a tense cyst, giving the signs of dullness, fluctuation and thrill. The cervix was normal: the sound was not passed. The tumour appeared to be separate from the uterus and was diagnosed as a tense ovarian cyst. At the operation on March 30, 1896, a smooth, apparently cystic, tumour free from adhesions was found growing

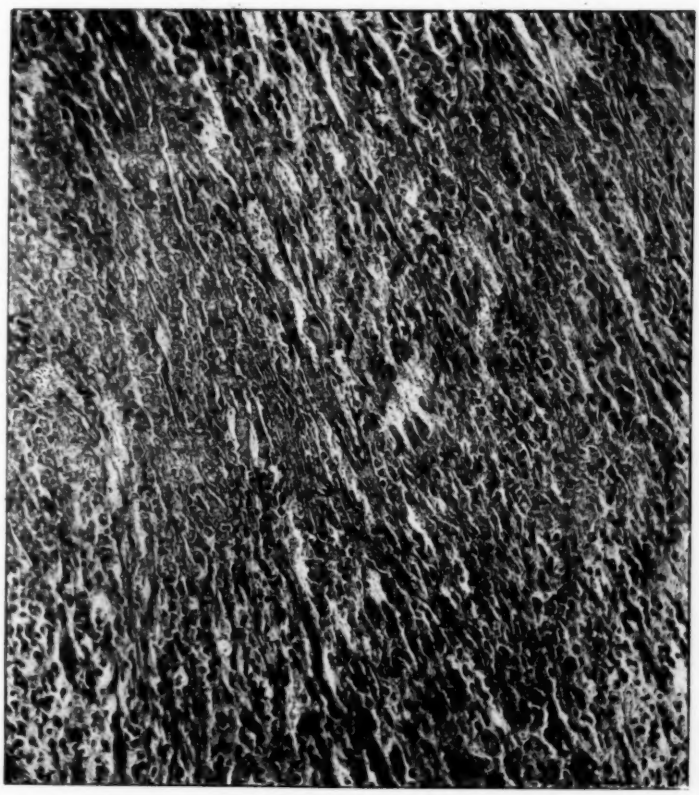


FIG. 2.

Photomicrograph of sarcoma of uterus (Case I) showing spindle cells and round cells. Some of the cells and nuclei are very large and stain deeply. In some sections the transformation of muscle-cells into sarcoma-cells can be seen.

from the fundus uteri: it was thought to be a fibroid: large veins coursed over its surface and were also seen in the broad ligaments. Supravaginal amputation was performed with subperitoneal treatment of the stump. The ovaries, which

appeared healthy, were left behind. The wound healed by first intention, and the patient left the hospital on April 22, 1896. At the operation and for some weeks afterwards, while the tumour was being hardened, no suspicion of malignancy arose. Recurrence took place in the lungs (diagnosed in 1897) and in the pelvis and hip (diagnosed in 1898), and the patient died on August 18, 1899.

The uterus removed weighed 5 lb. 15 oz., and measured 19 cm. by 19 cm. by 11·5 cm. The section (*see* fig. 1) showed that the whole of the body of the uterus was occupied by a tumour which extended nearly to the peritoneum. There was a distinct capsule to the tumour, well seen below. The upper part of the tumour, however, was continuous with the uterine wall. The lower part of the tumour was slightly granular and at the same time showed indications of a fibrous reticulum. The upper half of the tumour was of waxy homogeneous appearance. The peritoneum was smooth and showed no growth. The uterus had been divided just above the internal os, exposing the uterine mucosa, which was smooth and bulged slightly, as it would from the presence of a submucous fibromyoma.

Microscopic Structure.—The tumour is a mixed-cell sarcoma, evidently arising in a fibromyoma, for at the periphery of the tumour the general appearance is that of the fasciculated arrangement of a fibromyoma, the muscle cells of which are large, with an occasional very large cell. Towards the central part of the tumour the structure is that of a typical large round and spindle-cell sarcoma (*see* fig. 2).

Case II.—E. F., a virgin, aged 48, was sent to me by Sir Thomas Barlow, on March 12, 1899, complaining of distension of the abdomen for some years and excessive loss for ten months. Menstruation, usually lasting for a week, had been profuse, of three weeks' duration, for the last six months, but was attended with little pain. At times there had been a slight discharge, not hæmorrhagic. The patient was thin and had a sallow complexion, but her friends had not noticed any change in these respects, nor had the patient noticed any increase in the abdomen of late. She was quite free from pain and, except for the sallow complexion, she seemed in good health. In the abdomen was a softish tumour, of elongated oval shape, which reached up for 5½ in. above the pubes, 1 in. above the umbilicus. It was regular in outline and was diagnosed as a myoma. No souffle could be heard over the tumour. The cervix was healthy. There was no sign of fluctuation. On June 12, 1899, I saw her again. She had been taking ergot, which at first diminished the loss, but the last period was again profuse. She was not thinner and the legs were not swollen. As she seemed to be suffering from toxæmia, due to what was supposed to be a degenerated myoma, I advised removal of the uterus by abdominal section. Two days before the operation I found slight pleuritic effusion on the right side. Sir Thomas Barlow did not think this would interfere with the operation, and accordingly I amputated the uterus by the supravaginal operation with subperitoneal treatment of the cervical stump, on



FIG. 3.

Sarcoma of the uterus (Case II), natural size. The uterus has been amputated through the unaffected cervix. During the process of hardening, part of the soft growth has been extruded into the os; but the cut surface of the cervix was quite normal in appearance in the fresh state. The tumour (which, unopened, resembled a myoma, and contains a small, subperitoneal myoma of normal structure on its posterior wall) is seen, on section, to contain masses of sarcoma separated by fibromuscular septa.

July 13, 1899. The patient made a good recovery and left the nursing home in a month apparently well, but still having a little effusion at the right base. Some months later a large amount of blood-stained fluid was removed from this side by Mr. Bilton Pollard, and it was evident that there was malignant disease of the pleura and lung. In February, 1900, the patient was found to be

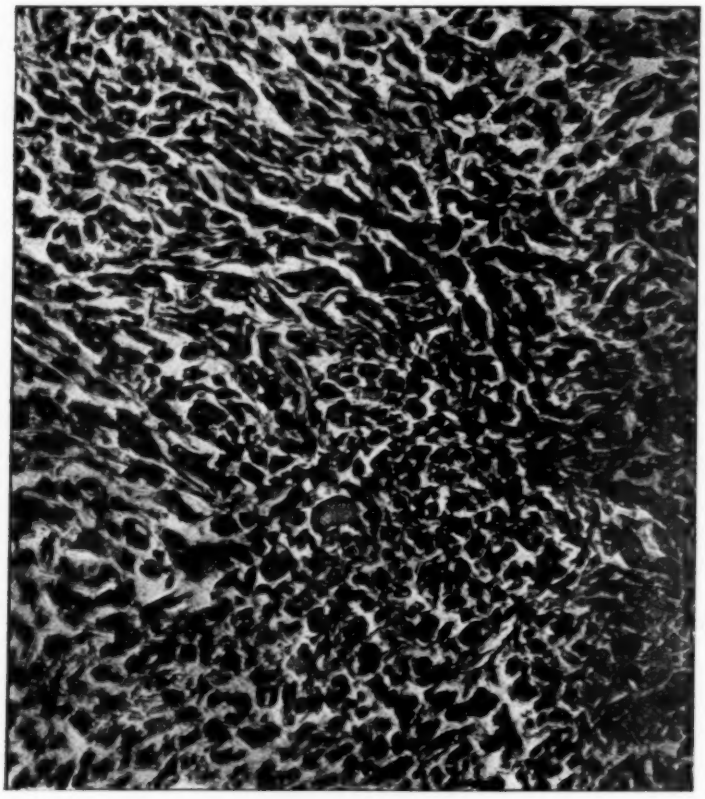


FIG. 4.

Photomicrograph of sarcoma of uterus (Case II) showing large spindle cells and large round cells, with large and deeply staining nuclei. In other sections the transformation of muscle-cells into sarcoma-cells can be seen.

very ill with growth in the lung, liver, vagina and vulva, from which she died in the course of the year. At the operation and up to the time of her leaving the home, no suspicion had arisen that the tumour was malignant. It was

kept to harden, and when the bloody fluid was obtained from the pleura the tumour was examined and found to be a sarcoma.

The uterus weighed 3 lb. 12 oz., and measured 15 cm. by 14 cm. by 11 cm. The peritoneum was smooth. The uterus had been amputated at the lower segment, exposing the lower end of a submucous and intramural tumour which occupied the whole of the body. On section (*see* fig. 3) the tumour was surrounded by the uterine wall which varied from 2 mm. to 1 cm. in thickness. The margin was a little irregular, particularly above, where at one spot it had penetrated nearly to the peritoneum. The tumour was divided up by fibrous strands like those met with in a fibromyoma. The central part of the tumour had a somewhat hyaline appearance; the rest was rather granular and in it smooth-walled cysts and numerous orifices of vessels were seen. On the surface of the posterior wall was a subperitoneal tumour having the usual appearance, and showing microscopically the structure of a fibromyoma.

Microscopic Structure of the Main Tumour.—A section was made through a fibroid strand in the tumour and the surrounding growth. The fibroid part shows fibromyomatous tissue, some parts of which do not differ very much from ordinary fibromyoma; but other parts show the muscle cells and their nuclei increased in size and less fasciculated in arrangement. Scattered among the muscle-fibres are small round cells. On the edge of the non-fibroid portion of the growth the cells become very large, some being giant cells and having an irregular arrangement, some are round and a few leucocytes are seen scattered through the tissue (*see* fig. 4). It is a mixed-cell sarcoma arising from the transformation of the cells of a fibromyoma. The secondary growth in the vagina was of similar structure, but the cells were mostly spindle-shaped.

(*March* 1, 1917.)

Four Cases of Undiagnosed Cancer of the Cervix in 200 Total Hysterectomies for Myoma.

By Herbert R. Spencer, M.D.

Amongst 200[1] total abdominal hysterectomies which I have performed for myoma I have met with four cases of undiagnosed carcinoma of the cervix, without taking into account a number of cases in which hysterectomies for carcinoma of the cervix were complicated with myoma. In two of the four cases the carcinoma was unsuspected before operation; in two it was suspected on account of the bleeding on examination, but was not diagnosed owing, in one case, to the os being out of reach (large pelvic subperitoneal cervical myoma), and, in the other case, to the portio being uninvolved and the cervix so large that it was thought possibly to contain a myoma. The results of these four operations are as follows:—

The first patient, who suffered from heart disease, died suddenly within a year of the operation of syncope when going up stairs. As the cancer was in a very early stage there can be little doubt that, had she survived, she would have remained free from recurrence.

The second case remains free from recurrence after six years.

The third case remained well for five years, when a recurrence was detected of which the patient died six years all but three weeks after the hysterectomy.

The fourth case, in which the cancer was very extensive, has already a recurrence.

I submit these four cases to the consideration of those gynæcologists who still continue to amputate the uterus for myoma, leaving the cervix, instead of performing total abdominal hysterectomy, an operation which in my opinion is in every important respect (except that it takes a few minutes longer to perform) superior to the supravaginal operation, and which accordingly I have performed in every case of abdominal

[1] Within one or two on either side, the exact figure being as yet impossible to give owing to the difficulty of deciding whether one or two cases complicated with other tumours should be called hysterectomy for *myoma*.

hysterectomy for myoma and have persistently advocated during the last sixteen years.

Its value in removing cancer unrecognized at the time of operation is illustrated in these cases in which cancer of the cervix was found present in no less than 2 per cent. Its value in preventing the subsequent development of cancer in the cervix left behind is shown by the large number of cases already reported in which cancer developed in the cervix which was apparently healthy and in some cases certainly healthy at the time of operation. I have seen one such case in a hopeless condition from cancer of the cervical stump and surrounding parts six years after a supravaginal amputation by a surgeon for myoma: the patient had been free from hæmorrhage and discharge for more than three years after the operation.

In making this small contribution to the subject of hysterectomy I appeal to those who prefer total hysterectomy—a small but increasing number—to examine the cervix in all cases macroscopically and, if suspicious, microscopically, with the object of obtaining facts bearing out this important question and of increasing our knowledge of early carcinoma of the cervix; and I appeal to the advocates of amputation to publish all the cases known to them of cancer of the cervical stump at or following the amputation, and not to bemuse themselves with supposed disadvantages of total hysterectomy which do not exist, nor with the supposed rarity of cancer of the stump, while refraining from producing the only evidence on their side of the question which is of equal value—viz., the after-history of a consecutive series of one or two hundred cases of amputation for myoma of which every case has been followed up *and examined* five years after the operation.

Case I.—M. C., aged 44 (who had had no child but five miscarriages, the last thirteen years ago), was admitted to University College Hospital on October 14, 1905, complaining of a tumour, great abdominal pain, and almost constant losses of blood during the last seven months, the longest period of freedom from bleeding being one week. The tumour had been noticed for more than a year and was increasing. Menstruation began at the age of 14, was regular till this illness, lasted four to five days, and was attended by pain in the back during, and for three or four days after, the flow. A watery discharge had been present for eleven months. Micturition was painful and frequent (four or five times at night); the bowels were confined. On admission the patient was fat, anæmic and cyanosed, the pulse 120, intermittent; respiration 44. There was a feeling of faintness; the heart was dilated, the first sound was blurred and a systolic murmur was sometimes audible at the apex. The temperature was slightly raised (up to 101·8° F.), and only fell to normal

on two occasions while the patient was in the hospital. The girth of the abdomen was 44 in. at the level of 4 in. below the umbilicus, and the abdomen was distended by an enormous globular tumour reaching up to 11 in. above the pubes. The portio was healthy, the os small and round. The tumour was diagnosed as a degenerated myoma. No bleeding followed the examination. For several weeks the patient had to be propped up in bed on account of the condition of the heart. My colleague, Dr. Sidney Martin, kindly saw her and prescribed digitalis and nux vomica. Her general condition improved a good deal, so that, although she was still cyanosed and the pulse slightly intermittent, it was possible on December 1, 1905, to perform total abdominal hysterectomy by Doyen's method. The wound healed by first intention and the patient left the hospital, having lost the cyanotic appearance and in good



FIG. 1. (Case I.)

The cervix and part of the body of the uterus (which was over 10 in. in diameter, and weighed 10½ lb.) with intramural myoma. Two thrombosed vessels are seen in the myoma. In the upper half of the anterior wall of the cervix is seen a small growth striated towards the canal (papillary) with a few minute cystic spaces below it (dilated glands). This growth is a squamous-cell carcinoma. The portio is short, smooth, and perfectly healthy. The stratified epithelium extends up the canal as far as the growth (*see* fig. 2).

general condition, on January 6, 1906. She continued to improve and remained well for some months with occasional attacks of syncope, from one of which she died suddenly when going upstairs within twelve months of the operation.

The uterus weighed in the fresh state 10½ lb. (preserved, 9 lb. 10 oz.), and

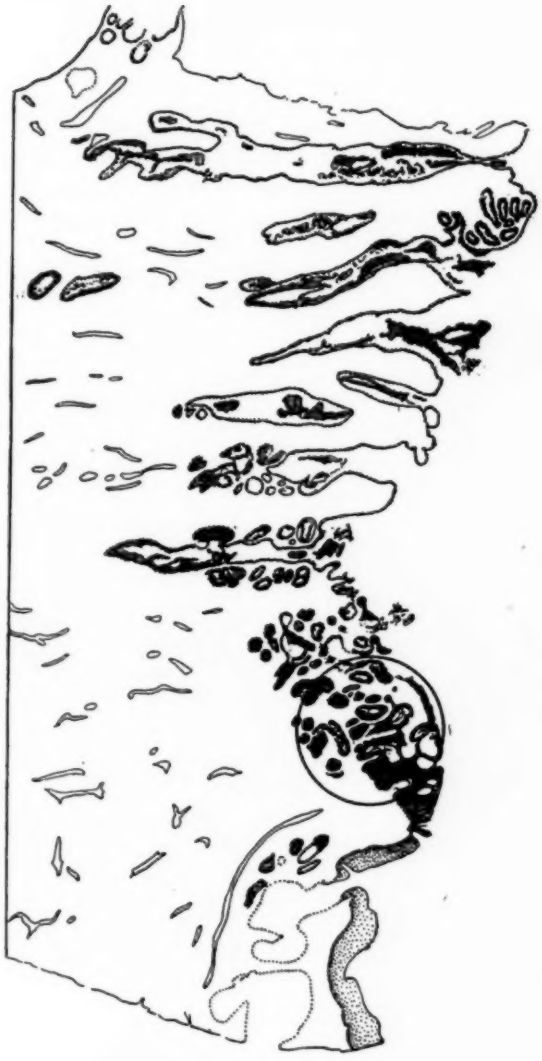


FIG. 2. (Case I.)

Camera lucida tracing of microscopic section of the papillary growth in cervix (low power). The black areas show squamous epithelium proliferated, and in some places desquamated, on the papillary processes, and invading the cervical tissues—squamous-cell carcinoma. The part within the circle is drawn under high power in fig. 3. At the bottom of the section is seen a strip of healthy stratified epithelium situated near the middle of the cervical canal.

formed a globular mass 10 in. in diameter (22·5 cm. by 23 cm. by 21 cm.) with two subperitoneal tumours on the top, one as large as a walnut, the other of the size of a filbert. The uterus was enlarged by a single intramural tumour growing in the anterior wall, measuring 19 cm. by 13 cm. on section, which had raised the peritoneum anteriorly for a height of 10 to 15 cm. above the external os. The tumour was slightly degenerated towards the periphery at



FIG. 3. (Case I.)

A microscopic section (high power) of the part within the circle in fig. 2, showing squamous-cell carcinoma and round-cell infiltration.

one spot and contained in the middle of the lower half three large vessels filled with clot, the largest of which measured 2·5 cm. by 0·5 cm. The portio vaginalis was healthy, the os round. The cervix was 3 cm. long. The canal

of the body was, in the sagittal section, 8 cm. long, its mucosa extremely atrophied, being only visible in the upper ½ in., and there not more than 0·5 mm. in thickness. The mucosa of the cervix was normal in the posterior wall, but was curiously striated and cystic on the anterior wall at its upper part, where it was 7 mm. thick (*see* fig. 1). This striated portion is a squamous-cell carcinoma. Below it the wall is lined with healthy stratified epithelium.

Microscopic Structure.—The mucosa of the body showed a single layer of flattened epithelium and no glands. The affected portion of the cervical mucosa showed stratified epithelium and cysts lined with columnar epithelium beneath it. The stratified epithelium goes down into the tissues and forms masses of squamous cells (typical squamous-cell carcinoma) from the centre of which the epithelium has fallen out (*see* fig. 3). The epithelium of some of the glands is also proliferated, forming large masses of epithelial cells. The papillary projections causing the striated appearance are very vascular and are covered with several or many layers of squamous epithelium which has desquamated in places (fig. 2).

This occurrence of a squamous-cell carcinoma high up in the cervical canal with outgrowth of proliferated squamous metaplastic epithelium on papillary processes and ingrowth into the tissues perhaps explains why some cases of carcinoma in the substance of the cervix have the appearance of squamous-cell carcinoma, although from the mode of growth we should expect them to be columnar-celled; of these, Case 4 is an example.

Case II.—Mrs. R., aged 43 (children three, abortions eight), was seen by me on October 31, 1910. Her last pregnancy occurred seven years ago, and she had suffered from dysmenorrhœa for eighteen months and from menorrhagia. Menstruation began at the age of 11 (the patient was born in Ceylon) and till lately was regular and lasted four days. There was no discharge and no bleeding between the periods or on coitus. The dysmenorrhœa was very severe and the patient sometimes woke up with a pain in the back, which passed through to the front, and was relieved by walking about. On examination the patient was in good general health. The uterus reached up for 3½ in. above the pubes. The cervix admitted the finger for ¾ in. and felt quite smooth and healthy. The uterus was enlarged by a myoma to the size of a uterus at three and a half months of pregnancy, and was fairly movable. The myoma appeared to be single and probably submucous. I proposed to remove it by enucleation through the cervix; but the patient and her husband, who was a doctor, did not want to run the risk of a recurrence. I therefore removed the uterus by total abdominal hysterectomy on November 4, 1910. The patient made a simple recovery and remains well in January, 1917.

The uterus measured 14 cm. by 9 cm. by 9 cm. and weighed 1 lb. 3½ oz. It contained a single myoma in the anterior wall of the size of a flattened

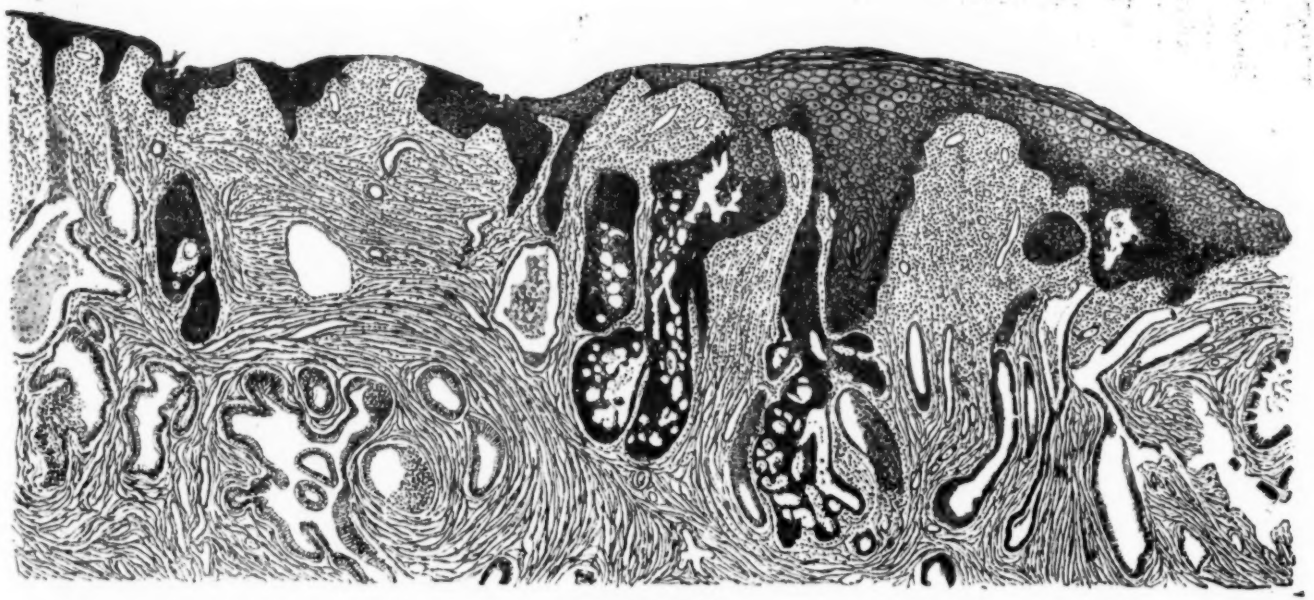


FIG. 4 (Case II).

Vertical section (low power) of raw area on portio in Case II, slightly reduced from drawing through *camera lucida* by Mr. Ford. The external os is just to right of figure. There is some glandular hyperplasia. The section shows a fairly even, slightly wavy surface. The superficial squamous cells have desquamated on the left. The deeper epithelium is dark in colour, and in several places is penetrating into the substance of the cervix. There is abundant round-cell infiltration. Several considerable masses of epithelial downgrowth are seen, showing vacuoles where the cells have fallen out. In one or two places the epithelial growth shows a tendency to invade the glands. The cells of the epithelium contrast distinctly with the normal epithelium (not seen in section). They are darker, and, under a high power, the nuclei are seen to contain more chromatin, the basal cells have entirely lost their regular shape and side-to-side arrangement, and the deepest cells and nuclei become elongated and radiate from the extremities of the columns diffusely into the tissues. This is the earliest stage of carcinoma of the cervix I have seen. The section exactly resembles, even to the invasion of the glands, one taken of the growing edge at the external os of the advanced growth in Case IV.

orange. The tumour was a good deal degenerated, being quite gelatinous at the periphery, but nowhere cystic. On the anterior lip of the portio (which was very short) there was a small raw red area not much bigger than a pea, slightly uneven, like a small erosion. As a matter of routine I had this cut for the microscope, though not suspecting cancer.

Microscopic Structure.—The raw area shows the early stage of a typical squamous-cell carcinoma with marked round-cell infiltration (*see* fig. 4).

Case III.—A. R., aged 44 (children three, one born dead), was admitted to University College Hospital on January 16, 1911, complaining of headaches for a month and of an abdominal tumour which had been noticed for two years. She had had coitus up to the time of admission, but this had never been followed by bleeding. The headache was attributed to a blow on the left frontal region six years ago, which caused temporary paralysis (and permanent weakness) of the right side, for which she was in the Hospital for Paralysis for fourteen weeks. The tumour caused no trouble but was getting larger. Menstruation began at the age of 15: at first was very irregular and lasted only two days; but after the birth of the first child, at the age of 24, it had been more regular, lasting five days and requiring twelve diapers. The patient had not menstruated for two years. Micturition had been frequent for the last year. On admission the patient's face was rather congested, the bridge of the nose sunk, the nails rather blue, the breasts flaccid. A tumour could be felt rising up for 8 in. above the pubes: it was situated almost entirely on the right side, and was fixed by the broad ligament. No souffle was heard over the tumour. The perineum was slightly torn, the cervix was in the middle of the pelvis, very far back, and bleeding on examination. Only the edge of the cervix could be touched, on account of the presence of a large fibroid behind and incorporated with it, which was part of the tumour felt in the abdomen. The diagnosis was myoma of the uterus with (possibly) cancer of the cervix, which was suspected on account of the hæmorrhage on examination, the os itself being out of reach. Total abdominal hysterectomy was performed on January 21, 1911. The tumour had invaded the right broad ligament extensively, and had to be enucleated before the uterus could be drawn up. Douglas's pouch was then opened and the cervix seized. It was brittle and appeared to be carcinomatous. The vagina was circumcised with scissors, but as the cut appeared to pass through growth, a further piece of vagina and of cellular tissue of the right broad ligament was removed with the cautery, which was also applied to the raw surface of the broad ligament. The peritoneum was closed by a purse-string suture and the abdominal wound was closed by silkworm gut through-and-through stitches and buried silk. The operation lasted ninety-one minutes. The patient was a good deal shocked. Two pints of saline fluid were injected intravenously. The wound healed by first intention. The patient made a good recovery, and remained well for over five years. She was in the hospital in January, 1906, for a broken tibia and fibula, caused by tripping over a mat. She felt quite well, but I took the

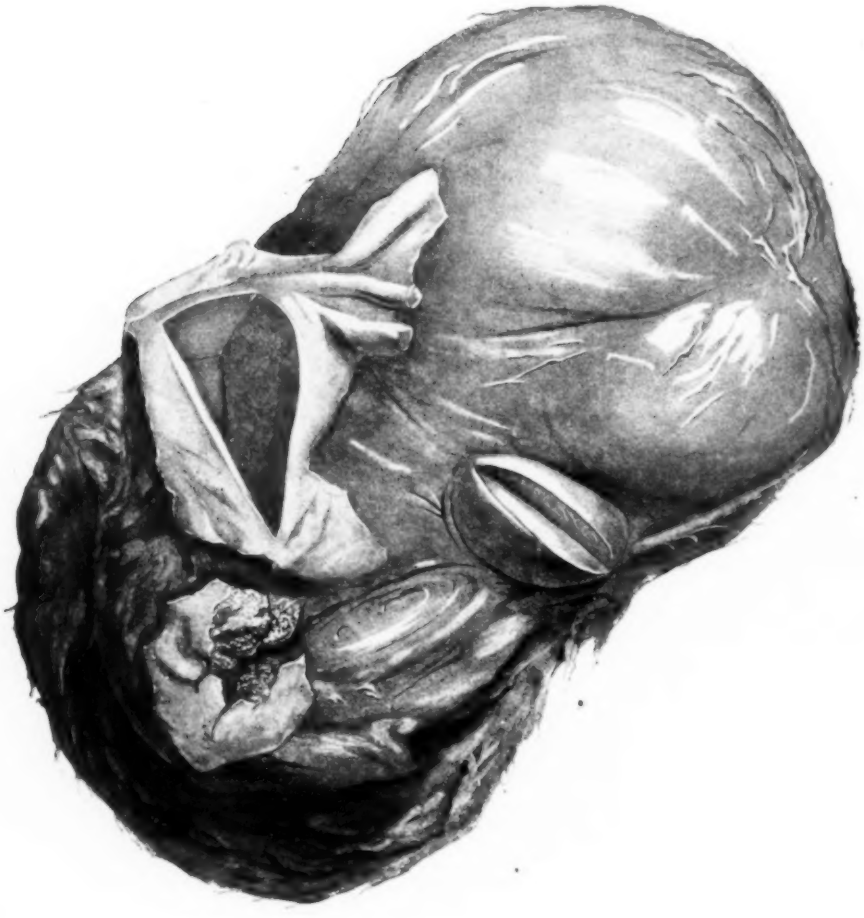


FIG. 5. (Case III.)

Uterus and cervical myoma, weighing 6 lb., seen from behind. The uterine body is free from growth. Part of the right appendages and the cut peritoneum are seen around it. The whole tumour is bare, enucleated from the right broad ligament. The lower pole blocked the pelvis, rendering the os inaccessible; it was enucleated from the cervical wall through the crescentic incision below. The portio has been opened posteriorly and shows cancer (squamous-cell carcinoma). (⅔ natural size.)

opportunity of examining her; she had some pain in the thighs and some thickening could be felt on vaginal examination. On February 22, 1906, this thickening was a well marked growth in the cellular tissue as big as a small apple. The tumour gradually enlarged and cachexia developed, and the patient died of recurrence on December 29, 1916, six years (all but three weeks) after the operation.

Description of Specimen.—A uterus with large cervical myoma and carcinoma of the cervix. The uterus measures 26 cm. by 15 cm. by 17 cm. and weighs 6 lb. The body of the uterus is not altered and its mucosa is normal. The appendages are missing, except part of the right tube; they were normal in appearance. The cervix is the seat of an intramural and subperitoneal myoma of irregular shape which has been enucleated from beneath the peritoneum and in front also from the cervix through an incision into its anterior wall. The muscular capsule has retracted towards the region of the external os, where it exists as a roll of tissue 3 cm. thick by 11 cm. across. On this is seen the external os cut open by an incision in its posterior wall, which is the seat of a friable carcinomatous growth extending nearly through the lip. The anterior lip is but little affected with cancer except on the right side. The external os is irregular (*see* fig. 5).

Microscopic Structure.—The large tumour is a myoma and the growth in the cervix is a squamous-cell carcinoma.

Case IV.—A. B., aged 44 (children three, abortions two), was admitted to University College Hospital on December 14, 1915. The last labour, fourteen years ago, was difficult, instruments being used and the patient has been tender ever since. She complained of flooding, which occurred three months ago at the time of the periods and was followed by pain in the knees and legs. Menstruation began at the age of 12, was regular every twenty-eight days, except at the age of 17 (when it stopped for a time) and was scanty, lasting three days. There had been a yellowish-white offensive discharge between the periods for two years. On admission the patient was rather thin and pale. A tumour could be felt reaching up to $5\frac{1}{2}$ in. above the pubes; it felt like a degenerated fibroid. On vaginal examination the patient bled freely. The cervix was very hard and enormously enlarged. The diagnosis was multiple myomata of the uterus and (possibly) cancer of the cervix. The suspicion of cancer was due to the hæmorrhage which followed examination, for the portio was smooth and greatly enlarged and, although the os was slightly open, considerable pressure did not show any brittleness at the external os. On December 18, 1915, the uterus and appendages were removed by total abdominal hysterectomy. In doing this a clip was put on the right ureter, mistaken for a vessel; the error was at once discovered and the forceps removed, but the pressure caused a leakage subsequently. The convalescence of the patient was interrupted by a little suppuration at the upper part of the wound and by leakage of urine *per vaginam*. By January 10 the wound had healed. There was a ureteral fistula at the top of the vagina when the patient left the

hospital on January 22, 1916. Later on there was clear evidence of recurrence in the pelvic cellular tissue.

Description of the Specimen (*see* fig. 6).—The uterus measures 14 cm. by 10 cm. by 8·5 cm. The body contains an intramural myoma 6·5 cm. by 5·2 cm. on section. The cervix is enormously distended (7 cm. by 6 cm.) and contains a carcinoma reaching up for 5·5 cm. above the external os but surrounded everywhere by at least 1 or 2 mm. of muscular tissue and at the portio by 3 mm. or more of unaffected tissue. The external os has a papillary erosion which does not appear to be cancerous. The right ovary is converted into a multilocular cyst, measuring 11 cm. by 8·5 cm. by 6·5 cm., from the inner surface of which depend a few pedunculated papillomata. One small papillomatous mass is attached to the outer surface. Both tubes are distended (hydrosalpinx). The left ovary appears to be somewhat fibrotic and is slightly enlarged and adherent to the tube.

Microscopic Structure.—The cervical growth is a carcinoma presenting the appearance of squamous carcinoma, with keratinization and round cell infiltration. The "erosion" on the cervix shows that the carcinoma has just reached the external os at that spot. The squamous epithelium at the edge of the os grows down into the tissues and shows that the growth is a true squamous-cell carcinoma. The warty growths in the ovarian cyst are simple papillomata.

DISCUSSION.

The PRESIDENT: The question whether total hysterectomy or subtotal hysterectomy is the best operation for the removal of fibromyomata of the uterus is one of great importance. With sarcomatous degeneration of such a tumour, however, the question is not so important as it is when a carcinoma develops in a fibroid uterus. As the specimen I have demonstrated this evening shows, the patient may remain free from recurrence, even when the body of the uterus has been removed and the cervix left, in the case of a sarcoma mistaken for a myoma. So far as I know the cases recorded in the literature show that the chances of recurrence are equally great whether the cervix is retained or not. I should like to know if the recurrence involved the cervix in the two cases recorded by Dr. Spencer. The possibility of the presence at the time of the operation, or the subsequent occurrence, of a carcinoma of the cervix, is a very important matter when subtotal hysterectomy is practised. With regard to the four cases of carcinoma brought forward by Dr. Spencer I think that two at any rate should not have been included, as the presence of the carcinoma was strongly suspected, although it was not proved, before operation, and I imagine that even the most ardent advocate of subtotal hysterectomy certainly would have performed a total hysterectomy in these cases. Personally I should never practise the partial operation in any case with a blood-stained discharge present between

the periods. Cullen, in an examination of 1,674 cases of fibromyomata of the uterus, has found eighteen cases of carcinoma or a frequency of a little over 1 per cent. If that is the average frequency, and I think it may be assumed it is, I consider that if total hysterectomy be practised by large numbers of operators the increased risk of the operation will exceed that of patients dying subsequently of cancer in the stump of the cervix, even assuming that all such cases end fatally, which is unlikely. I am of opinion that no hard and fast rule can be laid down, and I shall continue to practise subtotal hysterectomy in patients in whom the cervix is quite healthy at the time of the operation and the case an uncomplicated one of fibromyoma of the uterus, but in any complicated or doubtful case I should not hesitate to resort to total hysterectomy.

Dr. H. RUSSELL ANDREWS: Dr. Spencer has not made out a strong case for panhysterectomy for fibroids. Can he bring forward any evidence to show that sarcoma is likely to recur in the cervix after subtotal hysterectomy? Two of Dr. Spencer's four cases of carcinoma are hardly worth inclusion, as in one the cervix could not be felt and in the other Dr. Spencer had been suspicious of the cervix. At one time I performed panhysterectomy as a routine operation for fibroids, but for the last ten years I have removed the whole cervix only in cases in which laceration and eversion of the cervix have made it probable that there would be persistent discharge if the whole cervix was not removed. My practice is to scoop out most of the cervix, and in doing this I have only once found unexpected carcinoma, although I have removed many uteri which contained both fibroids and carcinoma. During the last eight years 761 hysterectomies for fibroids, chiefly subtotal, have been performed at the London Hospital. The late Dr. Maxwell and myself were for many years on the look-out for carcinoma occurring in a cervix that had been left behind, but no such case has been seen at the London Hospital. I have seen very many patients who came to the London Hospital to seek advice for ailments, some gynæcological and some having no connexion with gynæcology, after subtotal hysterectomy for fibroids, but I have never seen carcinoma of the cervix among any of them. I cannot believe that carcinoma of the cervix is common among these patients, as it is inconceivable that patients who develop carcinoma should always go elsewhere for advice, while for advice on every other ailment so many return to the London Hospital.

Dr. EDEN: Dr. Spencer has not pursued his argument to its logical conclusion, for he thinks the cervix should be removed because it may at the time be the seat of undiscovered cancer, or, alternatively, that it may subsequently become cancerous. But two of Dr. Spencer's cases, although in a very early stage of the disease, died of recurrence, and it cannot be doubted that an extended hysterectomy (Wertheim) would have given them a better chance. To be consistent Dr. Spencer should advocate Wertheim's operation instead of panhysterectomy. Again, does Dr. Spencer conserve the ovaries in

operating for fibroids? These organs also sometimes become the seat of primary cancer. It is very difficult to see at what points these precautionary measures should stop.

Mr. J. D. MALCOLM: I have for many years removed the whole uterus in operating for fibromyoma, because in two cases I attributed septic trouble to a spread of infection from the cervix uteri. Recently I have on two occasions left the cervix. In one case the removal would have been unusually difficult and the patient's condition was so serious that time was of importance. Death was due to gangrene of the cervix. I have seen one case in which there was an irremovable cancer of the cervix uteri and, as far as I could trust the evidence, the uterine body had been removed for fibromyoma.

Mr. T. G. STEVENS: My opinion is that Dr. Spencer's first two cases are not cases of cancer of the cervix at all. The appearances seen in the microscopic sections are those with which I am quite familiar in erosions of the cervix. They constitute a proliferation of the vaginal epithelium around the openings of glands and constantly occur in erosions. They are not of the nature of a carcinoma, and never give rise to a carcinoma.

Dr. LAPTHORN SMITH: Dr. Herbert Spencer has once again reminded us of the tendency of fibroid tumours to become infected with cancer. This is a point of vital importance, for there are still many people and a few doctors who tell women with fibroid uteri that there is no hurry about having them removed until they have attained a very great size. I have seen a great many tumours which the women have carried for many years, and they were told that it was a benign growth and that there was nothing to worry about. And yet some ten or fifteen years later those benign tumours had become malignant, and some of the women lost their lives in consequence. But in my experience, which is less extensive than Dr. Spencer's, I have not known a single case in which, after the removal of a fibroid of the uterus, the remaining inch or two of cervix has become infected. My own opinion is that when we have done a supravaginal removal of the tumour, and especially if we have done it early, we have permanently cured the patient, and that the risk of the cervix afterwards becoming malignant is very slight. Another reason why this is so is that the cancer cell does not grow on normal tissue very easily, but nearly always on scar or fibrous tissue. If the woman were a multipara and had a badly lacerated cervix healed not by sewing but by granulations, then she would run as great a risk of that lacerated cervix becoming infected as any other woman of her age. But, as we know, fibroid tumours are most common in childless women, the cervix is normal in structure, and therefore unsuitable for the cancer cell to be implanted into it. Such being the case, I have abandoned the longer and more difficult and dangerous operation of total extirpation, and now invariably leave the cervix from the internal os downwards. In a stout woman with the tumour low down in the pelvis even the most skilful operator has an arduous task to perform, which requires much time under anæsthesia. But there is

another reason why the majority of the operators now leave the cervix, and that is on account of the troubles which are due to the shortening of the vagina. One would hardly think that taking out that cervix would make any difference at all, but we must remember that the cervix helps to form the roof of the vaginal pouch, and when you have taken it out and brought the sides together the vaginal space has been quite considerably abridged. At any rate, a great many operators of the highest standing have told me that they had had enough marital troubles related to them to induce them to leave the vagina at its full length. Finally, I might point out that we might warn the women to be inspected every six months or year, and on the slighest appearance of any symptom of malignancy to report to the doctor or some other operator, when it would be only a matter of a few minutes to remove it without difficulty or danger. On the other hand, all the cases of cancer of the upper part of the vagina which I have seen were recurrences after the very operation of total extirpation. The disease was implanted on the raw edge of the vaginal incision by operators who did not understand the frightful contagiousness of cancer, and took no extraordinary precautions to disinfect the cut edges. And the last state of these patients was worse than the first.

Dr. HERBERT SPENCER: The papers read to-night deal with two questions only, the frequency with which sarcoma is mistaken for myoma, and undiagnosed cancer occurring in operating for myoma, as points in determining the partial or complete operation. The President's case is an interesting one, especially in view of four years' freedom from recurrence; but, I ask, Is this the only case he has had of supravaginal amputation for sarcoma mistaken for myoma? He asks if my cases occurred in the cervix. I do not know. In one the vagina was full of growth which probably affected the cervix: in the other there were numerous metastases, some in the pelvis, but, as no post-mortem examination was made, I am unable to say with certainty whether the cervix was involved in these cases. But I am astonished to hear the statement that uterine sarcoma does not recur in the cervix, of which there are (comparatively) numerous instances on record, one of which is illustrated by a most striking drawing, and another described in Kelly and Cullen's book on myoma, which the President quotes with deserved approval. Large figures do not carry weight unless they are based on thorough examination. Kelly and Cullen's 1,400 cases of myoma were admittedly not thoroughly examined in the early years. They give seventeen cases of undoubted sarcoma and seventeen suspicious cases. But they say "without doubt some cases have been overlooked." "During the last decade . . . . careful pathologic records have been kept. We feel confident that as a result of these careful studies . . . . in the next few years this malignant change will be found to be relatively common." Dr. Russell Andrews expects his patients to come back to him when they get recurrence; that is generally the last thing a patient would think of doing when she has been promised a cure of her bleeding by supravaginal hysterectomy. Dr. Andrews asks why he does not

see other gynæcologists' cases of recurrence in the stump. I can only tell him why he does not see mine. It is all a question of frequency: no one can deny that it occurs after the partial operation. I am sorry to hear that Dr. Fairbairn has given up doing total hysterectomy, although he recognizes its advantages. It will, of course, not always cure a case of myoma complicated with cancer; the important question is, Which is the better operation for such a case? Dr. Eden says the total operation involves more risk. I am of opinion that it involves less risk, and have published evidence in favour of my statement and will publish even more convincing evidence, but that is not the subject of these papers. Mr. Stevens says that the first case was an erosion. Who ever saw an erosion high up in the cervical canal, the lower part being healthy? Dr. Lapthorn Smith says the total operation shortens the vagina; it cannot shorten it by more than $\frac{1}{2}$ in., and if the cervix is large it may even lengthen it. But of what importance is this? Again I ask for a record of 100 or 200 cases of supravaginal amputation for myoma in which every case has been followed up and examined after five years. After more than sixteen years' experience of the total operation I am able to say that the results, both immediate and remote, are good.

REPORT OF THE PATHOLOGY COMMITTEE.

(*March* 21, 1917.)

(1) Dr. Roberts's specimen of "Tumour of the Cervix undergoing Curious Degeneration": "We have examined this specimen and the sections. We are of opinion that it is a round-celled sarcoma in which hæmorrhage and necrosis have occurred to a marked degree. Histologically the cells have an arrangement around vessels, probably meaning that these are the only parts fairly well nourished."

(2) Dr. Herbert Spencer's specimen of "Carcino-sarcoma Uteri": "We have examined this specimen and the sections. We are of opinion that the uterus contained two growths, one having the structure of a columnar-celled carcinoma, the other that of a spindle-celled sarcoma, and that in places these growths appear to mingle. On these grounds we consider that the term carcino-sarcoma is justified."

(3) Mr. J. P. Hedley's specimen: "We have examined this specimen and the sections. We are of opinion that the uterus contains two distinct growths, one of which is a columnar-celled carcinoma; the other has the appearance of a spindle-celled sarcoma."

Section of Obstetrics and Gynæcology.

President—Dr. G. F. Blacker.

(*May* 3, 1917.)

Case of Spontaneous Rupture of the Uterus.

By Gordon Ley, F.R.C.S.

The specimen shown was removed, post mortem, from the body of a multiparous woman aged 46. She was admitted to the City of London Lying-in Hospital on February 10, 1917, under the care of Dr. Eardley Holland. The present pregnancy was her twelfth. She had had nine full-term labours, one instrumental, the last being in 1913, and two miscarriages, the last in 1914. She was full term. The pregnancy had been normal to the thirty-fifth week, when she had a considerable hæmorrhage lasting for one day. Since that date slight losses had occurred at intervals of two or three days, till February 9, when another considerable loss occurred. There had been no pains.

The membranes had ruptured on February 8. She was seen by a midwife on February 10, and sent to the City of London Lying-in Hospital. I saw her at 11 a.m. on February 10. She was considerably anæmic. Her temperature was 97° F., her pulse-rate 80. The abdomen was pendulous. The baby was lying as a right occipito-anterior. The fœtal heart was heard. On vaginal examination, the cervix admitted three fingers, the canal being long. The head was firmly engaged. The edge of the placenta was felt posteriorly at a distance of 1 in. from the edge of the internal os. I considered that, with the aid of a tight binder, delivery would be completed without

further loss. This treatment was therefore adopted. There were no pains till 9.15 p.m. on February 11, when, after a labour lasting for one and a quarter hours, the baby was born. There was no ante-partum loss, and the baby was alive. The third stage of labour was completed in twenty minutes without excessive loss. A troublesome cough caused inconvenience to the patient and nurse during this stage. Ten minutes after the completion of the third stage, the patient complained of sudden dyspnœa. She became restless and her breathing was difficult and laboured. The anæmia increased, the pulse became feeble and rapid, and she died in fifteen to twenty minutes. A tentative diagnosis of pulmonary embolism was made.

On performing a post-mortem examination the following condition was revealed. The peritoneal cavity was intact. There was extensive retroperitoneal hæmorrhage into the substance of the left broad ligament, extending upwards to the lower pole of the kidney, and stripping up the pelvic and descending colon. This hæmorrhage originated from a rent in the left wall of the lower uterine segment.

The uterus, bladder and broad ligaments were removed and examined. The uterus measures 27 cm. by 14·5 cm. by 3·8 cm. Attached to it is a vaginal cuff 5 cm. long. The tissues of the left broad ligament and the peritoneum covering the left broad ligament, as well as the peritoneum covering the left half of the lower uterine segment, are purple-red in colour, due to infiltration of the subperitoneal tissues by blood. On opening the uterus longitudinally along its right border, the placental site is seen on the posterior wall of the lower segment. It measures 12 cm. from side to side by 7 cm. vertically, the lower margin of the site reaching to within 2·2 cm. of the internal os. The demarcation between the cervical canal and the body of the uterus is clearly defined. The canal has a circumference of 14·2 cm., and a length of 6·3 cm. Its mucosa is very slightly bruised. The body of the uterus is lined by a soft-velvety, pinkish decidua. The wall of the upper uterine segment has a thickness of 2·2 cm., that of the lower uterine segment 1·8 cm. to 2 cm. There is no line of demarcation between the segments. In the left border of the uterus commencing in the cervical canal 2·3 cm. below the internal os, and terminating in the body 7·5 cm. from the fundus, is a tear 13 cm. long. The tear passes from below vertically upwards for a distance of 2·2 cm., then obliquely upwards and forwards, and finally again vertically upwards; it passes through the left margin of the placental site. Its depth varies, but in its central and deepest part it extends through the wall of the uterus

into the tissues of the left broad ligament. Microscopical sections show an apparently normal puerperal uterus lined by a decidua.

The remaining organs show little worth noting.

There is vesicular emphysema of both lungs, and a fine portal fibrosis of the liver.

I have thought this case worth bringing before the notice of the Section, first, because of the rarity of spontaneous rupture in a previously normal uterus; secondly, because of the association in this case of rupture and placenta prævia. I have seen three other cases of spontaneous rupture, all of which occurred through a Cæsarian section scar. It might possibly be considered that erosion by villi would weaken the wall of the uterus in the region of the placental site, and therefore tend towards the occurrence of rupture, but in all sections of placental sites I have examined a thin layer of decidua has separated villi from muscle. I think, therefore, we must assume that the causative factor does not lie here. The actual cause of the rupture in this case cannot definitely be proved. In past ages it would have been confidently attributed to fatty degeneration, seen by the naked eye. This did not exist in this case. The other factors here present—age, parity, and anæmia from previous losses—may play some part as causative factors.

(*May* 3, 1917.)

Decidual Reaction in a Subperitoneal Fibromyoma of the Uterus.

By Gordon Ley, F.R.C.S.

The specimen here described was removed from a primigravida aged 35. She was admitted, at full term, on January 29, 1917, to the London Hospital under the care of Dr. Russell Andrews. She had had ante-partum hæmorrhage a few weeks previously. She was also suffering from partially compensated aortic regurgitation and mitral stenosis. The head was above the brim, and could not be made to engage. She was in labour. Cæsarean section was decided on, and was performed by Dr. Russell Andrews under stovaine. A small subperitoneal fibroid was situated low down on the lower uterine segment, posteriorly. It was removed by myomectomy. The tumour was date-shaped, and lay with

its long axis parallel to that of the uterus. It was indefinitely encapsulated. Mother and baby did well.

The tumour was hardened in 4 per cent. formalin. It measured 5 cm. by 3 cm. by 3 cm. Its external surface was extremely ragged, and was covered by tags of greyish, tough tissue. On section it showed a mottled grey and white appearance. The white areas were larger than the grey. They were clearly defined, irregular in shape, and consisted of shiny, fibrillar closely-packed strands. The grey areas were depressed, softer than the white areas, and were both patternless and translucent.

Microscopic sections were stained by hæmalum and eosin, and by hæmatoxylin and van Gieson's method. The sections show bundles of unstriped muscle fibres, the bundles being of large and medium size. They pass in varying directions, and are seen cut longitudinally, obliquely, and transversely. The bundles are separated by strands of fibrous tissue, from which a collagenous meshwork arises and encircles the individual cells of the bundles. The muscle-cells are large. Scattered throughout the section are nodules composed of large polygonal cells. These nodules are infiltrated by a few polymorphonuclear leucocytes and lymphocytes. Blood spaces, lined by a single layer of endothelial cells, are present in considerable numbers in the nodules. The individual cells of which the nodules are composed are very regular in size and shape, and consist of a pale cytoplasm, and a single, large, rounded, usually eccentric nucleus with well-defined chromatin net, and one or two nucleoli. They lie in a hyaline collagenous matrix. Transition from the rod-shaped muscle-cell, with rod-shaped nucleus and ill-defined nucleolus, through the rod-shaped cell with spindle nucleus and sharply-defined nucleolus, to the spindle-shaped cell with oval nucleus, and then to the polygonal cell with rounded nucleus, is clearly seen. Fibroblasts are seen lying between the polygonal cells in the nodules. The appearances suggest transition from fibroblasts to polygonal cells, but this cannot be followed through the various stages with that facility which obtains in the case of the muscle fibres.

Decidual reaction arising in structures other than the endometrium is of frequent occurrence. It occurs to a slight or considerable extent in the connective tissues of the Fallopian tube in all cases of tubal pregnancy. It has also been described in the ovarian connective tissues in cases of ovarian pregnancy. It is frequently met with post mortem on the peritoneum forming the floor of Douglas's pouch, lateral pelvic walls, and has been noticed as far distant as the appendix, pelvic

colon and surface of ovary. I have seen three specimens in which it occurred in the stroma of a cervical erosion. In each of these cases carcinoma was suspected, and in two of them hysterectomy by Wertheim's method was performed without sections being taken, so apparently certain was the diagnosis. I can find no other mention of decidual reaction in a fibromyoma in the literature, but its existence has been known to many of us for some considerable time.

I have to thank Dr. Russell Andrews for kind permission to publish this case, and also Dr. Hubert M. Turnbull for kind assistance in compiling the pathological report.

DISCUSSION.

Dr. LOCKYER: Mr. Gordon Ley is to be congratulated on making a very definite addition to our knowledge, in demonstrating the transition of involuntary muscle fibres into decidual cells in response to the stimulus of pregnancy. I have found decidual reaction in the ovary associated with ovarian gestation, and there are two ovaries in my collection showing a coating of decidual tissue from a case of intra-uterine pregnancy. As stated by Mr. Ley, the same reaction has been found on the serous coat of the appendix vermiformis and in various situations on the pelvic peritoneum. I have seen it on the surface of a uterus removed by Cæsarean hysterectomy, and Penkert has demonstrated the same in the omentum. Decidual reaction has also been noticed in the "cytogenous" or periglandular tissue of an adenomyoma by several observers, including Cullen, Whitridge Williams, Amos, Robert Meyer, and, in this Section, by Dr. W. S. A. Griffith. I feel sure Mr. Ley is right in affirming that the specimen removed by Dr. Russell Andrews is the first recorded case in which a pure fibromyoma has demonstrated decidual metaplasia of its muscle cells.

Dr. H. RUSSELL ANDREWS: I was puzzled by the appearance of the small tumour on the back of the uterus. It had a peculiar roughly granular surface and a rather grey colour. I thought that it was probably an adenomyoma, and was much interested to find that the microscopical sections showed decidual reaction in a fibroid and no trace of glandular tissue.

(*May* 3, 1917.)

Post-operative Tympanites: Its Nature and some Points in its Treatment.

By JOHN D. MALCOLM, F.R.C.S.Ed.

IN October, 1914, I published[1] five cases in which a post-operative tympanites, threatening to cause immediate death, was treated by making an intestinal fistula, and four of the patients recovered. The tympanites was attributed to an increased resistance to the passage of the intestinal contents, associated with an enfeebled peristaltic action, and especially to delay in the large intestine, these being regarded as effects of exposure and manipulation of the abdominal contents during the operation. It was suggested that the condition might be described as one of "acute intestinal stasis."

In 1915, Professor Arthur Keith[2] recorded the results of investigations into the nature of certain areas of nodal neuro-muscular tissue which consist of specializations of Auerbach's plexus of nerves. These areas of nodal tissue are situated at various parts of the alimentary canal where delays in the passage of its contents normally take place, for example at the ileo-cæcal valve and at the pylorus. Professor Keith concluded that the nature and functions of the myenteric plexus of the intestine are physiologically similar to those of the nodal and conducting system in the heart, and that each specialized nervous area is the originator and regulator of peristaltic activity in the section of bowel immediately beyond it. He pointed out that a want of co-ordination between the muscular activity of the various sections of the alimentary canal may give rise to effects comparable to those of heart block, and argued that the condition of intestinal stasis is produced in this way.

The application of Keith's explanation of intestinal stasis to the condition which I ventured to call an acute intestinal stasis aids greatly the understanding of the nature of a post-operative tympanites. In the course of a satisfactory convalescence from an uncomplicated operation

[1] *Proc. Roy. Soc. Med.*, 1914-15, viii (Sect. Obst. and Gyn.), pp. 16-24.

[2] "The Cavendish Lecture," *West Lond. Med. Journ.*, 1915, xx. p. 149.

upon the abdomen, gases pass to, and are expelled from, the anus, as a rule, on the second or third day. Unless these gases escape very early, say within twenty-four hours, the patient suffers from more or less abdominal distension, or a feeling of such distension with discomfort or crampy pains and nausea or vomiting. Prompt relief may generally be given after a day or two by administering a dose of castor oil, and apparently the purge is useful simply by restarting the muscular activity of the intestine, which without this stimulus may remain in abeyance. The symptoms above mentioned cease when gases escape freely through the anus, but if this relief is not obtained a condition of tympanites arises and becomes more obvious, distressing and dangerous. The distension and vomiting increase rapidly. Every variation, between the very slightest discomfort and a complete fatal retention of the intestinal contents, may be observed and therefore the most serious of these conditions would seem to be in some cases an exaggeration of changes which take place in nearly every case of abdominal surgery.

When a post-operative tympanites is fully developed the surgeon may still occasionally save his patient by opening and draining the intestine, at the cæcum or at some higher point. If the condition is cured in this way the distension subsides as quickly as the excess of fluids and gases in the small intestine is expelled through the fistulous opening, sometimes within two or three hours, and the case, if otherwise uncomplicated, is then put in the position, as regards symptoms, of one in which gases have passed freely from the anus. In the majority of such cases, after a delay of one or two days, an evacuation of the intestine *per vias naturales* is easily obtained, although every effort in this direction before the fistula was made had been ineffective. The conditions show that there is in these cases no real paralysis and no real obstruction of the intestine either above or below the place where the fistula is made.

When patients die with a fully developed tympanites unrelieved, even if the conditions are aseptic before the operation, and if the most careful precautions have been taken to prevent septic contamination of exposed tissues, an early stage of diffuse peritonitis is generally found at an autopsy. In my experience this condition is invariably present if the patient dies sufficiently slowly. Infecting organisms pass through the wall of the distended intestine and so reach the peritoneum when the patient is *in extremis*. If, during an operation, septic parts are manipulated or removed the surgeon often closes the abdomen, knowing that contamination of the peritoneum has occurred and trusting that

the special powers of this membrane for destroying bacteria will protect his patient from infection. In properly selected cases there will be no danger so long as the convalescence is uncomplicated, but if under these circumstances a post-operative tympanites develops and is persistent, a diffuse septic peritonitis will arise earlier and spread further and more rapidly than in an aseptic case. Micro-organisms which have not been completely destroyed assert their pathogenic powers as the tissues become enfeebled.

On the other hand, if death is fairly rapid there may be no evidence of a diffuse peritonitis. Dr. Mary A. D. Scharlieb, in the debate on my last paper on this subject,[3] recorded a case in point. At a second operation nothing was discovered to account for a fatal tympanites except that the colon was enormously distended by fæces. No peritonitis was discovered at an examination afterwards. Moreover, after a tympanites is fully developed, when an operation is performed to look for an intestinal obstruction or to make a fistula, if the tissues are aseptic to begin with and carefully treated, there is rarely any sign of a diffuse peritonitis until the patient is moribund. Therefore the tympanites, for the relief of which the second operation is performed, cannot be caused by the peritonitis found after death in the majority of these cases. On the contrary the tympanites develops first and the peritonitis is an invariable result of the intestinal distension if the patient dies sufficiently slowly to allow an inflammation to develop.

It would appear, therefore, that in the cases under consideration no obstruction and no paralysis and no inflammation of the intestine precede the onset of tympanites. But it is certain that before the fistula is made the colon is not capable of receiving and removing the contents of the small intestine, whilst it is essential for the patient's safety that the small gut should be allowed to empty itself. The tympanites is therefore due to a complete obstruction of the intestine, with no other cause than a want of co-ordination between its upper and lower parts. The condition is one of acute intestine block or acute intestinal stasis.

The large intestine is by far the commonest situation of this trouble, but cases of acute dilatation of the stomach after abdominal operations are on record, and Keith's explanation of the nervous arrangements of the alimentary canal applies to this gastric distention as fully as to the ordinary every-day symptoms which sometimes develop into a dangerous state.

[3] *Proc. Roy. Soc. Med.*, 1914-15, viii (loc. cit.), p. 25.

A difficulty arising from a want of co-ordination between the various parts of the alimentary canal must be aggravated by any condition which tends to hamper the action of the part physiologically in distress, and so a formation of intestinal adhesions, an overloaded condition of the colon and any other circumstance which delays the passage of the bowel contents must help to bring about their retention after an abdominal operation. A subject of intestinal stasis will almost certainly develop an acute intestinal stasis unless the bowel is stimulated. The continuous administration of opium after these operations, at one time usual, was a definite cause of delay of fæces in the colon, and one of the most remarkable improvements in abdominal surgery is the reduction in the number of cases of difficulty and of death from post-operative tympanites which took place when surgeons ceased to lock up the bowels as long as possible after every abdominal operation. Without some complication tending mechanically to hamper the activity of the intestine, a serious degree of tympanites rarely, if ever, arises in aseptic cases, a brisk purge being generally sufficient to restore the functions of the bowel.

If a post-operative tympanites is to be treated by making a fistula there should be no delay in operating after the necessity for this measure has been determined, and it is evident that in every case in which a persistent post-operative tympanites can be cured in this way the development of that condition would be prevented if the surgeon could foretell the event and make a fistula as part of the original operation. This view is supported by the behaviour of three cases in which I performed major abdominal operations upon patients in whom a cæcal or lower ileal fistula existed. In each case during the first three or four days of convalescence there was an increased discharge from the artificial opening in the bowel, and then the flow ceased or greatly diminished, and recovery was rapid and free from trouble.

In two of these cases[1] the cæcum was closed subsequently without any complication during convalescence, so there was no intestinal obstruction. In the third case[2] the patient died of general tuberculosis, with the fistula and a very large tuberculous pelvic abscess still unhealed. In these cases the pressure in the cæcum and lower ileum was certainly increased during the first few days of convalescence from an abdominal operation.

[1] Loc. cit., Cases II and V.

[2] Loc. cit., Case I.

After considering all the facts I treated and published, in the communication already quoted, two cases,[1] in which a fistula was made in the course of an abdominal section with a view to preventing the onset of a post-operative tympanites when this complication was thought likely to occur. The recovery, in both, was free from all cause for anxiety concerning the functions of the alimentary canal, and I have since operated upon eleven cases of this kind, and assisted at one successful operation for the relief of a fully developed tympanites.

Case I.—A healthy strong woman, aged 34, was seized suddenly with intense abdominal pain and became very ill. Two days later she had all the signs of an acute inflammation of the lower part of the abdomen, which was thought to be due to mischief in the vermiform appendix. The abdomen was opened without delay and opaque serous fluid and flakes of lymph escaped. The vermiform appendix had been inflamed at some time and was removed, but it lay on the outer side of the cæcum, and evidently it was not the cause of the patient's illness. There was no exudation of lymph in its neighbourhood. Both Fallopian tubes were dark-purple coloured and very much swollen, but the fimbriated extremities were free, and no pus could be expressed from them. On the left side the ovary and tube were partly adherent but easily released. There was no lymph on the tubes or ovaries. The pouch of Douglas, the back of the uterus, and the small intestine and its mesentery in the lower part of the abdomen were extensively covered by thick flakes of adherent lymph, which in places fixed the coils to each other. The intestines in the upper abdomen were red and distended, but there was no lymph on them. Nothing was removed except the appendix, the free fluid and some of the lymph. The pelvis was drained, and I fitted a Paul's tube in the cæcum through a second incision. The patient had a pulse of 150 when taken from the operating table, but it was 96 next morning. The Paul's tube acted well, and the patient's condition caused no anxiety during convalescence. The tube in the peritoneal cavity was removed after a few days. There was in this case great difficulty in getting the bowels to act by the natural way. Twenty-nine days after the operation I re-opened the abdomen. All the pelvic parts seemed normal except the left ovary, in which a cyst measuring about 2 in. in diameter had developed. This burst in removal. To avoid the effects of any cause of obstruction in the colon, I performed an ileo-sigmoidostomy and closed the cæcum. Recovery gave no anxiety, and eight months later the patient was in perfect health, except for a small incisional hernia which should be repaired.

The cause of the symptoms in this case was obscure. A cyst similar to that removed at the second operation may have burst when the patient became ill, but the bursting of such a cyst would hardly account for the conditions found.

[1] Loc. cit., Cases VI and VII.

I have since operated under very similar circumstances, but there was no exudation of lymph. The Fallopian tubes closely resembled those above described, and treatment by drainage of the peritoneal cavity alone was successful. There was a very foul discharge from the drainage tube for three days. Neither of these patients had any vaginal discharge at any time. In the second case, repeated examinations of swabs from the vagina before the patient became acutely ill and before I saw her were negative, but there was no doubt that this patient transmitted gonorrhœa from one man to another. In both cases the mischief was probably gonorrhœal. The degree of inflammation in the first case was certainly sufficiently severe to justify a belief that intestinal difficulties were likely to arise.

Case II.—A patient, aged 43, had pan-hysterectomy performed on account of a painful inflamed adherent fibromyoma of the uterus. Between the uterus and the rectum there was a calcareous mass measuring about ½ in. in diameter, and in removing this I opened the rectum. It was closed by sutures, and the peritoneum was arranged so that if a fistula formed the fæces would pass into the vagina rather than into the peritoneal cavity. To rest the rectum, and so facilitate its healing, a Paul's tube was fixed in the cæcum. Convalescence was uninterrupted except that for about three days, after the bowels had begun to act naturally, there was a discharge of fæces through the vagina. The cæcum was closed thirty-five days after the operation, and the patient was reported well a year later.

Case III.—On exploring the abdomen I found a cancerous growth of the transverse colon, narrowing its lumen and causing distension between the stricture and the ileo-cæcal valve. It was doubtful whether the patient could bear a resection of the part or an ileo-sigmoidostomy, and therefore I opened the cæcum and fitted it with a Paul's tube. When this was done the patient seemed to maintain her strength, so I removed the transverse colon wide of its growth and joined the ends. The pulse did not rise above 108, and the Paul's tube was acting well by the end of the operation. The cæcum was closed after twenty-four days and recovery was satisfactory, but fifteen months later a recurrence affected the liver.

Case IV.—The specimen removed from this case has already been shown to the Section.[1] An abscess formed around a fibromyoma in the posterior wall of the uterus. This burst into Douglas's pouch, and the abscess cavity at the operation was formed by the widely opened gap in the uterine wall and by adherent peritoneum, the sigmoid flexure and its mesentery forming the greater part of the upper boundary. A sloughed fibroid about the size of a billiard ball was free in a large collection of pus, which occupied nearly the whole pelvi

[1] *Proc. Roy. Soc. Med.*, 1914-15, viii (Sect. Obst. and Gyn.), p. 104.

cavity. The uterus was removed and the pelvis was packed with gauze. The sigmoid flexure on its lower surface had a blue-black colour, and appeared likely to become gangrenous. It was also so fixed that it could not be pushed down to meet the bladder peritoneum. I therefore introduced a Paul's tube into the cæcum to rest the pelvic parts. The cæcum discharged its contents through the fistula, until a dose of castor oil on the third day induced a free evacuation *per rectum*, after which the artificial anus hardly acted at all, and recovery was so satisfactory that the need for the fistula seemed doubtful. It must, however, be remembered that there was a considerable escape from the cæcum during the first three days of convalescence, and this is the time when a post-operative tympanites usually begins, and when an undisturbed pelvic condition is important. After thirty-nine days the cæcum was closed easily, and there was no subsequent trouble. The pelvic parts were completely healed when the patient recovered from the second operation.

Case V.—A patient, aged 63, twenty years after a hysterectomy by another surgeon, had a hernia in which a large portion of the intestine was fixed by adhesions. The cæcum and many coils of small gut were separated and replaced in the abdominal cavity with difficulty on account of their size. I made a cæcal fistula in this case to prevent undue tension, which must have arisen from the slightest degree of tympanites. The cæcum was closed thirty-nine days later, when the size of the intestines and of the peritoneal sac had become accommodated to each other, and there was no trouble after either operation.

Case VI.—Six days after removal by Dr. Hubert Roberts of a ruptured extra-uterine fœtation with much blood-clot, the patient, aged 33, showed symptoms of intestinal obstruction. Three days later I saw her in consultation and assisted at a second operation, concerning which Dr. Roberts kindly allows me to publish the following. On reopening the abdomen about half a pint of serous fluid escaped. Distended small intestine, 2 in. in diameter, was seen in the upper abdomen, where a slight volvulus of many coils had formed. This part of the intestine was incised and drained, the wound being carefully closed. Many recent adhesions, mostly in the pelvis, were broken down. The whole of the peritoneum was reddened, and it looked very unhealthy, but there was no pus anywhere. A Paul's tube was fixed in the cæcum and a rubber tube in the pelvis. An uninterrupted convalescence followed, and I closed the cæcum twenty-six days later. Recovery from this also was uneventful.

Case VII.—A child, aged 4½, complained of abdominal pain, which passed off. At the end of a week she was very ill. A doctor was then called in, and when I saw her, on the eighth day after the first symptom, the abdomen was distended and immobile. I removed an enlarged, very red appendix from far back in the abdominal cavity. To get the intestines out of the way it was necessary to incise and drain them. The opening in the bowel was sutured, a Paul's tube was fixed in the cæcum, and the abdomen was closed, but the child died a few hours later. The only way to have saved this patient's life was to

have operated much earlier. The intestine was paralysed as a consequence of septic peritonitis.

Case VIII.—This case is already published.[1] After removal of both ovaries and Fallopian tubes an enormous pneumo-peritoneum developed. The gas was let out, and it was seen that all the intestines were adherent, although they were not so at the first operation, seven days earlier. They were rather empty than distended. The cæcum was drained, and recovery was in every way satisfactory. The fistula was closed four weeks later.

Case IX.—An exceedingly anæmic, unhealthy-looking woman, aged 21, became acutely ill, and on opening the abdomen a cystic ovary almost universally adherent on the right side, measuring about 2½ in. in diameter, was removed with an inflamed Fallopian tube. On the left side the tube and ovary were adherent and seemed to be enlarged. In separating them my finger slipped into a mass of exuberant granulation tissue, which constituted the apparent enlargement of the ovary. This tissue was continuous with the mucous membrane of the sigmoid flexure through a considerable gap in its muscular wall. After removing the unhealthy mass, the edges of the opening in the bowel wall were freshened, and it was closed by catgut sutures, but these were necessarily placed in unhealthy tissues. The pouch of Douglas was drained by means of a rubber tube, and the cæcum by a Paul's tube. Recovery was not complicated in any way. Fæces escaped freely at first from the fistula but gradually passed downwards more and more. The peritoneal discharge soon ceased and gave no trouble. After four weeks I closed the fistula, and an adherent unhealthy vermiform appendix was excised with some difficulty because of the depth and fixity of the cæcum. Recovery was uneventful, except that the wound did not heal by first intention.

Case X.—A woman, aged 28, was sent into hospital acutely ill. Her condition improved, and after five days I operated and found an appendix abscess in the pouch of Douglas rather to the left side. A large granulating surface on the sigmoid flexure and another on the last few inches of the ileum and around the base of the vermiform appendix were cleaned as much as possible. The ileum was divided above the raw part, and the upper end was inserted into the side of the ascending colon. The lower end was brought out through a lateral incision and fitted with a Paul's tube. Drainage of the pelvis was arranged through the vagina, and the abdominal incision was closed. Gases and fæces escaping from the fistula had to pass backwards through the ileo-cæcal valve, and hardly any fæces were discharged in this way, but enough gases escaped to prevent any abdominal distension, and recovery was satisfactory, being altogether free from intestinal difficulties. The fistula was closed twenty-three days after the operation, and recovery was again uncomplicated.

Case XI.—When this patient's age was 21 both her Fallopian tubes, her right ovary, and part of the left ovary were removed for suppurating

[1] *Proc. Roy. Soc. Med.*, 1916, ix (Sect. Obst. and Gyn.), p. 85.

inflammation. Seven years later she returned with a swelling on the left side of the pelvis. An inflamed cystic ovary, buried very deeply in the pelvis, was enucleated with difficulty, but I thought satisfactorily, and the abdomen was closed, after the uterus had been fixed, so as to keep the intestines away from the large area of raw tissue necessarily left. During the following night some blood escaped from the rectum, and next morning fully half a pint of fluid blood was discharged when a tube was introduced through the anus. As it was obvious that there had been an injury of the bowel, I fixed a Paul's tube in the cæcum through a lateral incision without delay. I then reopened the median incision. There was no blood in the general peritoneal cavity, but on lifting up the uterus a clot was seen. After clearing this out two small vessels were ligatured, drainage through the vagina was provided, and the abdomen was closed. No opening in the bowel was discovered, and no visible fæces had escaped from it. This patient's condition never became satisfactory, gases and fæces escaped both from the fistula and from the anus, but never freely either way, and after a time they ceased to pass. The patient then began to vomit, and she died on the eighth day after the second operation without becoming greatly distended.

The following statements are taken from Dr. Salusbury Trevor's report of the autopsy:—

The deep part of the central wound had not healed well. There was a general peritonitis with lymph in the pelvis over the viscera. The last 3 ft. of the ileum were empty and collapsed, the upper end of this collapsed portion being fixed to the back of the central wound by adhesions which caused an obstruction. The upper parts of the small intestine were moderately distended.

The colon was empty and mostly collapsed. The uterus was fixed to the rectum. To the left of the uterus was a cavity containing grumous fluid, shut off by adhesions and well drained through the vagina. There was a tear in the rectum $\frac{3}{4}$ in. long low down on its left side. Death was attributed to "syncope from general septic peritonitis and acute small gut obstruction."

After the autopsy it seemed to me that in this case the intestinal adhesions to the back of the central abdominal incision followed immediately upon the second operation. These adhesions were the cause of the inefficient flow of the bowel contents. After a time the intestine became completely obstructed, and the diffuse septic peritonitis followed. This patient lived eight days after the second operation, and a spreading septic peritonitis as the primary source of trouble would, in my opinion, have caused death much earlier. The upper intestine was not paralysed even just before death. If it had been it would have been much more dilated. A fistula formation higher up the bowel, or a short circuiting, should have been undertaken by a third operation.

Case XII.—A woman, aged 54, was seized with pain early one morning. She consulted Dr. James Gaff next evening and was admitted to the Samaritan Free Hospital a little more than two days after the first pain was felt. The condition was not alarming but the pain, which had become localized in the vermiform appendix region, had not abated. I operated exactly two and a half days after the illness began. A median incision exposed a very red peritoneum and adhesions over the cæcum. On separating these some pus escaped and when the parts were fully exposed a black slough consisting of the mucous membrane of the appendix was seen lying in a hollow on the lower part of the cæcum. This hollow consisted of the muscular wall of the appendix firmly adherent to the cæcum and ruptured along its whole length. On each side of the hollow the bowel wall was of a blue-black colour over a width of about ½ in. The surrounding coils of intestine were thickened and ulcerated. The slough was lifted out and the proximal end of the mucous membrane of the appendix was tied and cut off through fairly healthy tissue. The cæcum had become so fixed that it could not be brought forward. I divided the ileum near the ileo-cæcal valve and closed the lower end. The upper end was fitted with a Paul's tube and brought out of the incision. The area around the appendix region was drained by tampons and tubes. A separate tube drained the pelvic cavity. The Paul's tube acted well and there were no intestinal symptoms. The drain in Douglas's pouch was soon removed, but a large unhealthy cavity remained connected with the cæcum and when the Paul's tube separated, on the eighth day, this cavity was constantly covered by fæces. On the thirteenth day there was some secondary hæmorrhage on three occasions. I would have preferred to wait until the parts were more healthy, but in view of the hæmorrhage I opened the abdomen fourteen days after the first operation. To avoid the cæcal region I made an incision through the left rectus muscle, divided the bowel as low as possible and introduced its upper end into the transverse colon. In case I had made a mistake as to which was the upper end I brought the lower end on to the surface, thus leaving a part of the ileum with both its ends open. After this the intestinal condition gave no trouble, the wound over the cæcum became rapidly healthy, and secondary hæmorrhage did not recur, although for a short time fæces escaped which must have come through the wall of the cæcum. It was seven and a half weeks after this operation before I could remove the lower piece of ileum, which to my surprise measured 41½ in. between the two openings. The patient made a good recovery, going home twenty-eight days later.

The results of these operations with two exceptions, were altogether satisfactory as regards the absence of intestinal difficulties, but it is impossible to prove or to disprove the view that the making of a fistula to prevent the onset of a post-operative tympanites was useful. Operations upon the abdomen may appear so desperate that overwhelming complications seem almost inevitable and yet there may be no hitch in the progress of recovery. It may be, therefore, that my

prognosis was wrong every time. On the other hand a consideration of the cases will, I think, show that many of them were fairly likely to have had, at the best, a troubled convalescence if no fistula had been made, and yet the recoveries were more free from anxiety than the average. This is one of the chief features of the method. If a fistula is made to prevent the onset of a post-operative tympanites and this object is attained, the patient escapes in great measure, or altogether, even the small discomforts which are very common before gases pass from the anus. And although these discomforts are usually not important, so long as they last the surgeon cannot be sure that the resistance to the downward passage of the intestinal contents will not overmatch the propelling forces. It seems to me, therefore, that by making a fistula in the course of surgical treatment of an abdominal condition, and allowing an escape of gases and fæces through the cæcum or at some higher point, we have an exceedingly powerful means for averting intestinal troubles after these operations.

It may be suggested that it is time enough to make the fistula when the tympanites has arisen and all other methods have been tried. I am convinced however that in some cases, for example in the case already published,[1] in which the making of a fistula failed to relieve the tympanites, the patient would have a better chance of recovering with a fistula made to prevent the onset of a post-operative tympanites, than by any method for curing this condition.

It is obvious, and I would strongly urge, that on account of the uncertainty as to when a fistula is necessary, the disagreeable features of this treatment, the prolonged illness, and the need for a second operation, this method should not be employed without good reasons and it ought not to be required very often, although the frequency of its usefulness must vary with the nature of the work coming to hand.

The seven cases published before[2] and the first five of those recorded now include all[3] in which I made a fistula for the prevention or cure of a post-operative tympanites in a series of 1,000 consecutive operations involving the peritoneal cavity. In 554 cases the disease was in the female pelvic organs, the mortality of these being 2·52 per cent., and I considered myself fortunate in getting a death-rate of 3·8 per cent.

[1] *Proc. Roy. Soc. Med.*, 1914-15, viii (loc. cit.), Case IV.

[2] *Proc. Roy. Soc. Med.*, 1914-15, viii (loc. cit.), p. 16.

[3] Cases of fistula for acute intestinal obstruction, of an anastomosis for any cause, and of colotomy for cancer are not counted.

for all the cases, the lowest I have had in so long a series of abdominal operations.[1] This would have been at least 4·2 per cent. if post-operative tympanites had never been treated by making a fistula, for life was certainly saved in four of the cases[2] when the patients were beyond all hope of recovering without this method. I have not made a fistula for the cure of post-operative tympanites since these cases were published, but I have assisted at one operation of this kind, Case VI *ante*. Whether I have forestalled the onset of this complication by making a fistula in advance must be a matter of opinion. I feel sure that some of the cases now recorded would have developed a post-operative tympanites, and some would have been added to my death-rate, if the fistula had not been made, but quite likely in some this treatment was not essential.

In making a fistula for the relief of a post-operative tympanites signs of a diffuse peritonitis are occasionally seen, and it may sometimes be doubtful whether this peritonitis is the cause or the effect of the abdominal distension. In many published cases,[3] when treatment by draining the intestine was carried out, the signs of a slight diffuse peritonitis were ignored, the abdomen was closed and the patient recovered well. When this peritonitis is more advanced, as in Case VI above, and in many other published cases, drainage of the intestine must be supplemented by drainage of the peritoneal cavity if success is to be obtained.

The circumstances resemble those observed in certain cases of gangrene of the vermiform appendix. In removing this condition the surgeon frequently finds some inflammatory serous exudation from a local diffuse peritonitis, which as a rule may be safely neglected if the exuded fluid is clear. When a peritonitis is more advanced it is often a nice question whether the peritoneal cavity should be drained or not, but if the septic mischief is still further developed drainage becomes necessary. These changes may arise without any tympanites. When, as in Case VII *ante*, the peritonitis is so advanced as to paralyse the intestine the chances of recovery by any treatment become very remote.

[1] Notes of the first 400 of these cases have been published: "Remarks on 400 Operations involving the Peritoneal Cavity and a Comparison of the Death-rate, 3·75 per cent., with the Writer's Earliest Works," *Practitioner*, March, 1912. In the last 600 cases the mortality from the gynæcological work was higher, that from the others lower; although the mortality was practically the same, a considerably larger number of the simpler operations was handed over to the house surgeon, and all of these recovered.

[2] *Proc. Roy. Soc. Med.*, 1914-15, viii (loc. cit.), Cases I, II, III, and V.

[3] For example see *Proc. Roy. Soc. Med.*, 1914-15, viii (loc. cit.), p. 16, Case III.

The conditions in a case of gangrenous appendix treated by its removal and those of a post-operative tympanites successfully treated by drainage of the intestine seem to me exactly parallel as regards their relationship to any existing diffuse peritonitis which is curable. Removal of the gangrenous appendix or of the distension of the intestine is essential, and nothing else is essential to the cure of the peritonitis in the milder cases; and therefore the appendix condition and the distension of the intestine, at least in these milder cases, are respectively the causes of the peritonitis. The peritonitis in these cases of post-operative tympanites is an early development of the inflammation which, as described above, invariably occurs before death if the patient dies slowly. The more advanced cases clearly may have the same origin as the milder cases.

Peritonitis is, of course, a well-known cause of paralysis of the intestine and of tympanites, but when, as in the cases just considered, it is caused by an abdominal distension and curable by removing the distension, it cannot at the same time be the cause of the distension.

The word peritonitis is not infrequently used in the vaguest manner. On a former occasion[1] I compared it with dermatitis. The superficial area of the peritoneum cannot be very far short of that of the skin and we convey very little information when we say that a patient has dermatitis or peritonitis without some qualification describing the nature and position of the inflammation. In the paper referred to I pointed out in particular that the expression "general" peritonitis is often used when a diffuse spreading, but not general peritonitis, is obviously meant.

Lister taught that "a certain amount of inflammation, as caused by direct irritation, is essential to primary union,"[2] and according to this view a localized aseptic peritonitis arises in connexion with the healing of all wounds of the peritoneum. This inflammation is harmless, but it may cause a formation of adhesions, and such adhesions are also harmless, except when they are placed so as to hinder the activity of the intestine or obstruct its lumen, by which they may, and not infrequently do, become an important cause of intestinal difficulties, and of death after operations.

A spreading peritonitis is always septic, but except in some cases of tuberculous inflammation, it seldom becomes general. Even if a general

[1] "Inflammation and Peritonitis considered as Physiological Processes," *Med. Chir. Trans.*, lxxxix, 1906, p. 292.

[2] Lister, *Phil. Trans.* (1858), 1859, cxlviii, p. 700.

peritonitis does arise the inflammation spreads from some focus of infection, and usually involves only a part of the peritoneum before it is arrested by death or by curative measures. To cure a diffuse septic peritonitis it is essential to remove its cause or to localize its effects by draining the part of the peritoneal cavity which is affected. As a rule, in the early stages of a diffuse peritonitis, when the cause is removed the peritoneum, as already stated, destroys all remaining septic organisms, and the inflammation subsides and is harmless. Under such circumstances, there is no paralysis of the bowel. But if the peritonitis continues to spread, a paralysis of the whole intestine eventually follows, and this condition when thus produced is hopeless. Drainage of the peritoneal cavity is essential to recovery when the cause of a spreading peritonitis is irremovable, and it cannot be provided too quickly. Indeed, if, after an abdominal operation, the peritoneal sac is closed without drainage, and if a diffuse peritonitis arises and is the cause of a tympanites within a few days, the surgeon must have committed an error of judgment in failing to provide drainage during the operation, just as, if it becomes necessary to drain a tympanitic intestine, the conclusion is clear that drainage of the bowel would have been arranged with advantage at the primary operation, for both draining the peritoneum and draining the intestine are more certain in their effects when they are employed to prevent trouble than they are as curative measures.

When the peritoneum is drained to cure a septic peritonitis or to prevent its onset, drainage of the intestine also may be useful by removing or preventing the onset of a dilatation of the intestine which is an originating and aggravating cause of peritonitis, but when the bowel is paralysed as the effect of a diffuse septic peritonitis, the making of a fistula will not empty more than a small portion of it, and even if we could drain the whole intestine this could not have any curative or even arresting effect upon a diffusing peritonitis, which is the cause of the paralysis.

It is of the utmost importance to recognize that tympanites and peritonitis may each be an indirect cause of the other, but each may be brought about also directly in another way. Tympanites may be caused by an obstruction of the intestine (mechanical or physiological), and septic peritonitis by bacterial infection. In one set of cases an intestinal obstruction causes tympanites, and peritonitis follows. In another set of cases septic infection causes a spreading peritonitis, and tympanites follows. Neither of these developments can be understood

without recognizing the other, and if they are not both fully considered much confusion must arise between the peritonitis which causes distension of the bowel and the peritonitis which is caused by distention of the bowel.

It may be exceedingly difficult to disentangle the symptoms in an individual case, and the two causes of tympanites may arise independently but practically at the same time. Thus, as a consequence of an operation, and dating from immediately after it, there may be a few or many peritoneal adhesions, and a post-operative tympanites may arise from the presence of these adhesions, aggravating the usual delayed action of the bowel. There may also, in the same case, be a diffuse peritonitis. This peritonitis may be due to a bacterial infection from a stirring up of some septic focus during the operation, or from an introduction of septic organisms and neglect of the drainage necessary in such a case, or the diffuse peritonitis may be due to a septic infection from a passage of organisms through the distended bowel—a condition which must arise sometime if the patient with post-operative tympanites dies slowly. Unless the preceding history indicates it clearly, there is no way in which the surgeon can tell which of these two conditions is the cause of the diffuse peritonitis except by the effects of treatment. If the post-operative tympanites and all signs of peritonitis are removed by draining the intestine, and especially if no other treatment is necessary, the conclusion is obvious that the distension is the cause of the peritonitis. If this treatment does not effect a cure, unless the history is clearly in favour of one view, it must, I think, remain uncertain whether the sepsis arose from some condition existing before or introduced at the operation, or whether micro-organisms escaped through the distended gut, and the consequent septic inflammation advanced too far to be curable by draining the intestine.

When there is a possibility that both causes of post-operative tympanites may arise, the need for doing all possible to prevent its onset is most urgent. For example, when the peritoneum has been fouled by septic material the patient may make a perfect recovery if no complication arises, but a fatal septic peritonitis may rapidly develop if the intestine becomes distended. On the other hand, a septic peritonitis may arise and cause intestinal paralysis and tympanites. In such circumstances the surgeon, by making a fistula in the course of the operation, if this succeeds in draining the small gut, may ensure the patient against an intestinal distension which, once started, might render the case hopeless, because a septic infection might follow and

develop so quickly. In the same way, by draining the peritoneal cavity, he would reduce the chances of septic mischief spreading.

The indications for making a fistula as part of an operation are difficult to formulate. A prognosis of the patient's progress two or three days in advance is necessary. But conditions in which it seems essential to rest the lower bowel, either because it is wounded or to allow of undisturbed drainage of the pelvis, point clearly to a need for tapping the cæcum.

In cases of much manipulation or injury of the small intestine the advantages of making a fistula should also be considered. I have never had an opportunity of treating extensive wounds of the abdomen such as those which must frequently come under the notice of the members of the Royal Army Medical Corps at the present time, but if in such circumstances I could make a continuous alimentary canal, and if the patient's condition were otherwise satisfactory, it would still be wise in some cases to provide efficient drainage of the peritoneum, and in some I should consider that provision for an immediate escape of the contents of the small intestine through a cæcal fistula would offer a better chance of recovery than leaving the fæces to escape after a day or two by passing through the colon. As pointed out in the early part of this paper, the small intestine in most cases of abdominal surgery is ready to push down its contents very soon after the abdomen is closed, but the large intestine rarely acts until twenty-four or forty-eight hours later. An immediate and free escape of the contents of the small intestine from a cæcal fistula, without making it wait for the large bowel, removes all strain upon sutures or weak parts in its wall and, by facilitating vermicular movements, reduces the tendency to a formation of adhesions. Damage to any part of the large bowel would be an additional reason for making a fistula.

In conclusion, I submit for the consideration of the Section that by making a fistula in the cæcum or in some other part of the intestine in suitable cases in the course of an operation, the mortality from abdominal surgery may be reduced by a case here and there, and that patients whose lives may be saved in this way are generally suffering from an intestinal obstruction, brought about by a disturbance of co-ordination between the muscular activity of the different parts of the alimentary canal, aggravated perhaps by mechanical, physiological, or therapeutic hindrances to the flow of the intestinal contents, the colon being the part in which difficulty most frequently arises.

DISCUSSION.

Dr. AMAND ROUTH: I have had considerable experience of post-operative apyrexial tympanites but have never felt called upon to re-open the abdomen for this condition; I consider that the formation of a cæcal fistula is a drastic procedure, unless other measures have been previously adopted, such as hypodermic injection of pituitrin or better still of eserine. On several occasions I have used one of these drugs and usually in about twenty minutes after their injection, a copious expulsion of flatus and fæces has occurred, with immediate cessation of all the symptoms.

Dr. CUTHBERT LOCKYER: Mr. Malcolm has drawn attention to the problems involved in dealing with post-operative intestinal complications. Major operations on the pelvic organs should be undertaken only by those who possess a practical working knowledge of intestinal surgery. Circumstances may arise during an operation on the pelvic viscera which necessitate, first, an enterotomy for the reduction in size of distended coils of intestine; secondly, enterorrhaphy, or even enterectomy, for injured or gangrenous bowel; thirdly, colostomy, for injury to the rectum or for malignant disease. With all these procedures I am quite familiar. But Mr. Malcolm's practice of creating an intestinal fistula during a pelvic operation as a preventive measure to possible intestinal stasis, or, what is commonly called *ileus paralyticus* is, to me, quite a novel idea. I have not gathered, from listening to the paper, how a decision is reached as to when such a procedure is to be adopted. If, after this treatment, recovery takes place, how can it be shown that the happy result was in any way dependent on the "preventive" fistula? Opening the cæcum as a "preventive" to small-bowel distension is a plan the *rationale* of which I fail to understand on the other hand I have proved the value of eserine, pituitrin, and also of hormonal, in cases of distension due to inhibited peristalsis. Enterostomy when once post-operative ileus has set in, is a well recognized method of treatment, the success of which depends on early interference; in my opinion it is rarely successful after the fourth day. In such cases the distension commences in the upper part of the intestine; the opening should, therefore, be made in the jejunum.

Dr. HUBERT ROBERTS: Mr. Malcolm has made a careful record of some very difficult cases. I have recently had under my care two cases of the same nature in which the intestine was opened for post-operative tympanites. Certainly Mr. Malcolm's results seem good, but I am not clear as to whether his cases were all operated upon for post-operative tympanites, or whether some were thus treated to "prevent" this condition; if for the latter reason, in what class of case does Mr. Malcolm propose to operate? Franklin H. Martin, of Chicago, in Eden and Lockyer's "System of Gynæcology" just published, gives a very good account of this condition and has divided the post-operative obstructions into: (*a*) real obstructions; (*b*) paralytic ileus;

(*c*) peritonitis. There is a great difficulty in the diagnosis of these conditions, and it is a grave question as to whether or not in such cases the abdomen should be reopened. In real obstruction, whether immediate or late, operation is essential. In the so-called paralytic ileus, a very indefinite term, operation in certain cases is necessary if other therapeutic measures such as the administration of pituitrin, eserine, purgatives, turpentine and castor oil enemata, &c., fail. In these cases the distension of small bowel without grave constitutional symptoms is very insidious and deceptive and the whole question centres itself in arousing the intestine to act at all. If the distension gets worse, then enterotomy or enterostomy is called for in certain cases. The cause of so-called paralytic ileus is not clear. Keith's suggestion of the non-action of the neuromuscular tissue, specialized from Auerbach's plexus, situated where delays in the passage of the bowel contents normally take place (Malcolm), seems acceptable. In my opinion the action of the intestine may be in a way comparable to that of the uterus. If the uterine muscular coat is the analogue of the intestinal muscularis mucosæ, it is possible that intestine and uterus may behave in a similar way. In the uterus we are cognizant of "disturbances of polarity," vicious retraction and contraction, or complete exhaustion, as seen in post-partum hæmorrhage. Possibly paralytic ileus is due to a similar cause. Opening the intestine or abdominal drainage in peritonitis is preventable by aseptic technique, and real obstruction from adhesions is less likely to occur if greater care be taken in abdominal operations to avoid injury to bowel and to cover over all raw surfaces, especially those made in the removal of chronically inflamed appendages. Further, it is good practice in such conditions not to work in small spaces, but to make adequate abdominal incisions to "see" what we are doing. I have found it useful to give small doses of strychnia ($\frac{1}{60}$ gr.) hypodermically for one or two days before an abdominal operation, with the idea of "sensitizing" the intestines, and after the operation the sooner the bowels are made to act by purgatives or enemata, the better. In conclusion, I agree with Mr. Malcolm, that "in certain cases" either enterotomy or enterostomy gives good results, but the operation should be done directly the condition of tympanites becomes evident.

Mr. J. D. MALCOLM (in reply): Indications for making a fistula to prevent a development of tympanites are very difficult to formulate. I have been led to adopt the treatment because in certain cases of intestinal obstruction the formation of a fistula offers the best, sometimes the only, chance of saving a patient's life. The more the conditions approximate to those of an acute intestinal obstruction, the more the formation of a fistula will be found useful. The method is not one for frequent use, and in the thousand cases mentioned a fistula was made to cure or prevent the onset of tympanites in only 1·2 per cent. of the operations.[1] It was distinctly stated in the paper that it is impossible to prove that any patient who recovers with a fistula made to prevent

[1] Cases of fistula formation for acute intestinal obstruction, of anastomosis for any cause, and of colotomy for cancer, are excluded from this statement.

the onset of tympanites has recovered because of the fistula formation. It can only be said that when the conditions seemed likely to give rise to tympanites in a number of cases a fistula was made and a complete absence of all bowel difficulties followed. I have been unfortunate in my experience of the use of pituitrin and eserine, which have not always been effective. A cæcal fistula was not made when a difficulty had arisen in the small intestine, but in a large number of cases of tympanites the difficulty is in the colon. The expression "ileus paralyticus" has been used. If this means that the ileum is paralysed, that condition does not exist in the cases that are curable by making a fistula. When a patient has paralysis of the ileum nothing can save him, and when the formation of a cæcal fistula is followed by a subsidence of ileac distension, there is certainly no paralysis of the ileum. The chief object of my paper is to show that in the great majority of the cases under consideration the difficulty is due to a delayed action, but not a paralysis of the large intestine, and that peritonitis is very often a consequence and not the cause of tympanites.

(*May* 3, 1917.)

Two Cases of Primary Ovarian Pregnancy (with a Review of the Literature, 1910-17).

By Cuthbert Lockyer, M.D.

Up to the year 1909 the subject of primary ovarian gestation has been critically reviewed by several writers, but since this date, so far as I can ascertain, no systematic record of recent cases has been published. The task of deciding which cases are genuine from the large number published is no easy one, and this fact is borne out by the different conclusions arrived at by those who essayed the work prior to the year 1909. For example, Werth, in 1887, collected twelve "genuine" cases. Two years later, Leopold brought the number to fourteen, giving a short account of each case. In 1902 Füth raised the series to twenty-one; eight of these, or 38 per cent. of the total, being full-term pregnancies. Warbanoff, in 1909, collected thirty-three cases, including that of Madlener, which he described in full. So much for German statistics. We find a contrast directly we turn to American literature. Whitridge Williams, writing on the subject in Kelly and Noble's "Gynæcology and Abdominal Surgery," which was published

in 1908, accepted as authentic thirteen cases only, his series beginning with Gottschalk's case published in 1893 and ending with that of Webster, published in 1904. In addition to the thirteen "positive" cases, Williams classed seventeen as "highly probable"; eight of these had gone to term or beyond. In a third division we find five "probable" cases, four early and one late. Finally, there were eleven so imperfect as to be considered "doubtful." Thus a total of forty-six is reached, of which thirty-five were among the "positive," "highly probable," and "probable." In the thirteen "positive" cases two went to term.

In 1909 C. C. Norris concluded that there were only nineteen authentic cases published between 1899 and 1909. It will be noted that Norris adopts the English standard of excluding cases published before that of Van Tussenbroeck in 1899, and herein lies, to a great extent, the disparity between the German and American estimates, which up to 1909 are, as shown above, thirty-three to nineteen. I have commenced my series with the year 1910, so as not to overlap the previous tables, but it is quite possible that this introduces a break in the continuity and that several of the 1909 cases are excluded. I cannot form a judgment on this point, because Warbanoff has buried his 1909 statistics in the oblivion of a Munich Dissertation, which, for obvious reasons, is not accessible at the present time.

Moreover, on account of the war, it is quite probable that many cases I have had to exclude, owing to the meagre reports in current summaries, would prove worthy of inclusion among authentic cases had the original, or fuller, accounts been at my disposal. I have discovered in all forty-two cases in the period 1910-17 (March). Twenty-two of these I have classed as genuine, which, added to those of Norris, makes a total of forty-one authentic cases up to date. I cannot take Warbanoff's statistics into consideration, not having seen his article. Twelve cases have not been accepted, but have been described so that the reasons for their exclusion may be demonstrated. Of the remaining eight nothing can be stated, as the periodicals in which they appear are not at my disposal. The cases regarded as genuine, and those which have been excluded, are described in Series I and II respectively, and in each series the cases are arranged according to the year in which they were published.

Before proceeding to deal with the published cases, I will record two examples which have been met with in my own practice:—

CASE I.

F. H., a married woman, aged 23, was sent to the Gynæcological Department of Charing Cross Hospital on July 14, 1915, by Dr. Vernon Muller, complaining of metrorrhagia and pain in the lower abdomen. She had borne two children, the younger being aged 1, and subsequently there had been an abortion (seven months before admission). The catamenia for the past six months had been regular; every four weeks, lasting from five to seven days, until three weeks ago, when the flow appeared one week before time and had continued ever since. For seven weeks there had been continuous pain in the lower abdomen and in the sacral region; this was sharp and stabbing in character, worse on movement and after food, and considerably aggravated during the periods. There was slight leucorrhœa. Micturition was normal. The bowels were regular. The patient had suffered from rheumatism in the right shoulder, left knee, and wrists.

Mr. Gordon Ley, the registrar, examined the patient in the Out-patient Department, and noted "cervix points to the left and behind; it is soft and a little patulous. Uterine body small, just a little mobile, and lies to the right. There is a mass the size of a large tangerine orange in the posterior fornix; it is tender, soft, and doughy; it moulds the pelvic floor and is fixed. Diagnosis: Left pyosalpinx." On September 4 the patient was admitted into the gynæcological ward under my care. I found a tumour in the left posterior quarter of the pelvis which was semi-fixed and tender. I regarded it as a tubo-ovarian inflammatory mass and proceeded to treat it with hot douches and electric-heat baths. This was continued for about five weeks, when, finding that there was very little, if any, reduction in the size of the swelling, I decided to operate.

On October 19 I opened the abdomen, and, with Mr. Gordon Ley's assistance, removed the left Fallopian tube and the enlarged left ovary. The opposite appendages were adherent. The abdominal end of the right tube was sealed. On this side I freed the adhesions and made an artificial ostium in the tube. Finally the uterus was suspended by Gilliam's method. There was no free blood in the peritoneal cavity. During the operation, as soon as the left ovary was brought into view, I remarked to Mr. Ley that the lesion appeared to be an ovarian molar pregnancy, so remarkably did it resemble the specimen published by Dr. Giles and myself in 1914.

Pathological Specimen.—This consists of the left Fallopian tube attached to the left ovary by its mesosalpinx. After hardening in Kaiserling-Pick's solution the following observations were made: Left tube measured 7 cm. in length and 1·2 cm. in diameter. To its outer coat were attached a few filmy adhesions. The abdominal ostium was closed, but the ampullary end was not dilated. The fimbriæ were indrawn, so that the tube ended in a depressed dimple. Attached to the

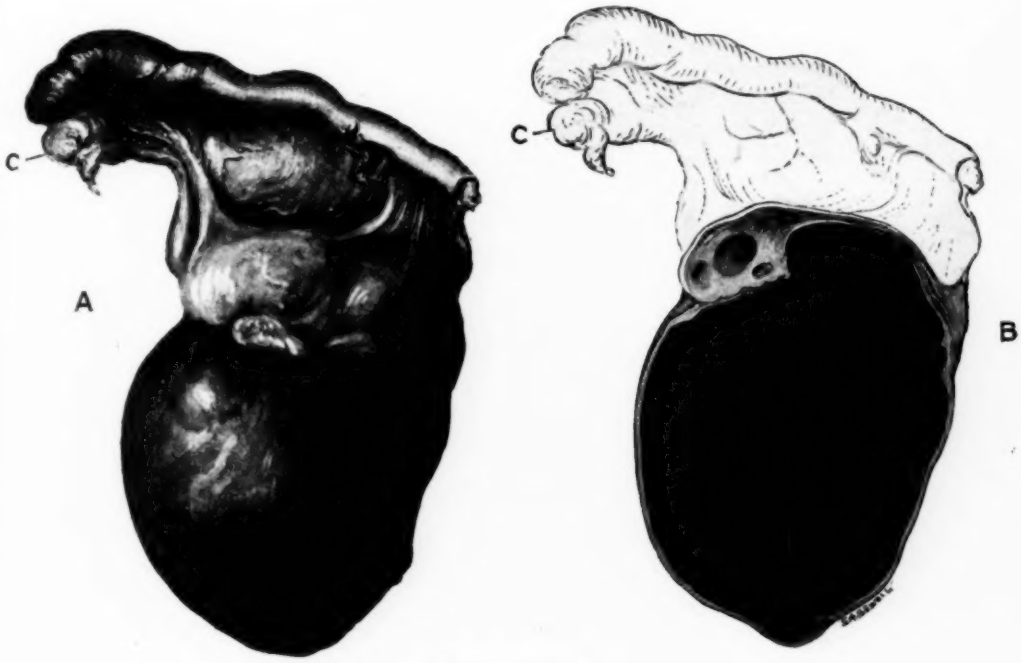


FIG. 1. (Case I.)

Primary ovarian pregnancy. **A**, posterior view of external aspect; **B**, mesial section through ovary and gestation sac; **C**, hydatid of Morgagni with adhesions. (Natural size.)

abdominal end was a hydatid of Morgagni, and to this adhered some tags of adhesions resembling fimbriæ of the tube (*see* fig. 1). The mesosalpinx was translucent; it separated the tube from the ovary by a vertical distance of 1·7 cm. The left ovary with its contained hæmorrhagic cyst measured 7 cm. in its vertical and 4·8 cm. in its transverse

diameter. It is divisible into an upper (apparently) solid portion measuring 2·5 cm. by 4·3 cm. and a lower cystic portion measuring 4·5 cm. by 4·8 cm. These two portions are sharply demarcated by a constriction, or waist, so that the cystic lower portion fits into the upper solid part like an inverted acorn in its involucre (*see* fig. 1, A). On bisection of the ovary, what had appeared to be solid ovarian tissue was hollowed out centrally, due to invasion thereof by the blood-cyst situated below. By hardening, the hæmorrhagic contents of this cyst had become a firm laminated clot in which were several irregular crevices due to shrinkage

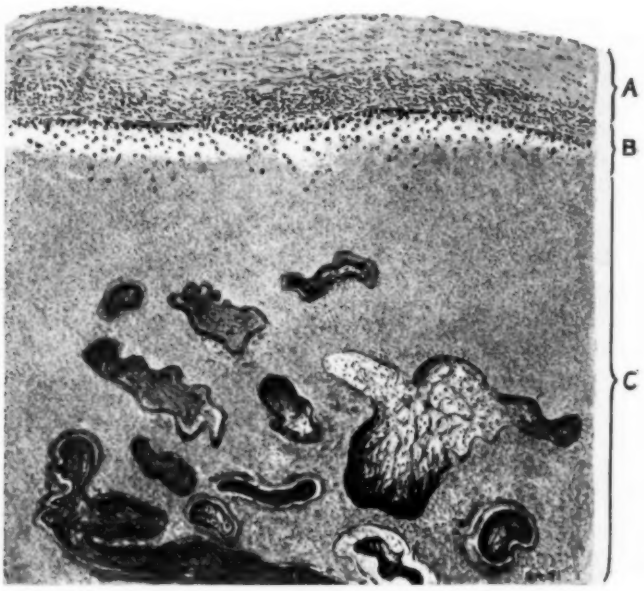


FIG. 2. (Case I.)

A, ovarian cortex; **B**, degenerate lutein cells; **C**, ovarian molar pregnancy (× 105.)

(*see* fig. 1, B). The ovarian stroma which capped the clot presented several atresic follicles, but no yellow tissue was seen on cross-section. The capsule of the clot was not ruptured, it consisted of compressed ovarian stroma which, in thickness, measured from 0·2 cm. at its upper pole (where it was attached to the mesosalpinx) to 0·1 cm. at its lower free pole. Sections of the tube showed evidence of a mild degree

of chronic salpingitis (shown by epidiascope), the plicæ were swollen, but were not adherent at their tips; there were no pseudo-follicles and no invasion of the musculature by mucosal epithelium. On the uncovered (lower) aspect of the tube the vessels were dilated and engorged. There was no evidence of fœtal tissue to be seen anywhere. Many sections of the ovary were cut and stained with logwood and eosin and also by van Gieson's method. Those taken from the lower pole of the blood-clot showed nothing of importance excepting the fact that the capsule consisted of ovarian tissue. Those from the upper half were more interesting. They showed (1) that the ovarian stroma contained (*a*) numerous dilated and thrombosed blood-vessels, (*b*) that as the blood-clot was approached the stroma became degenerate, but still clearly demonstrated the presence of layers of lutein cells intermingled with fibrinous material (*see* fig. 2, A and B). (2) Within the clot were seen chorionic villi for the most part very degenerated but showing, in some cases, strands of trophoblast bounding a fibrous core (*see* fig. 2, C). These findings clearly prove this to be a primary ovarian pregnancy which has resulted in the formation of an unruptured ovarian mole.

Case II.

E. P., married, aged 33. The patient was sent to me on April 9, 1916, by Dr. Hugo, who kindly supplied the following notes: "Seen on June 17, 1915. Previous history" (taken in 1915, ten months before I saw the patient): "Three children, youngest aged 5, and one miscarriage three years ago, since when the periods have been regular. Present attack (June, 1915): Last night, after getting into bed at 11 p.m., the patient was seized with very acute pain in the right iliac region shooting into the leg, which caused her to draw up the leg to obtain relief. It was described as 'worse than having a baby.' The last period was five weeks ago, that is, she was one week overdue. The last 'period' was preceded by the passage of clots. The pain lasted all night, but the patient got up this morning, though feeling very ill on account of pain. There had been a slight show yesterday, i.e., June 16. There was no retching nor vomiting, the bowels were regular, pulse and temperature normal. Bowels opened this morning without influencing the pain. No pain on passing water. Examination: No limitation of respiratory movements of abdomen. Tenderness on pressure over right iliac region. *Per vaginam* considerable tenderness. Ring pessary removed. Uterus retroflexed, fundus enlarged and tender. In the right broad ligament a

very tender swelling could be felt. I wished her to go to the Cottage Hospital for operation, but she would not consent, so I treated her at home, keeping her in bed until the end of July, 1915, by which time all acute symptoms had subsided. On March 11, 1916, the patient had a subacute attack, after which I persuaded her to go and see you. She gives a history of having had rheumatic fever which affected her heart."

On admission to the Samaritan Hospital on April 9, 1916, the patient said that she was seized with abdominal pain five weeks ago. The pain affected chiefly the right side, and was accompanied by irregular bleeding. She stated that her periods had been regular until the last attack of pain. Temperature 99·4° F., pulse 88. Heart and lungs normal. *Per abdomen* some resistance in the right iliac region. *Per vaginam* cervix forwards, uterus retroverted, tender and fixed. Appendages thickened on both sides. A mass the size of a tangerine orange to the right of the uterus. Left ovary slightly enlarged. Diagnosis: "Fixed retroversion, double salpingo-oöphoritis, ? right-sided pyosalpinx."

Laparotomy, April 19, 1916, assisted by the Matron, present Mr. Malcolm. There was no free blood in the peritoneal cavity. To the right of the uterus lay an enlarged ovary, which was adherent in the right posterior quarter of the pelvis, but which was brought up without difficulty. I showed it to Mr. Malcolm, who was looking on, and expressed the opinion to him that it would prove to be an ovarian gestation. The uterus was not adherent as was previously thought. The left ovary was cystic, and its lower half, containing two cysts, was resected. Both Fallopian tubes appeared to be quite normal. The right tube and enlarged right ovary were removed.

Pathological Specimen.—This consists of the right Fallopian tube, mesosalpinx and right ovary, and also the lower half of the left ovary. The tube is somewhat thickened and tortuous (*see* fig. 3, U). Its ostium is patent and the fimbriæ free and somewhat succulent. The tubal segment measures 5 cm. in length and is 1·2 cm. in diameter at its ampullary portion, 1 cm. in diameter through the isthmic part. The mesosalpinx is contracted but translucent; along its outer margin (infundibulo-pelvic fold) runs the ovarian fimbria to the surface of the ovary. The maximum vertical measurement of the mesosalpinx is 1·5 cm. The ovary measures 5 cm. by 4 cm. The upper part (2 cm. by 3 cm.) is comprised of solid ovarian tissue which caps, or overlies, the lower segment (3 cm. by 4 cm.), which consists of a blood-cyst (*see* fig. 3, A). The latter is very sharply differentiated from the former, not only by

its colour and by the smoothness of its capsule, as compared with the furrowed pale surface of the solid part of the ovary above it, but also, by the fact that a demarcating sulcus separates the solid from the cystic part. On hemisection, after hardening in preservative, the upper solid stroma presents numerous corpora albicantia, and an atresic follicle 1 cm. in diameter. The cystic portion is enclosed in a capsule made up of ovarian stroma which shows no sign of having ruptured. On microscopic examination the conditions revealed are very similar to those of Case I—i.e., the ovarian stroma shows numerous dilated and

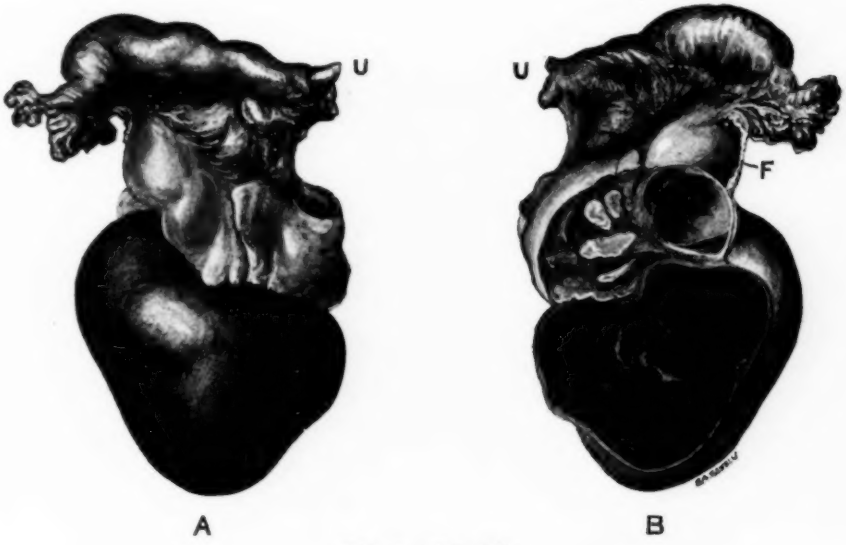


FIG. 3. (Case II.)

Primary ovarian pregnancy. **A**, anterior view of external aspect; **B**, mesial section through ovary and gestation sac; **U**, uterine end of tube; **F**, ovarian fimbria. (Natural size.)

engorged blood-vessels (*see* fig. 4, A) which lie outside a necrotic zone in which the remains of a ripe corpus luteum are still visible—but here and there is seen a definite invasion of ovarian stroma by chorionic villi, one of which appears within a blood-vessel as an instance of "verschleppung." Within the clot itself is seen the lining of the chorio-amniotic sac with large branching villi proceeding from the chorionic membrane (shown by epidiascope), but no fœtus could be found.

The gestation sac and the escaped blood are enclosed in a cyst, the inner lining of which is composed of degenerate lutein cells (*see* fig. 4, B). The villi are old and degenerate (fig. 4, C), but some of them still show the remains of a syncytial investment. Section of the Fallopian tube

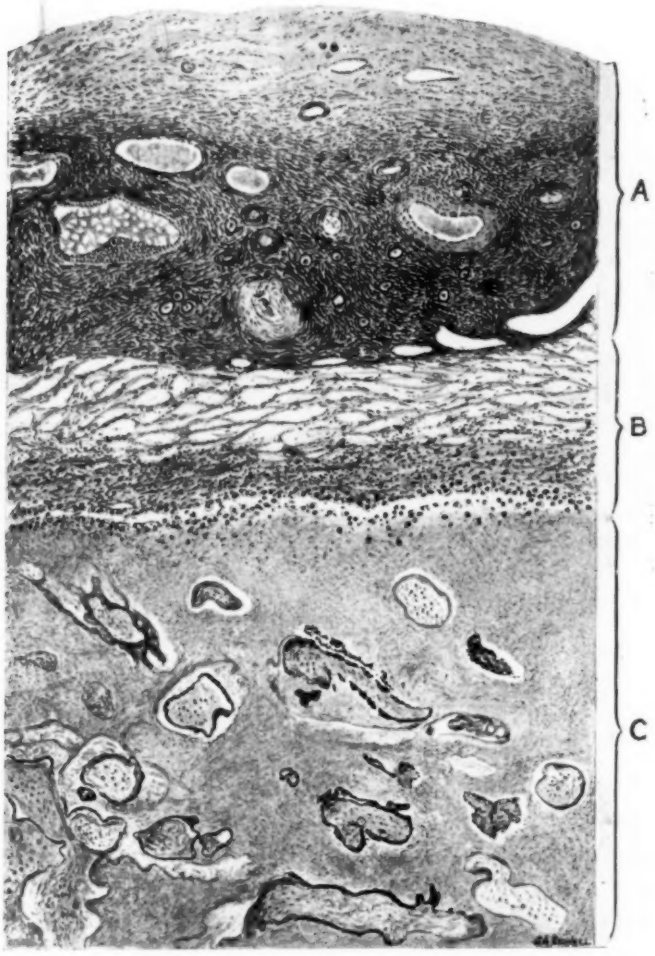


FIG. 4. (Case II.)

A, cortex of ovary; **B**, wall of corpus luteum; **C**, ovarian molar pregnancy. (× 105.)

showed no evidence of mucosal inflammation; there was some œdema of the muscular coat, otherwise the tube was normal (shown by epidiascope).

It is an interesting fact that this patient gave birth to a full-term child in March, 1917, eleven months after the removal of the right ovarian pregnancy, because, as previously stated, one half of the opposite ovary was resected on account of its cystic condition.

Series I.—Accepted Cases.

1910.

Barrows, C. C. "Ruptured Ovarian Pregnancy," *American Journal of Obstetrics*, New York, 1910, lxii, pp. 1078-1081.

Patient married, aged 25. Menstruation irregular for eight months; not excessive, no clots, no membrane. No pain. No change in breasts. Admitted September 26, 1910. Twenty-four hours before admission she became nauseated and fainted after lifting a heavy tub. Severe pain in lower abdomen. Ruptured tubal gestation diagnosed. Abdominal cavity full of fluid blood and a few clots. Both tubes and left ovary were normal. Lying below the right tube was the ovary the size of a large walnut. There was a rupture of the ovarian wall and protruding from this was a clot. Report of A. J. Elsen, of Cornell University Medical College: "Specimen N3,678 consists of a tube and a portion of ovary, also a blood-clot, which is irregularly spherical in shape, about the size of a walnut. The clot appears to be partially surrounded by a capsule and at one point it shows tissue closely resembling placental structure. The tube is normal in size and appearance. Microscopical examination reveals numerous chorionic villi. The capsule surrounding the blood-clot shows that it is made up of ovarian structures. Diagnosis: Genuine ovarian pregnancy."

Lea, A. W. W. "A Case of Ovarian Pregnancy with Diffuse Intraperitoneal Hæmorrhage," *Journal of Obstetrics and Gynæcology of the British Empire*, 1910, xviii, pp. 182-187 (three coloured plates).

Patient, aged 29, married two years. On December 31, 1901, she was seized with severe abdominal pain, vomiting and faintness. Extreme pallor. Menstruation normal, last period three weeks previously, no vaginal hæmorrhage. Indistinct fullness in pouch of Douglas. Large quantity of blood in abdomen. Tubes normal. Left ovary contained a thin walled blood-cyst which had ruptured and collapsed. The greater part of the ovary was normal, but a portion of the cortex was expanded to form the investment of a coagulum. The latter contained chorionic villi. Sections of tube showed it to be normal. Section of the ovary showed dilatation of blood-vessels, hæmorrhage in the

stroma and a moderately thick layer of lutein cells. In the wall of the blood-cyst were corpora albicantia and a Graafian follicle.

TWEEDY, E. HASTINGS. *St. Bartholomew's Hospital Reports* (1910), 1911, xlvi, p. 249.

The clinical report reads: "Ovarian pregnancy removed by operation from a woman who had borne three children and had aborted once. Her youngest child was born in June, 1909. On September 8 following she menstruated once, but there was no further loss. On November 5, seven weeks after the last menstrual period, she was seized with acute pain in the right hypogastric region. Examination revealed a tender mass the size of a hen's egg, lying to the right of a retroverted uterus, and a diagnosis was made of extra-uterine pregnancy. The patient refused treatment at the time, but returned ten days later giving a history of continuous vaginal hæmorrhage and intermittent pain since her previous visit. On November 16 laparotomy was performed. Both Fallopian tubes, together with the left ovary, were found to be healthy. The right ovary was obscured by a blood-clot attached to the ovarian ligament on this side. It was removed in its entirety, the tube which ran above it being left intact." St. Bartholomew's Hospital Museum report, No. 3,077, E: "Part of an ovary showing the effects of an ovarian pregnancy. In the recent state the specimen was an elliptical mass measuring 3 in. in its long diameter. This mass has been longitudinally divided and is seen to consist of two parts, a small area of dense tissue surrounded on three sides by blood-clot. The dense tissue again falls into two subdivisions. The greater part of it has the appearance of connective tissue, and contains blood-vessels visible to the naked eye; but at its lower extremity, as seen in the preparation, it is separated from the mass of blood-clot by a narrow tapering band of a yellower material. This last is a corpus luteum. Into it occurred the bleeding which subsequently ruptured the ovary and produced the enveloping clot. Microscopic examination shows that the dense tissue is ovarian stroma and the tapering band corpus luteum. No chorionic villi can be seen, but sections made from the other half of the specimen contain them in abundance." There is a good drawing in the *Proceedings of the Royal Society of Medicine*,[1] and the report of the Pathology Committee was confirmatory.

BONDI, JOSEF. *Wiener klinische Wochenschrift*, 1910, p. 774.

Patient, aged 22, married February 10, 1909. Period on February 25, also on March 25. Period missed in April. May 19: Severe hæmorrhage for four days; thought to be an abortion by family doctor. June 9: More hæmorrhage; no marked pain. Admitted on June 20 (operation June 21). Behind the uterus on the right side was a movable sensitive tumour the size of a fist; a rounded cord running over the tumour was felt. Operation, June 21:

[1] *Proc. Roy. Soc. Med.*, 1910, iii (Sect. Obst. and Gyn.), p. 132.

Omental adhesion easily separated, no escaped blood. Right ovarian tumour held by loose adhesions. Opposite adnexa: Left tube very tortuous and short. Left ovary small, no corpus luteum. The specimen consisted of the right tube and ovary; right tube tortuous, but it was normal microscopically; its fimbriæ were free. The ovary was the size of an orange; its outer surface showed adhesions. On section a cavity the size of a nut was found in the middle of the ovary. Attached to its wall was a portion of umbilical cord 5 mm. long and a fœtus 7 mm. in length. Microscopically the walls of the cavity showed no tissue characteristic of a Graafian follicle. The inner layers of the cavity consisted of the amnion and chorion. Two good drawings.

1911.

HOLLAND, EARDLEY. "A Case of Ovarian Pregnancy, probably Bilateral," *Journal of Obstetrics and Gynæcology of the British Empire*, 1911, xx, pp. 295-298.

Patient, aged 40, married twelve years. No children, one miscarriage some years previously. Admitted in March, 1909. In October, 1908, after six weeks amenorrhœa, abdominal pain and uterine bleeding. Menstruation regular during December, January, February. March 6: Another "attack." Diagnosed as ectopic pregnancy. Recent clot in pelvis; right ovary enlarged and adherent; it bled freely when drawn up. The left ovary was not enlarged, but on its posterior surface was an oozing rent with a small clot protruding. On section of the right ovary it was seen that a capsule of ovarian tissue enclosed a blood-clot, in the centre of which lay a gestation sac enclosed by chorionic membrane and an incomplete amnion. The amnio-chorionic stalk united it to the chorionic membrane. Remnants of trophoblast on membrane. No villi, no embryo. Left ovary: Curious "plasmoidal" cells found on section of projecting clot; ? nature.

YOUNG, E. B., and RHEA, L. J. "Ovarian Pregnancy: Report of a Case," *Boston Medical and Surgical Journal*, 1911, clxiv, p. 264.

Patient, aged 27, single. Catamenia regular before present illness. Sent to Young in October, 1908, as a probable case of fibrosis. Present illness (February, 1908): Menorrhagia; curetted, but loss continued until the end of September. Vaginal examination: Slight bloody discharge, uterus retroverted, tubes somewhat adherent, and an enlarged left ovary. The appendages and uterus were quite movable, and the old inflammatory process seemed quiescent. Ovarian cyst diagnosed. Operation: Extensive old pelvic inflammation; when adhesions were separated recent blood-clot was found in left side of pelvis; it completely surrounded the ovary. Fimbriated end of the left tube closed, and adherent to bottom of pelvis; the rest of the tube was free except for slight adhesions to bowel. Section of the left ovary after hardening in 10 per cent. formalin revealed a small embryo "lying in a rounded space"

(Young). A very convincing pathological report is furnished by Rhea, who describes the microscopic finding of chorionic villi and dilated blood-vessels in the ovarian stroma, but he does not refer to the embryo mentioned as being found by Young, who had incised the ovary some time previously in the presence of Dr. H. C. Low (pathologist). The only criticism I would offer is that the tube was unfortunately very close to that part of the ovary which illustrates the presence of villi, as seen in micro-section No. 1, but the lumen of the tube shown in the same section appears normal.

1912.

BANKS, A. G. "A Case of Ovarian Pregnancy," *Journal of Obstetrics and Gynæcology of the British Empire*, 1912, xxi, pp. 216-219.

Patient, aged 24, married five years. Two children, younger aged 2. Admitted for hæmorrhage of six weeks' duration. *No period had ever been missed.* Last period ceased on October 7. October 24: Hæmorrhage without pain continued daily for one month. November 11: The first of three attacks of severe pain occurred. Admitted on November 27 with labour-like pain and increased hæmorrhage. In Douglas's pouch, to the right, was a tender doughy mass, the size of a Seville orange. Right tubal pregnancy diagnosed. Not more than *half an ounce of fluid* blood in peritoneal cavity. Tumour easily brought to view; one or two thread-like adhesions. Tumour was ovarian, its tube was normal. Oöphorectomy, with segment of ovarian tissue retained. Recovery. Ovary contained a fœtus (good drawing shown) of about eleven weeks' development. The cord was attached to the thinned-out free lower pole of the cyst—i.e., the part opposite to the main solid mass of ovarian tissue. No "decidual reaction" in the ovary, but some multinucleated masses seen in the stroma: these were probably of trophoblastic origin. The villi present were old and degenerate.

GRAHAM, G. S. "A Case of Ovarian Pregnancy," *Journal of Medical Research*, 1912-13, xxvi, pp. 499-512.

Graham, as assistant professor of pathology in the Dartmouth Medical College, met with this specimen in the laboratory. Who the surgeon was is not stated. The patient, aged 36, was a 2-para; the younger child was born four years ago. No miscarriages. Menstruation regular until a month ago, since when constant menorrhagia. A little more than a month ago there was a severe attack of right-sided abdominal pain, and vomiting; the attack lasted four days. *Per vaginam* a small tumour palpable in region of left ovary. Left tube and ovary and appendix removed. Recovery. Laboratory report: "The specimen consists of an ovary with its Fallopian tube. The tube measures about 5 cm. in length, and about 0·5 cm. in its greatest diameter. Beyond an injection of the superficial blood-vessels no evidence of pathological change can be determined. The fimbriated end is patent. The ovary is about the size of

a hen's egg, reddish in colour, and rather soft. Its surface is irregularly furrowed or lobulated, and appears free from adhesions. Section reveals a cavity about 1·5 cm. in diameter placed excentrically within the ovarian mass, and bounded by a thin white membranous wall of rather firm consistence. The tissue around this cavity is soft, reddish in colour, and strongly suggests placental tissue. In its thicker portion is a corpus luteum, which borders upon the soft reddish mass and appears to be blended with it." Microscopic description: The Fallopian tube — Most careful description by the author shows that there was no evidence of old or recent inflammation. The ovary —Outer zone of typical ovarian tissue, then a "transition-zone" bounding a third innermost zone, within which were numerous chorionic villi, with intervillous spaces containing red disks, fibrin, trophoblast-elements, and leucocytes. The inner margin of this zone is defined by the fœtal membranes.

The paper is an admirable piece of histological work, and the theories as to how the impregnated ovum secures implantation in the ovary are well debated.

LENDON, A. A. (Lecturer on Obstetrics, University, Adelaide). *Australasian Medical Gazette*, 1912, xxxi, p. 492.

Lendon's article (which appears in the above gazette on the next page to Verco's) begins with the statement that he (Lendon) does not think that any genuine cases of ovarian pregnancy have yet been published in South Australia, and he proceeds to mention a case of Rothwell Adam, of Melbourne, but does not refer to Verco's case; perhaps exception was taken to the latter on the ground that no histological facts were forthcoming.

Dr. Lendon's case: Patient, 1-para, aged 25, married one year nine months. Partus on April 11, 1911; last menstruation, September 8 to 12. Pain in lower abdomen after defæcation was complained of on October 20. On October 29 the patient fainted. Examination revealed a swelling in Douglas's pouch. Dr. de Crespigny has, though at first reluctantly, conceded that it (the specimen) fulfils all the postulates of a true ovarian pregnancy. He writes: "The specimen consists of the left Fallopian tube, the ovary, and a part of the broad ligament. The tube is quite free from adhesions; it is slightly hyperplastic, but shows no sign of pregnancy, either macroscopically or microscopically. The ovary is considerably enlarged, white and tough. Adherent to its outer aspect and partly infiltrating its tissue, is a mass about 5 cm. by 2 cm. by 3 cm., with a rough, shaggy surface and a rather friable, reddish texture, apparently consisting mainly of blood-clot. After hardening, an incision was made through this mass and continued so as to hemisect the ovary. That the mass partly invaded the ovary was evident. Furthermore, a recent corpus luteum is present in the ovary, and instead of being in the form of a ring, the yellow layer of the corpus luteum is a curved line partly enclosing the pregnancy and separating it from the rest of the ovary. The appearance is as though the corpus luteum were ruptured by the growth of its contents. Microscopic examination shows this mass to consist of chorionic

villi invading blood-clot. There are also dilated venous spaces such as are found in the pregnant uterus. The fœtus was not seen."

VERCO, W. A. "Ovarian Pregnancy," *Australasian Medical Gazette*, 1912, xxxi, p. 491.

The author gives a brief history of the subject, then relates a case of ovarian pregnancy. The patient, aged 32, was a 3-para. Pain in the lower abdomen was thought at first to be due to appendicitis, but the history of a period being four to five days late raised the possibility of an extra-uterine fœtation. At operation there was found a blood-stained discoloration of the omentum. No decidual cast. The right ovary was nearly as large as a turkey's egg, and "had hæmorrhage into its substance." There was no twisting of its pedicle to account for this. The right ovary was removed, but its Fallopian tube, being healthy, was retained. The left ovary and tube were healthy. On incising the ovary "a small fœtus was seen in a cavity occupying the centre of the ovary and was hanging by the tiny umbilical cord attached to the placenta." There is no histological record.

In this case there was probable bleeding into the ovary during the fourth week of pregnancy, when the first attack of pain occurred. Size of fœtus not given.

1913.

CHIENE, GEORGE. "Ruptured very early Primary Ovarian Pregnancy." *Obstetrical Transactions*, Edinburgh, 1913, xxxviii, p. 96.

Patient, married, aged 34, 6-para; youngest child aged $2\frac{1}{2}$. Admitted as a case of acute appendicitis requiring immediate operation. Menstruation quite regular, expected next period three days after admission. Temperature 99·4° F., pulse 120. Pain, especially in right iliac fossa and through to back. There was no vaginal hæmorrhage. *Per rectum* distinct fullness in pouch of Douglas. Fainting attacks shortly before admission. Peritoneal cavity full of blood. Right tube normal. Bulging mass size of a cherry protruded from inner pole of right ovary $2\frac{1}{2}$ in. away from fimbriated end of the tube (good coloured drawing of this). The ovary was removed but not the tube. Left tube and ovary normal. Appendix normal. Photomicrograph shows ovum lying completely surrounded by ovarian stroma.

GRIMSDALE, T. B. "Case of Ovarian Pregnancy with Full-time Fœtus." *Journal of Obstetrics and Gynæcology of the British Empire*, 1913, xxiii, pp. 115-117.

Patient, aged 22, married three years, 3-para. Admitted on February 19, 1912, with abdominal swelling of three years' duration. Last confinement two months before admission. Says she "never became small" after her first child was born. No pain, except at times on micturition. Mobile mass reaching to

umbilicus, not tender; uterus lay to the right of tumour and free from it. Solid ovarian tumour diagnosed. At operation: Tumour freely movable and its relations clearly demonstrated. It was delivered whole through the abdominal wound and was then seen to be the left ovary with its tube and mesosalpinx quite normal. The left round ligament was seen to arise from the left horn of the uterus and to lie well to the right of the tumour proving that it was not an ill-developed left horn of the uterus. The left ovarian tumour contained a fœtus and a placenta. X-rays established that the fœtus had reached full term. *No history of amenorrhœa* apart from that relating to the two uterine pregnancies. No history of pain to suggest rupture of the sac with hæmorrhage at any time or a spurious labour.

The case appears to be one of coincident ovarian and uterine pregnancy.

HANNES, W. "Ovarian-Gravidität," *Zeitschrift für Geburtsh. und Gynäkologie*, 1912, lxxii, pp. 269-271.

This is a well illlustrated account of primary ovarian gestation with five coloured pictures (one macro- and three microscopic drawings). The patient was a 3-para, aged 27. The last period was on January 21 to 25, 1912. A fortnight later hæmorrhage, which lasted for a week, set in. It ceased for a week and occurred again lasting three to five days; it was accompanied with pain in abdomen and spine. Admitted on March 9, 1912. Uterus elevated and anteposed. Behind the uterus was a tumour the size of a small fist, filling up the pouch of Douglas and connected with the right adnexa. Tubal gestation diagnosed. The abdomen contained a fair amount of dark fluid and clotted blood. The right tube was empty and its fimbriated end "free." The swelling was ovarian. Recovery. The tube was quite separate and distinct from the ovary. The latter on section showed a small amniotic sac containing a tiny fœtus. The sac was surrounded with clot which in its turn was encapsuled by ovarian tissue. Blood-clot and penetrating villi were present in the stroma. A Graafian follicle is depicted, but no ripe corpus luteum was present.

MCCANN, F. J. "Primary Ovarian Pregnancy at the Fourth Month," *Proceedings of the Royal Society of Medicine*, 1913, vi (Section of Obstetrics and Gynæcology), p. 229.

Patient, aged 32, mother of one child, aged 5½. Regular periods ceased on February 10, 1912; subsequently acute attacks of pain and vomiting on left side of lower abdomen. Large cystic tender swelling up to umbilicus. Enlarged uterus in front and to right. Diagnosis: Cyst of ovary complicating early pregnancy or ectopic gestation. Old blood-clot in lower abdomen. Tumour was a left ovarian cyst, its Fallopian tube was normal. The ovarian cyst consisted of two loculi, containing adenomatous growth; separated from these by a septum (containing other small cystic cavities showing adenomatous tissue) was a third cavity 4 in. in diameter; in the latter was a fœtus and placenta of about four months' development. A good drawing is shown.

MALL, F. P.; CULLEN, E. K. "An Ovarian Pregnancy located in the Graafian Follicle," *Surgery, Gynæcology, and Obstetrics*, 1913, xvii, pp. 698-703.

Patient, aged 24, was admitted on June 1, 1910, with diagnosis of appendicitis. Paroxysms of pain, general throughout the abdomen, more acute on left side, worse on movement, occasional vomiting. Abdomen filled with blood; both tubes intact; right ovary swollen and bleeding. Finney, the operator, said: "The whole process was so definitely confined to the ovary that it seemed clinically to be a definite case of ovarian pregnancy." Recovery. Specimen: Tube and ovary from right side. Tube somewhat tortuous, few adhesions on the surface; mucosa somewhat thickened and blood-tinged. No gross evidence of a tubal pregnancy. Mucosa normal in microscopic sections. Ovary measures 5 cm. by 4 cm. by 3½ cm. Few old adhesions on surface. Transverse section through the ovary at the point of rupture of the follicle is shown in a drawing by Max Brodel. It shows a central cyst containing a clot. This clot was covered by an inverted chorion and contained villi. The cyst was lined by yellow lutein cells. The authors say it shows conclusively that the ovum had lodged itself in a Graafian follicle, undoubtedly the one from which it came, indicating that the sperm must have entered the follicle after it had ruptured. The fertilized ovum then found lodgment in the follicle, around which a corpus luteum developed.

OLDFIELD, C. "Ovarian Gestation," *Journal of Obstetrics and Gynæcology of the British Empire*, 1913, xxiii, pp. 41, 42 (2 plates).

Patient, aged 37, married twelve years; no previous pregnancy; no previous menstrual irregularity. In May, 1912, when the period was seventeen days late, sudden pain and vaginal hæmorrhage. June 24: Fainted in bed, but no pain. Hæmorrhage started again, lasting three to four days. June 26: Fragments of decidua passed. Abdomen not tender, fullness in posterior fornix. Under anæsthesia: Left ovary a little larger than usual. Dark fluid blood in abdomen and clots; amongst these a mole was found. Left tube and ovary removed. Right appendages normal. Left tube normal. Left ovary a little larger than usual, presented a saucer-shaped depression from which blood oozed; deep to this, the ovarian tissue contained large blood-sinuses as in an early uterine pregnancy; amongst them, in the stroma, was a chorionic villus, and Nitabuch's fibrin-layer between fœtal and maternal structures was demonstrated "so that there is no doubt it was on this surface that the ovum was implanted." The mole contained an amniotic sac with the blastoderm extruded and lying attached to the surface of the mole.

SENCERT, L., and ARON, M. "Une curieuse observation de grossesse ovarienne," *Bulletin de la Société d'Obstétrique et de Gynécologie de Paris*, 1913, ii, pp. 830-835.

Patient, aged 23, the wife of a colleague. Married in February, 1911; thought to be pregnant in April; only a slight show after this, but much

abdominal pain, especially in the left iliac region. The left fornix was tender, but no swelling was felt. Later, the left ovary was found to be enlarged. Patient demanded operation because of continuous pain. Laparotomy June 23, 1913: The left ovary was the size of a fresh walnut (noix fraiche) and dark in colour, and evidently very "altered." Uterus small. Right appendages normal in form but somewhat atrophic. The left ovary was removed; it was the size of a "mandarin," hard and irregular. At its larger pole was a rounded swelling, 2 cm. in diameter, like a corpus luteum, but in its centre was attached a yellow stalk 5 mm. long, 1 mm. thick. This proved to be an umbilical cord (mucous tissue, one artery and two veins), and the swelling in the ovary was proved microscopically to be placental tissue. A full account of the histology of this case is given, and although the tube was not removed there can be no doubt about the site of the embedding being in the ovary. No fœtus was found.

In the *Revue de Gynécologie*, 1914-15, xxiii, pp. 1-16, there are several interesting drawings and diagrams illustrating the features of the placenta in the above case. They show that the peripheral zone of insertion is occupied by a network of plasmodi-trophoblast to which the authors give the name of "Ectoplacenta." Outside this network are large blood spaces in the ovarian tissue, whilst the lacunæ between the plasmoidal tissue are filled with maternal blood. The embedding, therefore, shows the appearances which are now known to be typical in cases of normal intra-uterine pregnancy of about the second week, as seen in the Teacher-Bryce ovum.

1914.

CATURANI, M. "Ovarian Pregnancy with Report of a Case," *American Journal of Obstetrics*, 1914, lxix, pp. 409-428.

Patient, aged 30, married twelve years; three children, the last five years ago; five abortions, the last four years ago. Infected with syphilis and gonorrhœa. Acute attack of gonorrhœa fifteen months before first ectopic pregnancy. In June, 1909, Caturani operated for right tubal pregnancy. The tube and ovary of right side were removed. In January, 1912, missed her period for a few days. On January 10 severe abdominal pain and a show of blood. On January 24 abdomen distended and tender but no mass felt. *Per vaginam* mass to left of uterus. Slight dark bloody discharge. Operation fifteen days after initial attack. Abdomen contained fluid and clotted blood, no distinct hæmatocele. Fimbriated end of tube open. No trace of rupture in the tube. The ovary on its distal pole was apparently damaged and covered with clotted blood. The tube was removed, and with it almost a third of left ovary (i.e., the distal pole) was resected. At the operation the case was regarded as a tubal abortion and an ovarian hæmatoma from rupture at the site of a follicle. The real nature of the specimen was not found until microscopical examination was made. Gross specimen: This is made up of the left tube and part of the left ovary still attached to the tube by a slight band of

tissue. The tube is 7 cm. long. No marked enlargement, no kinks, no traces of rupture. The ampulla was thickened and hard. The fimbriated extremity was free from adhesions, but traces of blood were found adherent to some fimbriæ and to the external surface of the tube. Microscope revealed chronic salpingitis at ampullary part, but no evidence of fœtal elements. A thorough microscopic investigation of the resected ovarian segment was made and a full account is given. The central area was composed of blood, fibrin, and chorionic villi. There was an "attempt at decidual formation along the margin of the ovum"—i.e., in the boundary between the fœtal and ovarian tissues. The fœtal elements lay in intimate relation to a corpus luteum, which Caturani says might be regarded as a proof of the embedding of the ovum in the follicle. Eleven cases out of nineteen reported by Norris showed relation of ovum to corpus luteum.

Caturani attributes the causation of ovarian gestation to peri-oöphoritis. He may be right, but his arguments are very theoretical. Thirteen figures help to explain the text.

GILES, A. E., and LOCKYER, C. "A Case of Ovarian Pregnancy." *Proceedings of the Royal Society of Medicine*, 1914-15, viii (Section of Obstetrics and Gynæcology), pp. 2-10.

Patient, aged 32, married four years; no children. Consultation in June, 1906, to ascertain cause of sterility. Good health; menstruation regular, except that a period was missed six months before consultation. Uterus normal in size and position. Rounded swelling the size of a duck's egg to right of uterus. Tumour was an unruptured ovarian hæmatoma of the right side. Right tube normal. Hæmatoma contained a large chorio-amniotic sac and many chorionic villi. No embryo found. No corpus luteum. Ovarian stroma contained many large engorged blood-vessels and showed decidual reaction in the medulla and cortex.

KÖHLER, ROBERT. *Gynäkologische Rundschau*, 1914, viii, pp. 275-280.

Patient aged 43. In the abdomen fluid and clotted blood was found. Both Fallopian tubes were unaltered. Fimbrial ends of tubes "free." The left ovary was normal. The right ovary was enlarged. On its median pole was a blue cystic swelling (cf. Chiene's case, p. 172). The right adnexa were removed. Recovery. The microscopic diagnosis was follicular gestation. No embryo was found, but chorionic villi were present in the cavity of the corpus luteum. There were lutein cells in the sac wall in contact with the ovum. No infantilism, no abnormal shortening or tortuosity or lengthening or inflammation of the tube.

SERIES II.

For various reasons the following cases are not included in the above list; in many the evidence that they are cases of primary ovarian gestation is inconclusive, in others no histological proof is forthcoming. Whilst some cases may be genuine, only short reports have been at my command, and from these it is impossible to come to any conclusion one way or the other.

1910.

The Cases of Balleray, Prince, and Rubin.

BALLERAY, G. H. *Medical Record*, July 2, 1914, p. 40.

Patient married ten years; no previous pregnancy. Period two weeks overdue, then irregular discharge. Pain in lower abdomen. Abdominal distension and vomiting. At operation a mass containing a four months fœtus was removed. "Outside the mass was made up of ovarian tissue" (?).

PRINCE, E. M. *Alabama Medical Journal*, Birmingham, 1910, xxii, pp. 751-754.

Patient aged 35. Illness began in June, 1907. Thought to be pregnant. Movements of child very violent on November 7, also uterine hæmorrhage. The same happened in February, 1908, and pieces of "flesh" passed. March, 1908: "Severe chill" and pyrexia for forty-eight hours. Operation, April 30, 1909: A sac containing a dead child weighing 7½ lb. removed. Six "points" *approximately equidistant* on the sac were microscoped; they showed the remains of corpora lutea (corpora albicantia ?). J. S. McLester reported: "I feel justified in concluding from the presence of the above-mentioned structures that each specimen was taken from the ovary. The operator states that the corresponding (right) tube could be demonstrated as separate and distinct from the tumour. This fact does not appear in the drawing of the specimen. Further data needed."

RUBIN, Dr. I. C. "Specimen of Primary Ovarian Pregnancy," *Medical Record*, December 31, 1910, p. 1211.

Specimen shown at New York Academy of Medicine. Patient, aged 24, married three years; one child, aged 2. Last period seven weeks before admission. Two months before admission sharp pain in lower abdomen, especially on right side. On admission: Left of uterus was a tender mass the size of an orange; it was regarded as an ovary without having the usual hardness of this organ. Operation, July 28, 1910: Uterus normal in size. Left

tube bent on itself and attached to enlarged ovarian mass. Latter fixed by soft adhesion to left broad ligament. Left ovary looked œdematous and presented at one portion a dark chocolate-brown irregular protruding surface, thought to be a corpus luteum. On delivery of left adnexa a plum-coloured mass the size of a hazel nut seen; it was apparently free in the pelvis. Tubes neither enlarged nor ruptured. Left ovary still attached to uterus by its proper ligaments; it was, therefore, regarded as the first seat of the pregnancy. Dr. Rubin said there was *positive evidence* of pregnancy in the ovarian stroma, that the tube on the same side was separate and distinct, and showed "no gross microscopic signs of pregnancy." The "mole" had been lost. No evidence of decidual formation in uterus. What the evidence of pregnancy was in the ovarian stroma is not stated, but Tilden Frank, in discussion, thought that the decidual cells were "stroma cells, the result of the hæmorrhagic condition."

1911.

KIVLIN, C. F. *Annals of Surgery*, 1911, liv, July-December, p. 206.

Patient, single, aged 21. Last period, April 12, 1908. Hæmorrhage, June 14 to 28. When admitted there was a mass the size of a large orange in left side of pelvis. Right side free; colostrum in breasts. Operation, June 28, 1908: Left salpingo-oöphorectomy. Right tube and ovary normal. Incision of right large ovary revealed an embryo of six weeks' development. Right tube "apparently normal, not attached to ovarian mass." "Ovarian mass" not adherent to any surrounding structures. Pathological report: Gestation sac contained ovarian tissue. Tube normal, no evidence of impregnation in the tube. No illustration, no means of testing the validity of statements. The author says every man should be given credit for a true diagnosis on his own personal observations!

PRAWOSSUD, TH. G. *Zentralblatt für Gynäkologie*, 1911, xxxv, p. 1230.

A nearly full-term ectopic pregnancy with a living child. Both Fallopian tubes normal. Left ovary wanting. The left tube ran over the gestation-sac. ? Ovarian pregnancy.

KUSMIN, S. J. "Zur Frage über extra-uterine Gravidität," *Zentralblatt für Gynäkologie*, 1912, xxxvi, p. 402.

In seventy-five cases of ectopic gestation there were two tubo-ovarian pregnancies and one ovarian. In the ovarian gestation the corresponding tube was unaltered. Ovarian tissue was not proved, and the patient was seven months pregnant with an intra-uterine gestation.

1913.

BENEKE. *Verhandlungen der Deutschen pathologischen Gesellschaft*, Tagung 16, 1913, p. 355.

Patient, aged 27; one child, aged $1\frac{1}{2}$; one abortion. When menstruation was a week overdue there was sudden pain and fainting, followed a week later by hæmorrhage lasting three days. Operation: Salpingo-oöphorectomy. This purported to be a demonstration of ovarian pregnancy, but was, in fact, an example of embedding of the fertilized ovum in blood-clot situated between the tube and ovary. It is true that the coagulum overlay a corpus luteum of the ovary, and from this it was assumed that the clot came from the rupture of the follicle. There was no evidence, however, to show that the embedding was ever intra-ovarial. This case was also reported by Keil in the *Zentralblatt für Gynäkologie*, 1913, xxxvii, No. 13, p. 465.

ENGELKING, E. "Intra-ligamentär entwickelte Eierstocksschwangerschaft. Ein Beitrag zur anatomischen Diagnose vorgeschrittener Fälle." *Monatsschrift für Geburtshilfe und Gynäkologie*, 1913, xxxvii, pp. 740-756.

Patient, aged 33, 1-para; no abortions. Normal labour nine years after marriage when she was aged 30. Periods always regular. Came to clinic because of abdominal enlargement, noticed for ten months, with irregular hæmorrhage for the past fourteen days. Uterus anteflexed and fixed by a fluctuating tumour which filled the true pelvis and reached to the ribs. Clinical diagnosis: Ovarian tumour or ectopic gestation. The latter was made certain during operation; the sac was on the right side and adherent to the right ureter. In its transverse diameter the sac measured 52 cm.; it contained a macerated fœtus of eight months' development. The sac contained, although partly unrecognizable (wenn auch vielleicht teilweise unerkennbar): "Tube, ovary, lig. latum. and ovarii proprium," all enveloped in rough adhesions (operation took two and a quarter hours). The sac could not be tubal, as on examination the tube was found to be separated from the sac in one place by a lamella 3 cm. wide, thought to be the mesosalpinx; elsewhere it disappeared in the sac wall (geht also hier scheinbar in den Aufbau der Wand ein).

In the illustration the tube is indicated by a schematic dotted line which takes a tortuous course across the sac wall. The tube itself is nowhere apparent as such, and further the author says "das Abdominalende der Tube vollständig in der Fruchtsackwand aufgeht." In microscopic sections the closer they approached the end of the tube the nearer was the tubal epithelium to the placental tissue, the intervening connective tissue between the two being reduced to a tenth of a millimeter. The last remains of fimbriæ lost themselves in the connective tissue of the gestation sac. No ovarian tissue was found in the gestation sac; it contained muscle and was therefore not a peritoneal

implantation. From serial sections of the tube the author excluded a tubal pregnancy, and from a fan-shaped dilatation of the tube as it lay on the outside of the sac he concludes a tubo-ovarian implantation. This fan-shaped dilatation of the tube I have myself shown in a secondary embedding of the gestation sac in omentum. It can be produced by tubal abortion. This case cannot be regarded as a primary ovarian pregnancy. (C. L.)

PAUCOT, H. DEBEYRE A. Étude sur les Grossesses ovariennes jeunes. *Annales de Gynécologie*, 2e série, x, 1913, pp. 129-144.

A critical review based on seven cases in the literature and a description of a case of the author's. Tubal gestation was diagnosed in a single woman, aged 24. At operation the uterus was enlarged. Left cystic ovary and tube removed; no free hæmorrhage. The cyst contained blood. No torsion of appendages. After hardening, the ovary was found to contain a cavity 2 cm. in diameter resembling a corpus luteum of pregnancy, also a cavity containing a whitish membrane composed of "a sort of granulation tissue," and further some caseous looking material which microscopically resembled granulation tissue. The authors regarded this as "pathological vestiges of the amnion and the embryo."

ROUFFART, E. *Annales et Bulletin de la Société royale de Science médicale et naturelle de Bruxelles*, 1913, lxxi, pp. 232-235. "Hémorragie intra-péritonéale par rupture spontanée d'une grossesse de l'ovarie." *Journal belge de Gynécologie et d'Obstétrique*, 1re an, January, 1914, No. 1, pp. 46-49.

Patient underwent Alexander-Adams operation in February, 1913. After this the periods were regular until August. Five days late in September, irregular bleeding followed, and "*crise douloureuse*" on October 7. Two days later another attack of pain and again on November 7, which was six weeks after the last regular period. Ruptured extra-uterine gestation was diagnosed. Subtotal hysterectomy and removal of both appendages was carried out. In the left ovary was a cavity containing clot. No trace of embryonic tissue could be found either in the clot or in the ovarian cavity. The right ovary contained a ripe corpus luteum, the size of a corpus luteum of pregnancy. Rouffart argues that ovarian pregnancy ought not to be excluded in such a case merely because the products of gestation cannot be found.

SEREBRENNIKOWA, O. S. (Petrograd). "Ovarian Pregnancy," *Zentralblatt für Gynäkologie*, xxxvii, No. 43, p. 1592.

Patient, aged 28, three children. Ovum of 5 cm. found in the pouch of Douglas (cf. Oldfield's case). Left appendages normal. Right tube normal. Right ovary enlarged by a blood-cavity into which was a rent 1 cm. in length. This cavity was lined by lutein tissue. Probably a true ovarian pregnancy but no mention of chorionic villi in ovary.

1915.

SEEDORF, M. *Zentralblatt für Gynäkologie*, xxxiv, No. 37, p. 657 (quoted from *Monatsschrift*, xlii, p. 1).

A case of ruptured ovarian gestation; reporter's view was that this is an undoubted intra-ovarial gestation.

The following cases are published in journals which are not at my disposal :—

1912.

ELLIOT, J. B. *Southern Medical Journal*, Nashville, v, p. 354-356.

ROITERS. *Pest. Med.-Chir. Presse*, Budapest, xlviii, p. 208.

STAPLETON, R. P. *Southern Medical Journal*, Nashville, p. 357.

TRAMBLIN and LAMBERT. *Bulletin Société de Médecine du Nord*, pp. 35-41.

MACLEOD, J. K. *Canadian Journal of Medicine and Surgery*, Toronto, pp. 81-91.

Idem. Two cases.

1913.

FRAZER, F. C. *Indian Hospital Gazette*, Calcutta, xlviii, p. 146 (one photograph).

HOPKINS, J. G. *Proceedings of the New York Pathological Society*, n.s., xii, pp. 84-87.

The following record is of interest as showing the type of case which used to pass muster as an example of ovarian gestation.

GEMMIL, JOHN, Surgeon in Irvine. "An Impregnated Ovarium."

A woman, aged about 30, of a strong, robust constitution, was hanged here on Thursday, January 16, 1735, for the murder of her child. I was informed by a sure hand that on January 1 she had her menstrua, and have reason to suspect that she was too intimate with some of her fellow prisoners. In dissecting her body I found both the tubæ Fallopianæ greatly distended; the left one was pale coloured, but the right tube appeared inflamed in its external coat. Having gently squeezed the left tube, a white body inclining to an oval figure, about the size of a large garden pea, dropped out at its extremity, with a good deal of whitish viscid liquor; and, having squeezed the tube several times, a considerable quantity of the same sort of liquor was pressed

out. The membranes of the roundish body were strong and tough, and contained a transparent gelatinous substance; the colour and consistence of the white-coloured liquor were like to the *semen virile*. The left ovarium was more than twice the bulk of the right one, and looked like a bag full of a dark brown-coloured water; but when it was opened a transparent viscid fluid ran out, and what remained was a reddish substance much of the same consistence as the crystalline humour of the eye has, being the greater part of what this ovarium contained. There was nothing uncommon in the right ovarium; about halfway between it and the extremity of the Fallopian tube I found in a duplicate of the membrane such a body as was squeezed out of the left tube, but it was shrivelled and decayed, with little in it of any liquor; the right tube was full of white matter as I squeezed out of the left tube.[1]

[1] "Medical Essays and Observations," published by a Society in Edinburgh. Printed by Hamilton, Balfour, and Neill, 1752, article 23, p. 277.